A History of Physics from Antiquity to the Enlightenment

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This book first published 2022

Cambridge Scholars Publishing

Lady Stephenson Library, Newcastle upon Tyne, NE6 2PA, UK

British Library Cataloguing in Publication Data
A catalogue record for this book is available from the British Library

ISBN (10): 1-5275-8076-8
ISBN (13): 978-1-5275-8076-3

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FOREWORD

Mario Gliozzi (our father) worked on *A History of Physics* right up to his death. To honour his memory, in 2005 we curated its posthumous publication by Bollati Boringhieri. The critical scientific and popular acclaim in Italy prompted us to suggest a version in English, to make the work available to a wider public. We also thought it opportune to complete the text with a final chapter illustrating the complex development of arguments, theories and experimental proofs that have characterised the *physics of fundamental interactions*.

The historian of science Mario Gliozzi was a pupil and friend of the renowned mathematician Giuseppe Peano, who bequeathed him his letters (subsequently donated by his children to the Library of Cuneo), some antique books and his library. As a pupil of Peano, Gliozzi was secretary of the Pro Interlingua Academy, coming into contact with the international historical-scientific world and being elected member of the *Académie internationale d'histoire des sciences.*

In one of his first research projects (presented by Peano to the Turin Academy of Sciences), Mario Gliozzi retraces the definition of the *meter* by Tito Livio Burattini. The 1934 work *"A History of electricity and magnetism from its origins to the invention of the battery"* won the Accademia dei Lincei prize and was the start of the *"History of electrology up to Volta"* issued in 1937. Gliozzi's historical studies comprise articles, treatises and anthologies of scientific writers.

The most challenging and certainly most stimulating work to which Gliozzi dedicated himself is, however, this "*A History of Physics"*.

Before leaving the readers to judge the book for themselves, we would like to add a personal note about the author: Mario Gliozzi, our father. We have many childish memories of our family life, but one that, now we ourselves are old, is set in our minds: somewhere in the shadowed study of our old house in Turin, there was the continuous tack-tack of an *Olivetti Lettera 22,* proof of a dedicated and exemplary life.

The English translation is presented in two books. The present one "*A History of Physics from Antiquity to the Enlightenment*" has been translated by David Climie, M.A. Oxon (English Language and Literature). The second book entitled "*A History of Physics over the Last Two Centuries*"

has been translated by Jacopo Gliozzi, great-grandson of the author and PhD student in Physics at Urbana Champain University (Illinois, USA), who also added some updating notes; we warmly thank both for their invaluable contribution.

We are very grateful to our friends and colleagues, in particular to Prof. Matteo Leone, Prof. Roberto Mantovani and Prof. Clara Silvia Roero for advice and suggestions during the different phases of this work. Sincere thanks must also be extended to Prof. Vanni Taglietti, for his indefatigable and wide-ranging help in the realisation of the project.

Alessandra and Ferdinando Gliozzi

1. CLASSICAL ANTIQUITY

1.1 Pre-Hellenic civilisation

Driven by the demands of a hard life, primitive man, who made his appearance a million years ago, watched what wild animals were doing, tasted and tried vegetables, and looked for the best materials for his tools: stone, bone, wood.

In the Middle Paleolithic Age (200.000-30.000 B.C.), the first graffiti and the first burials appeared; in the Upper Palaeolithic (30.000-8.000 B.C.), the first sculptures and paintings; in the Mesolithic period (8.000-5.000 B.C.), domestication and breeding of animals, and the first attempts at cultivation occurred. Towards the end of the Neolithic Age (5.000-2.500 B.C.), man had become what we recognise now, and the Stone Age was succeeded by the Copper, Bronze and Iron Ages.

Over thousands of years, humankind was driven by need and a curiosity about nature, and gradually learned useful, and magical, techniques based on a wide understanding of zoology, geology, botany, medicine, astronomy, and mathematics. This raw knowledge, technology, and rites gave rise, at the start of the third millennium of the pre-Christian era to the first civilisations in the valleys of the Nile and the Euphrates. Egyptians and Babylonians reached their highest point in mathematics and astronomy. But the construction of the pyramids and the transport of the huge obelisks is also proof of a profound understanding of mechanics and statics.

The invention of baked bricks and the potter's wheel is ancient, dating maybe to before 3.000 B.C. The introduction of wheeled vehicles in the same period gave rise to what historians have called the "first industrial revolution". This was shortly followed by smelting and metal working, the use of oared boats and sailing boats, the introduction of the plough, scales, the lead line, the spirit level, the set square, dividers; in the second millennium, bellows, levers, wedges, winches, pulleys, siphons, and the water hour-glass were introduced.

Techniques that appear with the first organised civilisations and that were already tried and tested at the dawn of Greek civilisation include water supplies, irrigation and draining; transportation by land and sea; cultivation of food products (production of cereals, preparation of flour and bread, fermentation, and so on); the use of pigments and paints, introduced in ancient times more for their religious and esoteric significance than for their decorative effect; making and use of cosmetics and perfumes, often employed for hygienic and esoteric-religious purposes, and subsequently to make one look more attractive.

Some attempts to put this vast store of empirical knowledge into some sort of order can be seen with the Babylonians and the Egyptians: for example, starting from 2500 B.C., the introduction of fixed units of length, weight, capacity; recognition of the passing of the seasons; division of the year into months, days and hours. However, in general, there was still no sufficient evidence for a unitary collection of empirical knowledge in a single doctrine, founded on a philosophical conception of the world.

In the first millennium B.C., the civilisations of the Near East - Egyptian, Babylonian, Assyrian, Phoenician, Jewish - began to decline, while Indian and Chinese civilisations grew and, around the XV century B.C., developed in autonomy, seemingly without any contact with the Mediterranean cultures, whose heritage was adopted by the peoples living around the Aegean.

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1.2 The pre-Socratics

The attempt to describe, co-ordinate, explain and predict natural phenomena, the first nucleus of what over the centuries would become physics (from the Greek Φυσις = nature) started in Greece in the VI century B.C., favoured by an advanced cultural environment and a language already refined by a long literary tradition.

The evolution of Greek science is usually divided into four periods: 1) the Hellenic era, from 600 to 300 B.C.; 2) the Hellenistic era, from 300 B.C. to the beginning of the vulgar period; 3) the Graeco-Roman period; 4) the period of the spread of Greek science to the Muslim and Christian worlds, up to the "renaissance" of philosophy and science in the XIV-XV centuries.

The Ionic school **-** According to Aristotle, the first attempt at a scientific system was made in Mileto, on the Western coast of Asia Minor. Thales (appearing in 585 B.C. - that is when he reached maturity, that Greeks reckoned at 40) and Anaximander (570 B.C.) and Anaximenes (546 B.C.) asked what was the "primogenial matter", the origin of all the changing things that the world present us with.

Thales believed it was water, perhaps because of biological considerations or perhaps because water was the only substance then known that existed in three different states: solid, liquid, gas. Anaximenes believed air to be the principle for all things and Anaximander believed in something indefinable, something "with no limits".

Asking these questions shows the replacement of magical-esoteric interpretations of natural phenomena, typical of primitive man, with a rational mentality that searches for a reason for change that is not supernatural. Reasoned answers to a question, that continue to our times, imply two common concepts, that should be highlighted: reduction of a maximum of phenomena to a minimum of principles; conservation of the matter, that even while assuming diverse forms, remains constant.

This continuous change, this eternal turnover of all things was re-affirmed by Heraclitus (c. 540-80 B.C.), aristocratic, sombre, scornful and contemptuous philosopher who believed fire to be the primal element.

The Pythagorean school - The political situation forced the philosophers to flee from Asia Minor to the Greek colonies in Sicily and southern Italy. One of the political exiles was Pythagoras, who left his native Samos and, after a long voyage, arrived in Crotone, on the Calabrian coast on the Ionian Sea. Here, he founded a school, known as the *italica* or *pitagorica* school. Some believe him to be a legendary figure and even Aristotle always refers to "pythagoreans" and never Pythagoras.

The pythagoreans taught that nature was in eternal motion and return: everything changes and nothing dies. The principle for all things is not this or that element, but the number. The claim would seem to mean that all matter is composed of physical points - the *monadi* – indistinguishable from each other, but whose numbers and configurations are the cause of the different properties of bodies. Thus, qualitative differences are traced back to quantitative differences, as Aristotle showed, although he criticized the theory.

Numbers assumed a fundamental importance in the construction of the world and gave rise to the especial interest in the study of its properties, the search for analogies and the related sense of mysticism.

The mystical quality of numbers led the pythagoreans to the study of musical chords. Philolaos (flourishing c. 400), the first pythagorean of which some fragment is preserved, carefully analysed the octave, to which he gave the term "harmony" (that originally meant "ideal agreement of numbers"). Although there exist various, and often contradictory, testimonies, perhaps the traditional attribution to the pythagoreans is not without foundation that credits them with experiments of sounds emitted by vibrating strings and the discovery that the highness of the sound depends (at the same tension) on the length of the strings. This led to argue that strings homogeneous and of equal tensions create a pleasant sound when their lengths are placed in simple numerical ratios.

The pythogoreans were the first to propose, against common acceptance, that the world and the other celestial bodies were spherical, perhaps justifying the hypothesis only on the basis that the sphere is the most beautiful and perfect solid shape. Philolaos first wrote a non-geocentric astronomical system, according to which Earth, other heavenly bodies and the "anti-Earth" rotate around a "*central fire*" that is invisible to us because it is at the opposite side of the inhabited world. The Sun, lit by the *central fire*, reflects the light received onto the Earth. The movements of the planets at different distances from the centre and at different speeds are compared to the vibrations of the strings, as their relative distances from the centre are in simple ratios, like the lengths of strings that produce pleasant sounds. This is the origin of the "music of the spheres", inaudible to mankind because it has heard it from birth: a poetical and mystical concept that fascinated later scientists right up to Kepler and beyond.

***The Eleatic school* -** The Eleatic school rose up against the belief in unceasing birth and death of all things. It opined it was an illusion and deception of the senses; the Eleatic school, in particular Parmenides of Elea (500 B.C.), initiated the distrust of the senses that did so much to hinder the progress of physics in following centuries.

The logical and mathematical parts of Parmenides were developed by his student Zeno of Elea, working around 440 B.C., whose paradoxes and "aporie" ("puzzlement" - *Achilles, arrow, stadium*) are famous and for a long time were used as evidence that he wanted to only to refute the movement; it is more likely that he intended, arguing against the pythagoreans, to affirm the continuity of time and space and the relativity of motion.

***Empedocles* -** Empedocles of Agrigento (c. 492-432) tried to reconcile Eleatic views with Ionic theories. He opined that the roots of nature are unchangeable, like the "being" of the Eleatics, but love unites them and hate divides them in a never-ending cycle, in the same way as the "becoming" for the Ionians.

Like the Eleatics, Empedocles believed that there is no birth or death; birth is union and death separation of the elements - fire, air, water, earth - the eternal elements, as each has its own nature, without changing from one to the other. They merge to create other substances: in the same way an artist prepares a few colours by mixing a little of that and more of that pigment to depict trees, men, fairs, birds, and fishes. A force, that Empedocles called "love", tends to unite the elements, while another, "hate", separates them. In terms of physics, it must be noted, in the Empedocles' system, in addition to the plurality of the elements, there is the introduction of two forces that we would call attraction and repulsion, at the root of natural phenomena and events.

According to Empedocles, air is a body, as water does not enter an upturned jug because of the compressed air. The Agrigento philosopher also opined that light was a "flowing substance" that, emitted from the luminous source, progressively reached the intervening bodies; in sum, light is corpuscular and moves at a finite speed. It may cross certain bodies because it passes through their pores contained in the granular constitution of matter. The pores, invisible because they are so small, are not totally empty, because a vacuum does not exist.

Anaxagoras - Anaxagoras (c. 500-428 B.C.), a younger contemporary of Empedocles, also tried to prove the non-existence of a vacuum, identified with the pythogoreans' air, using Empodecles' jar and other air-filled vessels resistant to pressure. Contrary to Empedocles, Anassagora believed that matter was continuous and that the transmutation of substances was impossible: "How can a hair be generated from what is not a hair, or flesh from not is flesh?" he noted. He believed in the pre-existence of all things and their existence in different proportions in every spatial region. If a given substance appears in a given point, it only signifies that it is there in greater quantity. Similarly, when we eat bread and water these do not change into meat and blood, rather from bread and water separate the invisible particles of blood and meat that they contained and, reunited in large numbers, become visible to us as meat and blood.

The atomists - In response to the Eleatics, Empedocles and Anaxogoras proposed different qualities of the matter. The atomistic school proposed a new solution: matter possesses no qualities, it is homogeneous, impenetrable, indestructible and discontinuous, therefore made up of parts that cannot be divided, thus called *atoms* (indivisible). Atoms are not the pythagorean equal "monadi" dispersed in the "pneuma" (air), but have different shapes and sizes, separated by an absolute vacuum.

Atomistic doctrine is linked to two names: Leucippus, a half-legendary figure whose actual existence is in doubt, and his pupil, Democritus, born in Abdera (Thrace) around 460 B.C. and died c. 370 B.C., of whom about three hundred fragments have survived.

According to Democritus, atoms are indivisible because of their hardness, not smallness, as there exist atoms as large as a world. They are made up of equal substance and differ by form and dimension and, in groupings, by order. Atoms are in perpetual motion in all directions and when they meet their innumerable combinations create all the bodies of infinite space (the infinity of space is explicitly postulated by Democritus). The sensitive qualities of our experience (heat, cold, sweet, sour, colour, sound, etc.) are subjective, depending on the individual experiencing them. They depend on the form of the prevalent atoms in each body, but the sensations produced also depend on the sentient subject so that the same atomic figure may have contrary effects and contrary figures may produce the same effect.

Weight and hardness, on the other hand, are real, therefore objective, qualities of the bodies. Democritus explained the varying macroscopic weight of bodies through they different mix of atoms and vacuum. However, one fragment that is not easy to interpret would seem to include in the explanation the different weight of the constituent atoms. The different hardness was explained by the different distribution of the atoms: in lead, for example, the atoms are distributed regularly and therefore lead is softer than iron where the atoms are distributed irregularly. It may be too much to claim, but simply an interpretation for our times, that for Democritus hardness was a property linked to the reticular structure of the material being studied.

***Plato* -** Plato, born in Athens (or Egina) 427, died 347, is one of the world's greatest philosophers and writers. His place in the history of philosophy and literature is of great importance, but in the history of physics, he will be remembered as a retarding force of the development of this science, despite his undoubted merits in mathematics.

According to Plato, truth is to be found in the world of pure forms, in the reign of "ideas", eternal and immutable models that shape our world of shadows. In the *Timaeus* dialogue, he tries to give a true description of the creation of the world by a creator or divine "demiurge" that, looking at ideas, first creates the soul of the world and then gives it physical form: everything is given a determined mathematical shape (air is a regular octahedron, water is a regular icosahedron, and so on). Plato also believed in the four elements, but mixed this belief with a cloudy pythagorean mysticism whose physical meaning is difficult to understand. He tried to explain the origins of the world and natural phenomena regardless of the observation and experimentation, but applying a moral teleology, already adopted by the pre-Socratic philosophers, based on personal concepts of beautiful and good: the world was created for a specific purpose by an ordered mind.

More than for the ideas it expresses, the Timaeus is important as historical source of the scientific theories of the time, that we would not know otherwise.

1.3 Aristotle

A history of philosophy would demand a much wider description than our previous treatment of the philosophical schools and philosophers before Plato.

But, with rare exceptions such as the pythagorean research on vibrating strings, the tradition and documents passed down to us (evidence from pre-Socratic philosophers is fragmented and mostly derives from a few quotations from later writers) gives us no proof that Greek philosophers before Plato made physical analyses, that is the study of single phenomena and individual natural objects. On the contrary the pre-Socratics, with the fearlessness and freshness of youth, immediately launched themselves on the search for the material principle of all things, posing questions of cosmic physics that, due to ignorance of particular natural laws, necessarily assumed a metaphysical nature. Their doctrines, by consequence, concern the history of philosophy rather than the history of physics.

This historical view does not mean that we should ignore the importance of Greek philosophy in the first two centuries for the history of physics. The speculations of the philosophers of the Ionic school on the primitive element, Empodocles' poetic forces, the atomism of Leucippus and Democritus, Plato's animism will become the guidance and inspiration also for physicists when over time, as we will see in this brief history, there will be a search for a broader and not gratuitous understanding of particular phenomena.

But, precisely because of this function, the works of the early philosophical schools emerge as a cultural base, contributing to making sense and giving a purpose to scientific research, opening the way to scientific discourse, creating that framework of forms of expression, causal links, accepted and widespread mental attitudes at the heart of "common sense" and "good sense", that are neither "common" nor "good" absolutely, but are related to the cultural level of the people of a certain epoch.

If we had documentation, perhaps we would discover in the first two centuries of Greek philosophy traces of observations and experiments on particular phenomena and bodies. This supposition can be supported by the first major scientific system in history: Aristotle's nature books, included in a vast encyclopedia of knowledge that cannot be the work of a single man but the result of collaboration between many people or many generations.

Aristotle organised the exhaustive material under his genius, boiling it down to units, systems, that would be the framework for science for the next two thousand years.

Aristotle, born in Stagira, Thrace, in 384 B.C., was a pupil of Plato up to the latter's death; he then left Athens and travelled the Greek world; from 343 to 340 he was at the court of Phillip of Macedon, tutor of his son, the future Alexander the Great. In 335, he returned to Athens and found a School, the Lyceum, taking its name from the sacred gardens of Apollo Lyceum where it was built. He oversee the Lyceum for 12 years when, on the death of Alexander the Great, the anti-Macedonian reaction forced him into exile in the Chalcis, where he died in 322, aged 63.

Aristotle's dialogues have been completely lost but almost all his expositive essays have survived. The ones of particular interest for physics include the treatises: *Physica* (in 8 books), *De coelo* (4 books), *De generatione et corruptione* (2 books), *Meteorologia* (4 books), to which should be added *Problemata* and *Mechanica*, that is questions of mechanics, collections in the form of questions and answers, both almost certainly apocryphal.

Aristotle's naturalistic works order all contemporary physical knowledge, referring to and, if necessary, confuting earlier beliefs. Reacting against Pythagorean and Platonic mysticism, Aristotle attempted to base physics on observation and experimentation. Like Plato, Aristotle believed that sensible understanding of the particular is contingent, connected to time and space, while scientific knowledge is absolute, beyond the bounds of time and space. But our universal concepts do not come from reminiscence, as Plato taught, but through deduction from the particular to the general, starting with the experience of the senses: consequently, observing assumed greater importance in building up science, while mathematics became less important and was little used by Aristotle. But this naturalistic approach was subject to a more general teleological axiom that limited its fruitfullness: every event has a defined purpose and the whole universe is the result of a predetermined plan. The teleologic concept of the world, exasperated by Aristotle up to his acceptance of an "intelligent nature", was a cornerstone of his thinking and was a keystone in later interpretations of Aristotelian philosophy. Notwithstanding the criticism of Theophrastus (372-288 B.C.), Aristotle's most distinguished pupil, the teleologic axiom, that became foreign to our physical mentality, even though it gave a good outline for biological research, remained firm up to the beginning of the modern era and, on some occasions, peeps out today.

Aristotle maintained the four elements but rejected the Platonic correspondence to polyhedrons; they are not the elements of Empodocles, even though they have the same names: earth, water, air, fire. Aristotelian elements are made of a single primary matter that takes on different forms according to its various qualities: heat, cold, dryness, wet, that are present always in four pairs of cold-dry, cold-wet, hot-wet, hot-dry. When the primary matter is cold-dry it is earth, and water when cold-wet, air when hot-wet and fire when hot-dry. The elements may transmute circularly, that is according to the above succession that imposes cold-dry follows hot-dry.

Earth occupies its proper "place" at the centre of the world, coinciding with the centre of the terrestrial globe; water surrounds the earth; then there is air, finally fire. All four make up the sub-lunar world.

Above fire is the sky, made up of the fifth element -*ether*- suggested by Philolaus, perhaps due to the discovery of the fifth regular polyhedron (the dodecahedron). Ether is the perfect element, pure, everlasting, unchangeable and incapable of being recreated.

The world is unique, limited in space but unlimited in time, complete in itself and split into two areas obeying different laws: the sub-lunar world in which all things are born, decay ad die, the world of the stars unalterable and incorruptible. This distinction, surpassed by earlier philosophers is not merely "a priori" thinking nor a return to Pythagorean theory but rather the result of common observation of earthly transformations, especially meteorological phenomena, while not noting any change in the sky, although astronomical observations had been uninterrupted for centuries.

Of particular interest to us is the Aristotelian science of motion that, after having dominated physics for many centuries, was challenged from the Renaissance onwards. Aristotle's theory of motion is wider than that, after Galileo, we are used to. Aristotle interpreted motion as a quantitative or qualitative variation causing an event: this broad description meant he could claim that in nature everything is in movement. He called the limited change in the position of one body in relation to others over time *local motion* and within the "local motions" there were *natural motions* and *unnatural* or *violent* movements, thereby breaking up the continuity and homogeneity of the phenomena, whatever their natural or accidental cause. In short, the Aristotelian universe has two fractures: it is split spacially and it is split phenomenologically.

Aristotle classified motion as circular and rectilinear. The first is the most perfect while the second has two forms: away from the centre and towards the centre (light bodies rise, heavy bodies fall). Ether is a perfect body and therefore has a perfect circular motion, and because the heaven is made up of ether, it has a circular motion. The regularity and eternity of the motion of the stars, that Aristotle calls the *primo motore immobile*, needs to have a cause that impresses motion to all the spheres in which the stars are set. If the concept of p*rimo motore immobile* is certainly metaphysical, even theological, placing the Earth at the centre of the Universe corresponds to the need to prove the everyday experience of seeing the stars rotating around the Earth.

Rough observation also corresponds to the laws of natural motion of bodies in the sub-lunar world. Common observation gives us elements that fall and elements that rise (for example, smoke and fire): therefore, heavy bodies naturally return to their place of origin, the centre of the Earth, while lighter elements move upwards, that is towards the limits of the sub-lunar world. In any case each body, be it heavy or light, moves towards its natural place: "heavy" and "light" therefore become absolute concepts. In this way Aristotelian physics is an obstacle to the notion of specific weight, that emerged much later and only with Archimedes (§ 1.6).

Aristotle judged the non-vertical motion of projectiles to be violent and divided the trajectory into three parts: the first rectilinear and oblique, the third rectilinear and vertical, the second circular and raccording the two. This theory would last until Nicolò Tartaglia's *Quesiti et inventioni diverse* (1546).

But how, once thrown, does the object keep moving? The cause cannot be the object itself, nor the person who threw it and who has no further effect on it: it must be in the middle. Aristotle had a bizarre theory that the thrown object continued to be driven, like a sail by the wind, by the air occupying the vacuum left behind the object thrown as it moves.

This theory of dynamics is very different to ours. In Aristotelian dynamics, the body in motion is always the result of the force applied at that moment and is inversely proportional to the resistance of the medium. Consequently, in a vacuum, as there is no resistance, velocity would be infinite, meaning the body would be ubiquitous. The deduction goes so against common sense that Aristotle concluded that a vacuum could not exist in nature: a conclusion that was the exact opposite of what the atomists had arrived at, who believed that motion would be impossible in the full. Aristotle debated for a long time with the atomists about this and supported his theory with other topics: it cannot be explained why in the vacuum a body in motion would stop in one place or another, because in the space, as a vacuum, there is no difference, but one could say that in the space (vacuum) everything should be at rest as there would be no reason why a body should move in one direction or another, or at different speeds. To conclude, Aristotle's basic argument against the vacuum is that it cannot contain any spatial arrangement: no high, no low, no right, no left. Emptiness would be inactive and impassible; therefore, it does not exist in our limited world. This is clearly more an abuse rather a use of the principle of sufficient proof.

Starting from these considerations, Aristotle (*Physica, IV, 6-9*) -who defined place as the limit of the contained body and space as the place without a contained body but that could contain one– concluded that emptiness is a contradiction of logic, because it would create a *locus sine locato corpore* (place without a contained body): a senseless abstraction, as modern relativists would say in another way when they criticise absolute space to which the movements should refer.

Horror vacui will be a cornerstone of Aristotelian physics and the polemics between supporters of "vacuum" and supporters of "fullness" continued up to the scientific renaissance (and maybe beyond, with the arguments over ether). But, to hear a new opinion on the physical question, we must move on to Torricelli's experiments in 1644 (§ 5.2).

Some historians have claimed to find in the Aristotelian anti-vacuum argument the principle of inertia. However, the chapter of *Physica* (IV, 8 215-19) in which the principle is to be found is used as a proof of the absurdity that would be reached with admitting emptiness (vacuum), Aristotelians in later centuries interpreted the chapter in this way. The chapter therefore confirms that the principle of inertia was completely unknown to classical science, that thought it absurd.

Another immediate consequence of Aristotelian dynamics is that the velocity at which a certain body falls is proportional to its weight (this seemed to be proved by the common observation that an apple falls faster than a leaf). On the other hand, careful and prolonged experiments arrived at the acute observation of a constant increase in the velocity of falling bodies, that Aristotle attributed to a gradual increase of the weight of the bodies that go getting closer to their place of origin. Another great merit of Aristotelian kinetics is the exact description of the rules of the composition of displacements, albeit for the particular case of perpendicular displacements.

Although it is very different from ours, Aristotle's interpretation of dynamics should be recognised as a great merit. He was the first scientist to advance a coherent semi-quantitative theory, in a field that was so difficult that it took another fifteen centuries before a new science took its first tentative steps.

The study of statics is closer to modern research: it expounds the theory of equilibrium in a lever, with an anticipation of the later principle of virtual workings, and a description of weight scales and pulleys.

The works of Aristotle, and especially the *Problems*, contain numerous interesting comments on music, meteorology, physics, and applied mechanics; references to kinetic energy, observations on osmosis, correct ideas on sound propagation in air, explanations of echo as reflection, a similar (but erroneous) explanation of rainbows, comments on the propagation of light, and so on. It is a highly commendable collection of observations that confirm how Aristotelian physics was based on observation, albeit ingenuous, and partly on experimentation, even if primitive.

What is missing in the Aristotelian physics, apart from the incapacity to separate single phenomena from their natural processes, is analysis, a critical eye and a wariness of generalisation.

We may claim that modern science experiments with a critical approach while Aristotelian science experimented ingenuously. In plain terms, Aristotelian mechanics did not distinguish passive resistances and he did not understand that the study of certain phenomena sometimes requires some "trick" that goes beyond mere observation. Naturally, this does not explain the Aristotelian failure in the study of physics, but is a comment on the insufficiency of his research methods. On the other hand, an explanation of why Aristotle and his school did not know how to make abstract, in the above sense, is an old and still unsolved question.

1.4 Criticism from the disciples

Aristotle's theories of physics were not immediately or generally accepted. It was only after 500 years that Alessandro d'Afrodisia, c. 200 A.C., expressed appreciation for his physical theories. Theophrastus (372-287 B.C.), Aristotle's successor as head of the Lyceum from 322 up to his death, gave a decidedly scientific character to the School and pursued the indications if his teacher in botanical studies. However, in physics he raised the first objections to finalism, the radically different nature of the motion of heavenly bodies to terrestrial movement, and the theory of the elements, from which he excluded fire.

Theophrastus's cautious critique was deepened by Strato of Lampsacus (d. 240 B.C.), called *the physicist,* who succeeded him as head of the School until 269. Strato preferred the study of particular physical phenomena over the grand summaries of cosmic physics of his predecessors, at least judging by the pneumatic experiments attributed to him by Hero. Unfortunately, only a fragment of his vast scientific work has come down to us, aside from the reports of later writers.

In the treatise (lost), *De vacuo*, Strato, while refuting the infinite vacuum of Democritus, admits, unlike Aristotle, the existence of small empty spaces inside matter, the *vacuum intermixtum,* or disseminated vacuum as Hero would call it. He did not accept the atomic theory, and also criticised Aristotle's theory of the elements, particularly the concept of the *natural* *places* of the elements and the consequent idea of absolute lightness and weight: every body, including fire, has weight, and the rising of light bodies is not due to a natural trend but the driving force of air. In a more radical criticism than that of Theophrastus, Strato opposed Aristotelian finalism, claiming that physical phenomena are the result of mechanical causes, not finalistic causes.

THE HELLENISTIC AGE

1.5 The Museum of Alexandria

The century before the death of Strato witnessed enormous political changes that had a profound effect on Hellenic culture. With victory of Phillip of Macedonia at the battle of Chaeronea (338 B.C.), the cities of Greece lost their freedom. Shortly after, Alexander the Great conquered the Persian empire in a flash, and founded military agricultural colonies, some of which soon became important commercial centres. The most illustrious city founded was Alexandria.

On the death of Alexander (323 B.C.), and the flight of Aristotle, Athens was no longer politically important and was also losing its intellectual supremacy. The schools of philosophy remained, but with the separation from sciences, were impoverished and increasingly concentrated exclusively on moral questions.

The scientific movement, promoted by the general use of Greek and the generous patronage of princes of regions resulting from the breakup of the Alexandrian empire was, at this point, so far advanced that science could no longer belong to the general public, but only limited to specialists. In the shadow of the thrones, scientists, honoured and generously rewarded, produced the best of antique science.

Some cities, like Syracuse and Cos, that already had a cultural tradition in the Hellenic age, gave a new impetus to scientific studies. Alongside these, scientific centres were added: Pella in Macedonia, Antioch in Syria, Pergamum in Asia Minor, and, later, Rhodes, Smyrna, Ephesus and so on: all took as their model the important institutions established in Alexandria that remained throughout antiquity the scientific capital of the Graeco-Roman world.

Ptolemy I Soter, the founder of the Ptolemaic dynasty in Alexandria, summoned to his court Demetrius Phalereus, who had been a pupil of Aristotle, and later, as a tutor of his son, Strato of Lampsacus. Demetrius was ordered to construct a school along the lines of the Lyceum and subsequently laid the foundations for the two cultural institutions in

Alexandria: the Museum, named in honour of the Muses, and the connected library, the core of which seems to be a collection of the works of Aristotle. With Ptolemy II (Philadelphus), who succeeded in 285 B.C., the Museum became an important cultural centre where intellectuals could live together paid by the state, and with access to two huge libraries, that in 48 B.C. held seven hundred thousand texts. This was the first example of a collective organisation of scientific research and we have to wait until the 20th century to see its imitation. Very soon, books began to be published by the Museum, thanks to the Egyptian *papyrus*, that gave Egypt a natural monopoly of the production of paper.

These extraordinarily favourable conditions for academics attracted numerous scientists to Alexandria from all over the world, giving rise to scientific schools that would continue throughout antiquity. More specifically, all the physics of the Hellenistic age, that constitutes classical antiquity's greatest and best contribution to the study of nature, is linked to the Museum of Alexandria.

1.6 Archimedes

Archimedes is also linked to the fortunes of the Museum; his work clearly demonstrates the contrast between the great philosophical syntheses of the Athens School and the systematic scientific research of particular natural phenomena instituted by the Schools of Alexandria.

Born in Syracuse, perhaps in 287 B.C., Archimedes studied for a long period in Alexandria under Phidias, the famous astronomer, and for the rest of his life kept up relations with the scientists of the Museum. In Egypt, perhaps on a second visit, when he was already famous, it seems he built bridges and dams to check the flooding of the Nile. But his most ingenious invention in this period was the *cochlea*, now known as *Archimedes' Screw* (Fig. 1.1) that, in the opinion of Galileo, an expert and severe judge, "is not only marvellous, but miraculous, because the water rises in the screws by falling continuously"[1]. The invention, the result of Archimedes' knowledge of geometry and constructed thanks to his exceptional ability in mechanics, was used by the Egyptians both to bring water to high lands (maybe up to 4 metres) that were not naturally affected by the floods and to drain low-lying areas.

[1] Galileo Galilei, *Mechanics*, in Id. *Works*, Ed. Nazionale, Barbera, Florence 1890-1909, Vol. 2, p. 186.

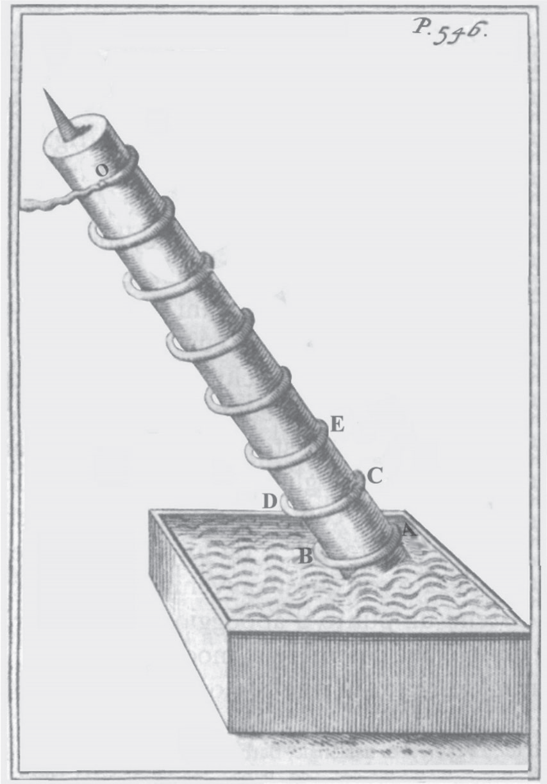

Fig. 1.1 - Archimedes Cochlea in an early 18th-century *Dictionnaire de mathematique*

But there are at least another forty mechanical inventions credited to Archimedes, and although historical sources are part of legend, historians have no doubt about some of them, such as the worm / gear wheel and differential hoist that he used to launch a large ship. This event is linked to what he is supposed to have said: "Give me a fulcrum and I will lift up the world". Undoubtedly, he created that precision mechanics model: the planetarium, described in a lost work; Marcellus brought it to Rome as a trophy of war and Cicero later admired it. Last, there is no doubt about the legend of his defence of Syracuse during the three-year siege of the roman army, commanded by Marcellus, employing a number of weapons that terrified the besiegers. In the end, Syracuse was conquered (212 B.C.) and, against Marcellus's orders a stupid Roman legionary murdered Archimedes while he was drawing on the sand: even if the story is not true, it is fitting.

Archimedes was the founder of statics and hydrostatics. Even though his writings are in the form of geometric expositions, based on postulates derived from experiments that he does not describe, it is certain that he carried out precise experiments. Archimedes himself wrote about one which he did to measure the angle of the apparent diameter of the Sun: "After having paced a long ruler on a vertical support, and placed where sunrise could be observed, a small lathed cylinder is positioned vertically on the ruler. When the Sun appears on the horizon and becomes visible to the eye, the ruler is turned towards the Sun and its ends observed; in the meantime, the cylinder, positioned between the Sun and the eye, completely obscures the Sun. Then, the cylinder is gradually moved away from the eye until the Sun begins to appear in each part of the cylinder, and is then fixed"[2]. Modern physicists cannot describe the experiments more accurately.

The first scientific work of Archimedes seems to have been on centres of gravity, in which he deals with the principles of lever and centres of gravity or barycentres. The condition of equilibrium of a lever can already be found in the mechanical theories attributed to Aristotle, as already mentioned, but these were unclear and mixed with ideas on dynamics. Now, Archimedes arrived at his conclusions from experiments on real levers, as it is obvious that his postulates on the equilibrium of levers are the result of experimentation. The first, and fundamental, theory is: "Let us suppose that equal weights positioned equidistantly remained balanced. Equal weights placed at different distances are not balanced, but (the system) goes down towards the weight furthest away"[3]. In Proposition VI, he deduces: "Commensurable weights will be in equilibrium if the distances at which they are suspended are in inverse proportion to the weights"[4] and in the next Proposition he extends that property to include non-commensurable weights.

This text contains a fundamental concept of mechanics: the centre of gravity. Propositions 4-7 describe it without defining it. We may suppose, therefore, that the concept was introduced by some unknown predecessor of Archimedes and by he himself in some now lost work. But in either case, Archimedes must be recognised as the founder of the rational theory of barycenters.

This concept is also linked to the discovery of another fundamental concept of mechanics: the moment of a force with respect to a straight line

[2] Archimedes, *Arenarius*, I, 12, in *Archimedes opera omnia*, Greek text and facing Latin translation**,** edited by J.L. Heiberg, Teubner, Lipsia, 1881, Vol. 2, p. 251.
[3] Archimedes, *De planorum equilibriis, svie de centro gravitatis planorum,* I,1, in *Archimedis opera omnia*, op cit, Vol. 2, p. 143.
[4] *Ibid*, p. 153.

or a plane, because he knew, as shown in *Il metodo*, discovered by Johan Heiberg only in 1906, that "two weights suspended from the arms of a lever will be in equilibrium when the result of the products of their surfaces or volumes by the distance from their centres of gravity from the fulcrum is equal"[5]**.** The modern history of mathematics underlies the importance that the understanding of the centres of gravity had for Archimedes mathematical discoveries.

His discovery of the principle of hydrostatics is more commonly known, and still bears his name. This is the legend recounted by various historians, among which the most recognized if Vitruvius, in which Hieron, the tyrant of Syracuse and maybe related to Archimedes, ordered him to find out whether a crown was completely made of gold or mixed with silver. The problem occupied Archimedes for some time until when one day he was taking a bath in a tub and noticed that the more he soaked down, the more water overflowed. He understood that this phenomenon gave him the key to solving the problem and, exultant, he got out of the bath and ran through the city shouting "*eureka, eureka*" (I've found it!).

According to Vitruvius, Archimedes used the following method to reveal the fraud: putting a quantity of gold equal to the weight of the crown in water, he collected and then weighed the amount of overflowing water; he repeated the experiment with an equal amount of silver and found that the overflow of water was greater (because, at equal weights, silver has a greater volume than gold); finally, he repeated the experiment with the crown and obtained a result that was between the previous two; he therefore concluded that the crown was not pure gold.

In an early treatise, Galileo acutely judged the Vitruvian version to be "coarse and far from the refinement; and this judgment will become more and more clear to those who have read and understood the very acute inventions of such a brilliant man. From which, unfortunately, we understand how much all other minds are inferior to that of Archimedes and how little hope remains to find an ingenuity similar to his (….). But knowing that this (the Vitruvian) description is completely false and lacking the exactness that mathematics requires, has frequently led me to think about how the mixture of two metals could be revealed by means of water, and last, after having carefully reviewed what Archimedes demonstrates in his writings about things in water, and about those of equal weight, I found a way, that I believe is the same as Archimedes's, to resolve our problem

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[5] E. Ruffini, *"Il metodo di Archimede e le origini del calcolo infinitesimale nell'antichità"* Stock, Rome 1926, p. 85.

precisely; I will use the same method as Archimedes, which depends, in addition to being exact, on Archimedes' demonstrations"[6].

In Galileo's historic reconstruction, Archimedes would have determined the weight lost in water by pure gold, pure silver and the crown respectively, and therefore, as elementary physics shows, determined the composition of the crown.

However, whatever procedure Archimedes used, we may be sure that he experimentally discovered the principle of hydrostatics, although in his works the treatment is conducted *more geometrico* without any mention of the experiment. Archimedes states only two basic hypotheses: in every fluid, the less compressed part gives way to the more compressed; the upward thrust of a solid immersed in a liquid is vertical and passes through its centre of gravity. Therefore, the surface of liquids at rest is part of a spherical surface whose centre is the centre of the Earth, therefore sea level is the same everywhere.

The third proposition of the treatise exposes a new concept, ignored by his predecessors, perhaps due to the influence of Aristotelian physics, as mentioned in (§1.3): the concept of relative specific weight. It is introduced in this way: "A solid body, with an equal weight and volume to a liquid, immersed in the liquid, will submerge so that no part of it shall emerge from the liquid, or sink further"[7].

The fourth and fifth propositions illustrate the cases of solid bodies specifically lighter or heavier than the liquids in which they are placed. The seventh proposition gives us the famous principle; "Bodies specifically heavier than a liquid, placed in that liquid, will sink to the bottom, and in the liquid will become as lighter as the weight of the liquid volume equal to the solid volume"[8]. The second book deals with conditions of floating, and especially the balance of a straight segment of a paraboloid of revolution, using the classic method still used in modern mechanics.

We do not have the books on catoptrics that were certainly written by Archimedes, but the story of the burning mirrors he employed to destroy the Roman fleet during the siege of Syracuse is certainly a legend.

In conclusion, if legend colours the figure of Archimedes particularly about his practical inventions, out any myth he has passed down to us his fundamental contribution to physics: the introduction of the concepts of the centre of gravity; static moment; specific weight; the laws of levers; the basic principle of hydrostatics. These are the pillars of two new branches of

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[6] G. Galilei, *La bilancetta*, in Id. Opere op. cit., Vol. I (1890), pp. 215-16.
[7] Archimedes, *De iis quae in humido sebuntur*, I, 3, in *Archimedes opera omnia*, op. cit. Vol. I (1860), p. 362.
[8] *Ibid.*, p. 369.

science: statics and hydrostatics. The traditional negative judgement of Greek physics should therefore be mitigated.

1.7 Alexandrian mechanics

A contemporary, or perhaps predating Archimedes, was Ctesibius, founder of the renowned school of mechanics in Alexandria. A legend grew up around him, although a dubious fragment has been passed down.

A distinctive characteristic of Alexandrian mechanics is the study and utilisation of air compression (pneumatics); it is very probable that the founder of this branch of technology, of great interest to physics, is Ctesibius. The unproven fragment attributed to him describes a hydraulic body, formed, according to the current organ, of pipes of various lengths, vibrating due to a blow of air compressed by water.

Tradition has it that Ctesbius made many other contributions to practical mechanics, including a clock, two types of heavy "cannon" using compressed air, the water pump, which he modified to be a fire pump and up to the Renaissance was known as the *ctessibia machina*.

1.8 Philo

Although the works of Ctesibius are lost, we can have an idea of their magnitude through the wide treatment of mechanics of his successor and pupil, Philo of Byzantium, who lived in Alexandria; Philo's mechanics, written around 250 B.C., has passed down in large part, albeit some through an Arabic remake.

After a general introduction, Philo begins his work with a description of war machines so accurately that in the early decades of the 20th century they were rebuilt and they were admired for their finesse. After a long discussion of the theory of levers, Philo moved on from the art of war to a description of automatons and an automatic theatre. The book of pneumatics contains a variety of fascinating toys to entertain party guests: curved mirrors, vases spouting different liquids, fountains with drinking animals and singing birds, and a suspension now called "cardanic", an automatic machine to provide lustral water at the entrance to the temple. In many of these machines there was the intelligent use of atmospheric pressure and the pressure of water vapour (steam); in addition, Philo demonstrates complete familiarity with the laws of a siphon.

There are also numerous descriptions of experiments in physics, ably carried out, even if the interpretations are generally different to ours. In the book on pneumatics, much admired by later scientists, and almost certainly inspired by Strato of Lampsascus' treatise on the vacuum, we can cite the following. To prove that air is a body, Philo takes "a vase, that is said to be empty, and wider in the middle and narrow at the top, like the amphorae made in Egypt", a small hole is made in the bottom, sealed with wax and the vase is immersed upside down in water, then the wax plug is removed. "So, the air escaping from the hole can be observed; if then the water level exceeds the hole, air bubbles can be seen in the water, while the vase will be filled with water as the air escapes through the hole (...) and this shows that air is a body"[9].

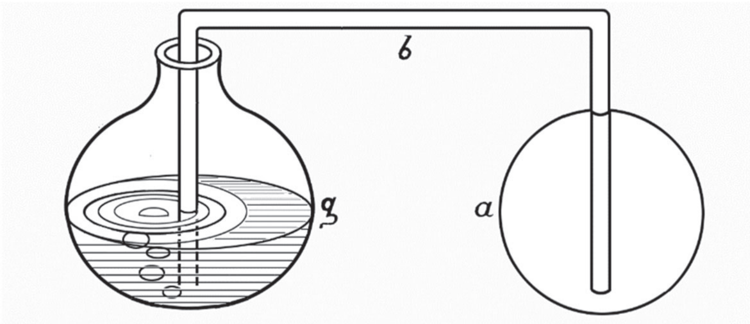

Fig. 1.2 - Philo thermoscope. The lead ball *a* is empty, *g* contains water. Placing *a* towards the Sun, or warming it, the air inside expands and passes though tube *b* and bubbles into *g*. If *a* is made colder, the volume of air decreases and the water in *g* rises in b and pours into *a. Source: Heronis Alexandrini, op. cit.*

Afterwards, Philo describes the first recorded thermoscope (Fig 1.2) with two spheres, one empty and the other completely filled with water, connected by a tube. By exposing the empty bulb to the Sun, air bubbles can be observed in the other, because, Philo claims, if the ball is warmed "some of the air in the tube escapes". If the ball is then put back in the shade, the water will travel back up the tube to the other sphere. "If I then heat the sphere with fire"- Philo concludes, "the same thing happens, and the same if I pour hot water over the sphere. On the contrary, if I cool it, the water will escape" (the water in the first ball will enter the second).[10] In this way Philo, and probably his teacher Ctesibius, understood, with an experiment

[9] *Liber Philonis de ingeniis spiritualibus,* 2 in *Heronis Alexandrini opera quae supersunt omnia*, original text and German translation edited by H. Nix and W. Schmidt, Teubner, Leipzig, 1899, Vol. I, pag. 462.

that is still basically used in early physics schools, the thermal dilatation of air, that they then employed to build their toys.

The particular experimental knowledge of pneumatics led the Alexandrians to enter into the argument between vacuumists and supporters of fullness, to adopt the thinking of Strato of Lampsacus (1.4): there cannot exist a vacuum in a large mass, but only in a disseminated vacuum, that is, the vacuum between a particle and the particle of a body. This type of vacuum explains the different densities of bodies, and the compressibility[10] and elasticity of air: when the volume of air is reduced, the particles are closer, but are therefore in an unnatural position, and then they tend to return to their original position, giving rise to the force of compressed air. Fire acts in a similar way and penetrates between the particles.

1.9 Hero

The reputation of Ctesibius and Philo was overshadowed by Hero, perhaps also due to the fact that his abundant writings have come down to us almost intact. We know for sure that Hero taught in Alexandria, but we have no detail of when. Authors that he cites, and those that quote him, would place him between 150 B.C. and 250 A.D. Hero estimated the distance between Rome and Alexandria using two observations of the same lunar eclipse. If this eclipse is the one that occurred in 62 A.D., as recent studies suggest, then Hero lived in the 1st century of the modern era.

One of his most famous works, at least among Renaissance scientists, was the two volumes on pneumatics in which the understanding of the compressibility of air is applied to a number of devices, most of which had already been described by Philo, as Hero admits while claiming to have improved them and invented new instruments. These include his famous *eolipila*[11], the first working steam-driven machine, a distant ancestor of modern turbine engines. As noted in many modern physics texts, this is a hollow, horizontally pivoted sphere (Fig. 1.3), in which at the two ends of a diameter are connected two right-angle bended tubes; the steam from the burner enters the sphere and exits through two tubes arranged perpendicularly to the horizontal axis, with their mouths facing in the opposite direction; by reaction, the sphere rotates. Hero describes the eolipile only for the effect of amazement, like all of his toys: he was only trying to demonstrate how it is possible, placing a pot on the fire, to spin a ball.

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[10] *Ibid.*, 7 pp. 474-76.

[11] Chapter 4 in: M. Gliozzi, *A History of Physics over the Last two Centuries.* Cambridge Scholars Publishing, in press 2022.

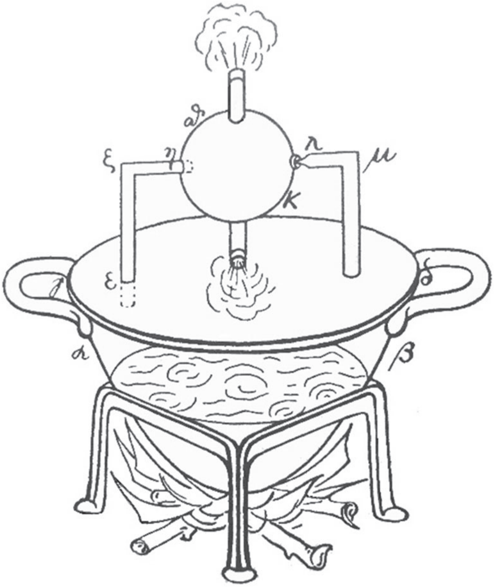

Fig. 1.3 - Hero's eolipila: αβ the burner; εξη the empty tube conducting the steam to the sphere δ, pivoted on the axis ημ. The steam exits the two right-angled tubes in opposite senses and the sphere begins to spin on its axis. Source: *Heronis Alexandrini*, op. cit.

Another famous trick is the opening of the doors of the temple by simply lighting a flame (Fig. 1.4).

A large part of Hero's pneumatics is dedicated to a description of spectacular games and it seems that his aim was to entertain and amaze his audience. The *Meccanica,* of which we have a complete version only in Arabic, is more scientific. Hero gives a long description of simple machines (winches, levers, pulleys, wedges, screws), gears and other more complex machinery; his "Mechanics" is like an encyclopedia of ancient engineering and is written in a clearly educational manner for the practical use of engineers and artisans.

In addition to the *Catottrica*, which we deal with in a later chapter (1.13), physics is also treated in the *Dioptra* with its descriptions of the construction and use of instruments to measure angles, distances, levels and so on; it is a treatise on antique precision mechanics and includes the *odometer*, as Hero called it, or *taximeter*, as we now know it - the tool to measure road distances.

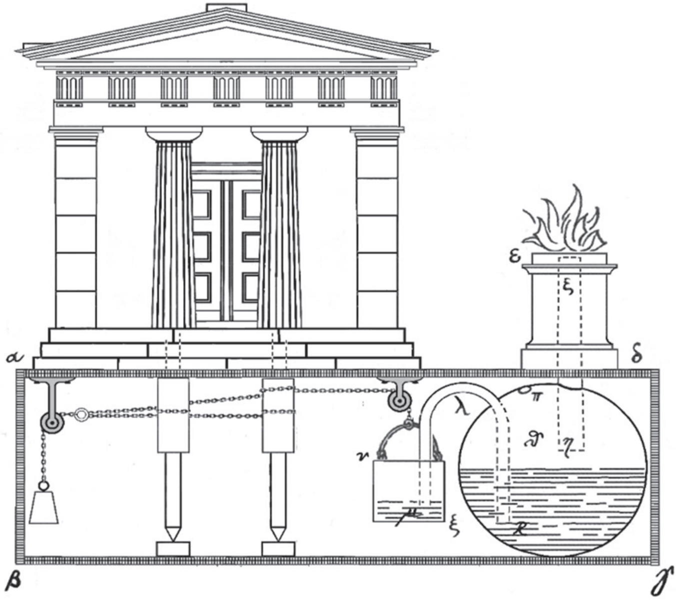

Fig. 1.4 - Hero's apparatus for the automatic opening of the temple doors. The flame lit on the bronze altar dilates the air in θ causing an overflow of water in ξ through the siphon; the heavier vessel falls and the doors open.
Source*: Heronis Alexandrini*, op. cit.

Opinions on Hero vary widely: some glorify him as a brilliant technician and others put him at the level of a mere "transcriber who paid little attention to experimentation and the practical construction of apparatus".[12]

There is no doubt Hero spread scientific knowledge and he made no mystery of that. His work, therefore, is testimony not to his genius but rather to the technical achievements of the Greeks in the Hellenistic age. They understood simple machines and gears, hydrostatics, all the various applications of the siphon, the compressibility of air and the motive power of steam. In other words, they possessed both the techniques and scientific knowledge to build industrial machinery and anticipate the 18th century. But what did they do?

They studied games and built toys for parties; they invented magical effects for religious ceremonies to increase people's superstitious beliefs;

[12] L. Heiberg, *Mathematics, natural sciences and medicine in the ancient world* It. translation, Stock., Rome 1924, p. 125.

built cannons and catapults. These trends in Alexandrine science and technology can only partly be explained by objective reasons, for example the lack of energy and primary materials (especially iron and fuels), but, at least in part, they can be explained by the social structure of the time, with the Grecian contempt for manual labour, their relative disinterest in material goods, coupled with the instinctive feeling of being at the mercy of occult powers. Greek philosophers and scientists firmly believed that humans could understand the world but not change it: harnessing the forces of nature and using them to their own profit was basically outside the scope of Greek thinking.

1.10 Greek optics

Another merit of the Alexandrian science that should be mentioned is the advance made in the study of optics.

Optics had fascinated philosophers in the Hellenic period who treated it more as a physiological question than a question of physics. They asked: how do we see? What is the relation between the sentient and the object observed? The argument must have been long and passionate; but there is not much documentation that has survived and what there is is difficult to interpret. We will outline the theories of the Hellenic age as they appear through history.

It seems that the Pythagoreans theorised that there was a special fluid emitted by the eyes that "touched" objects and captured, like tentacles, the sensations. On the contrary, the atomists believed in the emission by objects of image "rinds" (ε'ιδολα, which in the Middle Ages became "species") that, entering the eyes, endowed form and colour. Empedocles accepted the Pythagorean theory along with the idea of the light as "fluid matter" emitted from the light source (§ 1.2), with the consideration that vision is a subjective phenomenon and the eye is part of the process with the emission of rays to the outside: the sentient thus sees "something that is generated half way" by the meeting of the two opposing flows. The theory, totally unclear to us, was appropriated by Plato and therefore called "platonic". Plato admitted that objects emitted a special fluid that encountered the "dim light of day" that "smooth and dense" flows from our pupils. If the two fluids are similar, when they meet "they are tightly united" and the eye receives the sensation of sight; but if the "light of the eyes" (the only expression that has come down to us, in a translated sense, from the Platonic thinking) encounters a dissimilar fluid, it is extinguished and no longer transmits the sensation to the eye.

Aristotle adhered to neither the Pythagorean theory of expulsion nor the Democritian one of intrusion, but it is difficult to understand what theory he replaced them by. Some historians interpret an obscure part of *De anima* (II, 7) as an allusion to a theory of propagation of light based on the alteration of the medium between the eye and the observed object.

1.11 Euclidean optics

After abandoning philosophical speculation, studies in the Museum of Alexandria took a completely different direction. The oldest known document on these studies is the treatise on optics by Euclid, the great geometrician who flourished around 300 B.C. (Fig. 1.5). The treatise is divided into two parts, *Ottica* and *Catottrica*, but most experts attribute the *Catottrica* to a later author.

Euclid followed the Platonic theory of sight, as found in the first premise or postulate: "Rays emitted by the eye travel directly";[13] the second premise has given us the concept of the visual cone and, in a translated sense, of the view point: "The figure contained in the visual rays is a cone with its vertex in the eye and the base at the edge of the object".

Euclid based his geometric theory on these and another ten (or twelve according to other sources) premises. In the *Ottica* he examines the geometric questions of the postulated rectilinear propagation of light: shade, images produced through small apertures, apparent size of objects and their distance from the observing eye. The *Catottrica* studies the phenomena of flat and spherical mirrors. The second premise is a strike of genius: "Everything we see is seen in a straight direction"[14]. This is the fundamental principle of physiological optics (external projection or externalisation of luminous impressions on the retina) but it is not understood how that can be reconciled with the third premise that lays out a precise law on the reflection of light, known to the ancient Greeks. If the ray of light is identical to the *light of the eye*, how can it not bend in the mirror according to the second premise and bend to conform to the third? The readers don't be surprised by this contradiction. In the story of physics, there are many such contradictions and scientists have always dealt with them in the same way Euclid did: silence.

[13] G. Ovio, *L'Ottica di Euclide*, Milan, 1918, p. 21.
[14] *Ibid.*, p. 233.

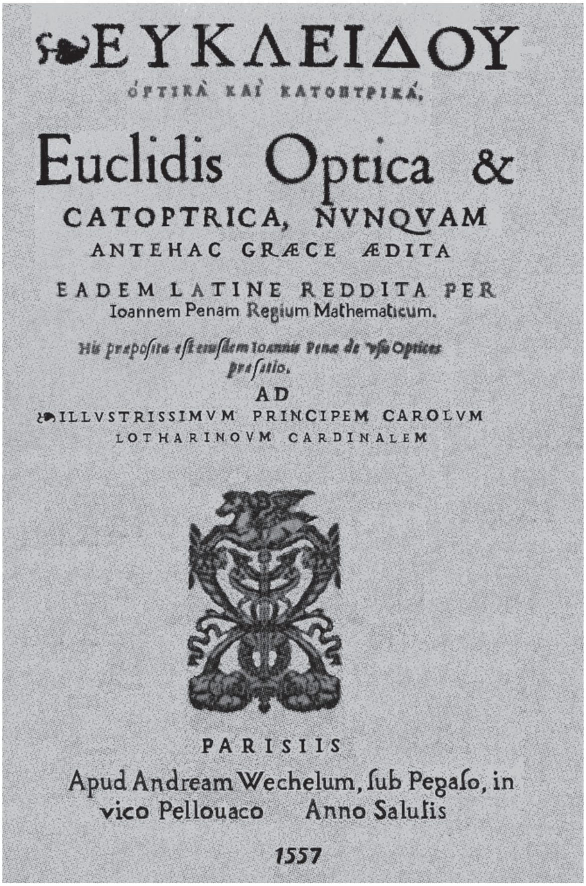
ΕΥΚΛΕΙΔΟΥ
ὀπτικὰ καὶ κατοπτρικά.
Euclidis Optica &
CATOPTRICA, NVNQVAM
ANTEHAC GRÆCE ÆDITA
EADEM LATINE REDDITA PER
Ioannem Penam Regium Mathematicum.
His præposita est eiusdem Ioannis Penæ de vsu Optices
præfatio.
AD
ILLVSTRISSIMVM PRINCIPEM CAROLVM
LOTHARINGVM CARDINALEM
PARISIIS
Apud Andream Wechelum, ſub Pegaſo, in
vico Pellouaco Anno Salutis
1557

Fig. 1.5 - Title page of a Renaissance translation of Euclid's *Ottica* (Paris 1557)

His captotrics also contain conclusions that conform to modern science. For example, that in flat mirrors the image is symmetrical to the object with respect to the mirror, but that in spherical mirrors the image is seen on a straight line between the object and the centre of the mirror; in convex mirrors, the image appears at a less distance from the mirror than the object itself, and is smaller, and so on.

The achievements of the Greeks in the field of geometric optics are largely due more to their success in geometry than in physics. Euclid in fact emphasises the geometric nature of his studies; however, some statements show a clear experimental origin. The seventh premise of the *Catottrica*, for example, describes an experiment that, still today, after more than two thousand years, is repeated in schools: "If any object is placed at the bottom of a vase, and the vase is gradually moved away until the object cannot be seen, the object becomes visible again at that distance if the vase is filled with water"[15]. It is interesting to note that this an experiment in refraction that has nothing to do with captotrics, and in the rest of the study it is not mentioned. Why did the author, Euclid or whoever, cite it?

In addition to the numerous other observations that there is no space for here, the last proposition is also of an experimental nature, the thirty-first: "By placing concave mirrors in direct sunlight, one may light a fire"[16]**.** The demonstration describes the Sun's rays that fall on the mirror, and readers must then once again ask themselves the question that Euclid does not answer: does the light come from the Sun or from the eyes? Evidently, the double flow theory permits both, whichever is easiest.

Even these few examples clearly show that Euclid, or the unknown author of these texts, has a place among the greatest physicists of antiquity, and also among theoretical physicists. He created the model of rectilinear light rays, a basic element of geometrical optics down the ages, and was the first to give a rational explanation of the formation of images in flat and spherical mirrors.

1.12 The optics of Ptolemy

Another treatise on optics followed in the wake of the Euclidean tradition, handed down to us from antiquity: the great astronomer Claudio Ptolemy, who lived in the Antonine age (II century A.C.). Ptolemy's *Ottica* has come down to us, apart from the first book, in a Latin translation from the Arab. It was severely criticised by some mathematicians, who judged it unworthy of a, albeit mediocre, scientist in geometry. In any case, the work remains a monument in Greek physics. The great astronomer does not stop, as Euclid did, at studying prospective, but deals also with the physical processes of sight and the consequent optical illusions. Like Euclid, Ptolemy adhered to the Platonic theory of sight.

The study of the refraction of light in air-water, air-glass, and water-glass is especially important. The experiments Ptolemy describes were carried out using an apparatus that is basically the same as that used in modern elementary teaching: a graduated circle with two targets, the centre of which coincides with the centre of a semi-cylindrical container full of water or the centre of a glass semi-cylinder. Ptolemy notes that refraction

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[15] *Ibid.,* p. 234.
[16] *Ibid.*, p. 411.

always occurs, in the passing from one medium to another denser one, at the surface of the separation of the two mediums and enunciates without difficulty the first law: the incident ray and the refracted ray are on a plan perpendicular to the refracting surface (or the plan tangent to the point of incidence). By experimentation, he later determined the angle of refraction corresponding to variable angles of incidence in an arithmetic progression of 10° in 10° from 0° to 80°. The results were applauded by historians for their exactness, especially the angles of incidence as 30° and 60°. For example, in the air-water passage, modern measurements give in correspondence to an angle of incidence of 30°, an average angle of refraction of 21° 54', and Ptolemy gave 22° 30'; for 60°, today we have 40° 6'against Ptolemy's 40° 30'.

But historians have also observed that the "second differences" between Ptolemy's angles of refraction are always equal to 30'. This constancy is questionable, leading to the conclusion that Ptolemy's data are not the result of experimental measurements, or at least are the result of direct measurements only in a few cases. In other cases, the results have been slightly altered to make them correspond to the theory that Ptolemy expounded but to which he makes no reference. According to Gilberto Govi, Ptolemy's theory may be expressed with the formula:

$$\rho = ai - bi^2$$

where ρ is the angle of refraction, a and b are the relative constants of the analysed media. For example, in the passage of air to water, the formula is $a = 0{,}825$ and $b = 0{,}0025$.

If Ptolemy rounded up the results of his experiments, his successors were even more dishonest, since they took as constant the ratio between the angle of incidence and the angle of refraction, simply writing

$$\rho = ai$$

And that was the law of refraction for physicists until Descartes.

According to Ptolemy, a refracted image is seen by the eye in the intersection of the extension of the incident ray with the normal at the refrangent surface conducted by the object-point.

Another important contribution to optics made by Ptolemy is the precise study of astronomical refraction that is mentioned (before or after Ptolemy?) in Cleomedes (mid-II century A.D.?). Ptolemy rightly deduces that due to astronomical refraction, the stars appear raised, therefore stars can be seen on the horizon that have not yet risen and stars that have already set are visible.

1.13 Hero's catoptrics

A short treatise by Hero on catoptrics, which we have only in the Latin version, would not be worth mentioning if it did not contain a proposition that has some relationship to Fermat's theorem, and the importance of which in modern physics the wave mechanics has revived.

Hero's treatise deals with mirrors and is mainly dedicated, according to the inclinations of the author, to demonstrate the astonishing effects of their attentive use. The IV proposition affirms that: "I say that of all rays exiting from the same point and reflected in the same point, the smallest ones are those that are reflected at equal angles in flat and spherical mirrors; vice versa, if this occurs, the reflection is at equal angles"[17]. This is followed by a very elementary and well-known demonstration. The second part of the theorem is an interesting geometrical observation that has made a direct contribution, as we will see, to Fermat's theorem, but that is still a long way ahead.

To conclude, Greek optics give us a considerable number of various physical, physiological, and psychological experimental observations, mixed together and sometimes confused, form which, however, Greek rational genius made possible the appearance of a new branch of science, geometrical optics, that stands by itself: quite an achievement.

THE GRECO-ROMAN AGE

1.14 Decline of Hellenistic science

The decline of Hellenistic science had already begun at the time of Ptolemy and Hero, towards the beginning of the modern era. The main reasons were the internal wars between the Greek states, the progressive disinterest of the princes in science, the loss of Alexandria's scientific leadership under Ptolemy VIII Physcon (146-145 B.C.), the fire that, in Caesar's time, destroyed a large part of the Library of Alexandria, the new religious sects that sprung up in the East, worldwide social unrest, and the new impulse from astrology and magic, especially in Egypt.

Fresh original research was replaced by compilation, tired repetition, erudite reworkings. The creative spirit gave way to a diffidence to science:

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[17] H*ero, De speculis, IV,* in *Heronis Alexandini,* op. cit, Vol. 2 (1900, p. 324).

Sextus Empiricus (c. 200 A.D.) is its most famous representative. The new scepticism criticised science for its presumptive deductive demonstrations; against inductive proof, Sextus Empericus observes that the collection of details cannot ever legitimately lead to a universal formula, because it is impossible to compile a complete collection of all the details that are infinite, and incomplete compilation risks missing that detail that contrasts with the universal. Sextus Empericus's famous criticism of the concept of cause is a basic element of XX century physics. If cause is that which produces an effect, it cannot be considered as preceding the effect, because, until the effect is seen, there is no relative cause. Neither can it be contemporary to the effect, as this presupposes the cause and its action; nor as after the effect, as it is ridiculous to consider the effect before the cause.

1.15 The Romans

Before coming into contact with the Greeks, the Romans has assimilated the Etruscan civilisation that, dominated by a religious mysticism, had little and rudimentary scientific knowledge, contaminated by magic and finalistic visions. That apart, the Etruscans were extremely able craftsmen: goldsmithing, architecture, irrigation, drainage.

There is no doubt that the Romans, like the Etruscans, made no appreciable contribution to scientific progress. They were not indifferent to, even depreciating of, science, as is often quoted: Lucretius, Cicero, Virgil, Pliny praised it highly in verse and prose; the Empire continued to administrate the Museum and Library of Alexandria. All the same, the Romans preferred literary and moral studies, and they preferred technical applications to pure "pointless" scientific research: roads, bridges, aqueducts, canals, architecture, metallurgy and glass-working are the practical applications in which the Romans excelled, writing an important chapter in the history of technique.

1.16 The Encyclopedias

Almost to compensate for the lack of original research, a large number of scientific encyclopedias were compiled and they are important as for centuries they were the only sources of information on Greek science.

Among the encyclopedias of direct interest to physics are the seven books of the *Quaestiones naturales* by Lucius Anneus Seneca (3 B.C. - 65 A.D.) that give an orderly treatment of celestial fire (streamers, rainbows, etc.); thunder and lightning; land water; the Nile; clouds and winds; earthquakes; comets. The major significance of Seneca's work, irrespective of some lucid albeit unoriginal pages on meteorology, is the precision and accuracy of his exposition of earlier philosophical ideas, that, in some cases, would not have come down to us.

One of the most famous ancient works, the ten-volume *De architectura,* was written by the Roman engineer and architect Vitruvius Pollio, who flourished in the Augustean Age. The first seven volumes deal specifically with architecture and art in general and have been translated and commented on since the Renaissance. With regards to physics, the last three books are of the greatest interest: the eighth on aqueducts, the ninth of the measurement of time, and the tenth on constructing machines.

Gaius Plinius Second the Elder (Como 23 A.D. - Naples 79 A.D.), a victim of the eruption of Vesuvius that buried under its ashes Pompeii and Herculanum, was a prolific polygraph. His fundamental work is the 37-volumes *Naturalis historia*, the fruit of the study of some two thousand books by one hundred different authors. It is a huge collection of diverse facts (twenty thousand, the author claims) concerning astronomy, meteorology, geography, the history of human evolution, zoology, metallurgy, engineering, etc. The facts are almost always reported without any critical evaluation, with a passive acceptance that often surprises the modern reader, also because Plinius often defines them as "experimental". But in Plinius, the *experimentum* does not have the unequivocal acceptance that will be found in the first Renaissance naturalists: for him, experimentation means as much the common experience and testimony and attestation; it also means medical prescription; it also sometimes has the modern acceptance.

The vast diffusion of Plinius's natural history, also in modern times, can be explained that from the first printed edition (Venice 1469) up to the end of XVIII century, there were no less than 190 reprints.

We may also include in this list of encyclopedias a great poem, the *De rerum natura* by Titus Lucretius Carus, born, it seems, in 99 B.C. and dead 55 B.C. Lucretius's poem was hugely influential in Renaissance science as a bridge between antique atomism, in the Epicurean version, and the Renaissance interpretation, and since it laid down the tradition of materialistic theories, and finally, and especially, because of the effect its ardent heartfelt verses had on driving Renaissance man towards freeing himself from the terrors of religion through science.

The first two volumes of *De rerum natura* deal with matter, space and vacuum, according to a particular epicurean atomistic theory. The third book concerns the spirit and the soul, both made up of atoms. The fourth volume deals with the theory of sensations, due to tiny images that break off from bodies. The fifth book is about cosmology (the formation of our world, the primitive spontaneous emergence of plants, animals and men, the survival of the fittest, social organisation, the prediction of the end of the world). Last, the sixth volume explains various meteorological phenomena such as lightning, clouds, rain, water-spouts, earthquakes, etc.

1.17 Philoponus

With the death of Manlius Severinus Boethius (480-524), also the tradition of Greek culture in the West was overwhelmed by the Barbaric invasions. On the contrary, in the East, Greek culture continued, although with some difficulty. Kept alive by Byzantine commentators, it would be conserved by the Arabs and would return to the West in the XIII century.

Commentators of note include John Philoponus, also known as John the Grammarian, flourished in Alexandria in the first half of the VI century. He wrote, demonstrating a notable free thinking, extensive comments on Aristotle. His commentaries of the first four volumes of *Fisica* have survived intact, together with fragments of the last four. In the commentary on the fourth volume, Philoponus devotes an entire chapter to a critique of the Aristotelian theories of vacuum and violent motion. If motion is interrupted by the mean, how can a body rotate around itself, as it does not move through the medium? And how can different rotating spheres move at times quickly and at times slowly?

These arguments led Philoponus to refute Aristotelian theory and propose that the motor imparts a certain force and power of movement to the projectile, that differs according to the greater or lesser speed. The force of movement, that in the Middle Ages was called "impetus", gradually decreases in the motion; so that, once the impetus is exausted, motion stops.

Contrary to Aristotle, and drawing on experience, Philoponus denied that heavier bodies fall faster. Although his commentary of Aristotle's *Fisica* has not survived intact, Philoponus emerges as the greatest mechanical theorist between Archimedes and Buridan: his ideas were so original for his time and anticipating 17th-century mechanics, that he was called, with a certain exaggeration, a precursor of Galileo.

2. The Middle Ages

MECHANICS

2.1 Arab mechanics

With the creation of their immense empire, the Arabs first scorned Greek culture; a diffidence that according to some historians led to the fire that destroyed the Library of Alexandria in 642. That accusation has never been proved and the first mention of the fire appears in the XIII century. Perhaps at the time of the Arab invasion very little was left of the famous library, partly destroyed by the Christians a few centuries earlier (a branch of the library, the Serapeium, was destroyed by bishop Theophilus around 390) and partly lost due to the negligence of the post-pagan culture that arose in Alexandria.

However, from around 750, the Arabs were fascinated by Greek culture. In the first phase of assimilation, that lasted a little over a century, Arab translations were made from the Greek of Syriac, Greek texts, overcoming philological problems and gradually creating an Arab scientific language. In the new capitals, Damascus and Baghdad, new schools were founded on the Alessandrian model. These were followed by an autonomous movement, first treating theological matters, and then questions of nature. Scientific research reached its greatest splendour in the XI century, and then rapidly declined.

The Greek origins of their science almost naturally led Arab physicists to study mechanics and optics, the only two chapters of physics that, as we have seen, were successfully cultivated by the Greeks. But the Arabs made truly important advances only in optics, as we shall explain later (§ 2.7).

In general mechanics, the Arabs followed Aristotle without introducing any significant changes. Without new theories, their great mechanical skill was limited, like the Alessandrians, to the construction of toys, automatons, and wheel and weight clocks. In the X century, there were some Muslim contributions to fluid statics: the astronomer al-Nairizi (Anaritius), died in 922, wrote a treatise on atmospheric phenomena; al-Razi (died 923), his contemporary who flourished in Baghdad, introduced the use of hydrostatic scales in the determination of specific weights, a subject of particular interest to Arab physicists, that seems to have led them to introduce the "rider" (counterweight) in precision measurement of weights.

In particular, al-Biruni (923-1048), renowned mathematician and astronomer, very accurately determined the specific weight of eighteen solids (metals and precious stones); he is also responsible for the explanation of natural fountains and artesian wells using the principle of communicating chambers, when artesian wells had not yet been introduced in the West. They would only appear in 1126 in Lillers, in the Artois region.

A contemporary of al-Biruni was the great Arab scientist, Abu Ali al-Husain ibn Abdallah ibn Sina, known on the West as Avicenna (980-1037), philosopher, physicist, mathematician and astronomer. In the theories of physics he generally followed Aristotle, but followed a separate path on some fundamental points, such as the theory of violent motion. Using diverse arguments, he refuted the Aristotelian theory of motion controlled by the means (§ 1.3). According to Avicenna, the impulse imparts a force to the object in the same way as fire heats water; during the movement, the force gradually decreases until being annulled: therefore, motion stops. In fact, it is Philoponus's theory, although expressed in an equally obscure way[18]. But, while Philoponus allows the possibility of a vacuum, Avicenna, following Aristotle, refutes it, proposing that, in the absence of an obstacle, motion in a vacuum would continue indefinitely, and the impetus imparted would never change or be cancelled. The principle of inertia would be treated as absurd for centuries!

Al-Khazini, flourished between 1115 and 1121, wrote a notable treatise on Medieval physics containing tables on the specific weights of solids and liquids, experiments of the gravity of air, observations of capillarity, and the use of air gauges to measure the density of liquids. His influence on the development of Western physics is very doubtful.

2.2 Cultural reawakening in the West

Even before al-Khazini, Arab physics was declining as fast as it had grown.

But at the same time, contact with the Arabs and the flourishing economy produced an intellectual reawakening in Spain, Lorraine, France and Scotland. The first bodies of learning and knowledge diffusion emerged in Italy: the universities. The universities of Salerno and Bologna were already

[18] The part first attracted the attention of S. Pines, *"Les precurseurs musulmans de la theorie de l'impetus*, in Archeion, "Archive of the History of Science", 21, 1938, pp. 298-306. Also P. Duhem (*Etudes sur Leonard da Vinc*i, Hermann, Paris 1909, Vol. 2, p. 191) discovered traces of Philoponus's teaching in Arab works.

famous around 1100 and at about the same time the universities of Paris and Montpellier had made a name for themselves. The universities of Cambridge (1209), Padua (1222), Oxford (1229), Naples, Rome and so on were set up following the same models (the dates of foundation are uncertain and purely indicative).

Between approximately 1125 and 1280, Spain and Italy produced translations of Aristotle, Euclid and Ptolemy that began scholastic teaching. It is almost certain that in this period the writings of Archimedes and Hero were unknown, so all mechanical studies were based on the treatises of Aristotle.

Between the XII and XIII centuries, in a patient and inestimably important undertaking, Latin translations were produced of Arab, Greek, Hebrew and Celtic works. Among the most famous of the translators from Arabic to Latin were Adelard of Bath, born around 1170, and Gherardo of Cremona (approx. 1114-1187), who has the great merit of having given the Latin world a large part of Greek and Arabic scientific culture. He lived most of his life in Toledo, then the principal centre for translators.

These activities were accompanied by a renewed interest in encyclopedias that gradually replaced the allegorical-mystical observations of nature and descriptions of technical processes. Examples of this type of encyclopedias are *De naturis rerum* by Alexander Neckam (1157-1217), in which we find one of the first Western descriptions of the compass; the *De proprietatibus rerum*, written around 1240 by the English author Berthelemy, and another *De naturis rerum* by Thomas of Cantimpré, compiled between 1230 and 1250. Other encyclopedic works were created by Albertus Magnus (1206-1280) that earned him the title *doctor universalis*.

2.3 Medieval statics

Pierre-Maurice Duhem (1861-1916), using numerous manuscripts held in French libraries, especially the Paris National Library, claimed in weighty and important works that the first medieval universities and schools not only preserved and assimilated the conquests of Greek physics, but also founded modern physics that would develop dialectically from Aristotelian physics. Drawn by his fascination for medieval science, Duhem arrived at setting the date of the birth of modern science: 1277, the year in which the bishop of Paris condemned the 219 errors that some students had discussed in the schools. The "contemptible errors" contained two relating to cosmology: that God could not have imparted translation to the heavens, as this motion would have left a vacuum behind it; and that God could not create more than one world. According to Duhem, by allowing new mental experiences, the sentence liberated the spirits from the finite in which Greek philosophy had limited the universe.

Some of Duhem's criteria are questionable, his anti-Galilean opinions do not always seem to have critical foundations and his general conclusions sometimes appear hasty. However, it is to his merit to have rediscovered such singular scientists as Giordano Nemorario, Giovanni Buridano, Nicola Oresme, and to have initiated fertile research into medieval science, that is still being developed. But the contribution made in this epoch to the construction of modern physics is too limited, as the reader may understand from the few references made in this, and following, chapters, to accept the idea of a "XIV-century scientific revolution".

These additions began with Giordano Nemorario, about whom we know nothing, not even his nationality and his dates, perhaps between the XI and XIII centuries. Duhem discovered in the French libraries some treatises on statics attributed to him, including a notable systematic use of the concept of *gravitas secundum situm* or *gravitas accidentalis* (accidental weight), that is that variation of the force of gravity of a body according to its position is equal to the minimum force required to impede the motion of a body subject to constraints. For example, with a body positioned on an inclined plane, the accidental weight is what we now refer to as the component of the weight in the direction of the inclined plane. For a body immersed in a liquid, it is the apparent weight, that is the difference between its weight and the Archimedes's thrust. According to Duhem's reconstruction, this concept leads Nemorarius to the principle of virtual work, expounded in the following way: if a certain weight may be lifted to a certain height, a heavier weight may be lifted to a proportionally lower height.

Another Nemorarius, perhaps a student of the former, and whom Duhem oddly calls the "precursor of Leonardo", is the creator of the idea of static momentum, already found in Archimedes (§ 1.6), and its application to the study of angular levers and inclined planes. It is important to note that this second Nemorarius, in a pamphlet included in the papers of Tartaglia and published in 1565, gives the exact equilibrium of a body positioned on an inclined plane, in a proposition that Tartaglia claimed as his own, as we shall see later (§ 3.1.2).

The writings of Nemorarius were known to medieval scientists up to the Renaissance. They inspired the unpublished manuscript *Tractatus de ponderibus,* handwitten by Biagio Pelacani, or Policani, better known as Biagio from Parma, died 1416, who was a professor at Bologna and Padua; copies of it are held in the National Library of Paris and the Laurenziana in Florence.

2.4 Dynamics in the Paris school

From the start of the XIV century to the end of the XVI century, commentaries on Aristotle multiply, as well as long and boring scholastic arguments over his physics. In these three centuries of assimilation and polemics, a very important period for the development of dynamics in the University of Paris was the first half of the XIV century; the “stars” were Giovanni Buridano, Albert of Saxony, and Nicola Oresme.

Giovanni Buridano (c. 1300-1358 or after) was rector of the University of Paris and author of numerous works, the most important of which for physics is the *Quaestiones totius libri physicorum*, only published in 1509.

The importance of Buridano lies especially in the critique of Aristotle’s theory of motion. Buridano takes up the subject, widens and applies Philoponus’s (§ 1.17) critique, that Western scientists, particularly Pietro Olivi (1248-1298), may well have come across through the Arabs. How can the motion of a wheel be sustained by air? And why can a heavier body be thrown further? If motion is due to the impulse of air on the back of the body, an arrow with a thinner end should move more slowly than another without a sharp end; in fact, the opposite happens, why?

The possibility of explaining these simple experiments led Buridano to support the theory of “impetus” (and it would seem it was he who introduced this term in a technical sense) given by the pitcher to the object in motion: the greater the quantity of the matter and the velocity of the projectile, the greater the impetus. Because of the resistance of the air and the weight of the stone launched, it functions contrary to the impetus given, it will decrease continuously. When the impetus ceases, motion stops. Buridano applies the theory to a bouncing ball, to the oscillations of a bell and to the free fall of a body, which acquires by itself impetus that, with the addition of gravity, accelerates the motion.

Duhem interprets the theory of impetus as a forerunner of the principle of inertia. But maybe he was too precipitous. For modern science, from Galileo onward, rest and uniform rectilinear motion are, as Annelise Maier and Alexandre Koré (1892-1964) correctly observed, a state of bodies, and therefore no-one ever asked why, or what force is responsible for, a body remains motionless. For Buridano, on the other hand, motion is always due to the action of an intrinsic force: we are still conceptually very far from the principle of inertia, even though the theory of impetus represents great progress compared to the Aristotelian concept.

The same theory was supported by a great follower of Buridano, Albert of Helmstaedt, also known as Albert of Saxony, who taught at the Sorbonne between 1350 and 1361. His most popular work is the *Tractatus proportionum*, published in Padua, perhaps for the first time, in 1482, in which there is an exact definition of angular velocity (*velocitas circuitationis*) and a first attempt to classify motions, translation (uniform and variant), and rotation. At the same time, and perhaps due to the influence of Albert, there arose the concept of uniformly variant motion, or *uniformiter difformis*. In the *Quaestiones subtilissimae in libros de Coelo et Mundo*, printed in Pavia in 1481, Buridano also studies the motion of falling weights and advances four hypotheses on the variations of falling velocities: that velocity increases in arithmetic proportion to space and time; that increases in speed form a progressive geometric decrease with respect to time and space. Using arguments that today seem worthless, he concludes, excluding the other hypotheses, that velocity increases in proportion to space.

The third Parisian master was Nicola Oresne, born in the third decade of the XIV century and died in 1382, professor of theology at Navarra college in Paris. A disciple of the dynamics of Buridano and Alberto, Duhem declares him, not without some exaggeration, to be a precursor of Copernicus, Galileo and Descartes. Oresme does have a claim to being the first to use a graphic representation corresponding to our use of coordinates, constructing a diagram of velocity as a function of time. This diagram allowed him to establish that in uniformly variable motion with no initial velocity, the space covered is equal to the space that in the same time would be covered by a body with uniform motion with a velocity reached after *t/2*. Oresme also made some interesting geometric observations on a series of uniform motions.

2.5 Dynamics in the school of Oxford

The school of Oxford was contemporary to, and rival of, the school of Paris: the latter, dominated by nominalistic Domenicans; the first by Aristotelian Franciscans and Augustinians, logicians and mathematicians more than physicists.

At its foundation, in 1209, the Franciscans appointed as teacher Robert Greathead (c. 1175-1253), considered by many as the true bringer of Aristotle's thinking to the West. Greathead holds an honourable place in science, but he does not seem to have made any specifically important contributions. His fundamental merit is to have founded a school which produced Roger Bacon, a person of great scientific importance as we shall see later.

The Oxford school also included William Heytesbury (Hentisberus), perhaps 1315-1371, author of widespread treatises, published in Venice in 1494. Heytesbury had an exact concept of positive and negative acceleration (*velocitas intensionis vel remissionis*); he understood Oresme's law on the space travelled in uniformly accelerated motion and knew that in the first half of the time, the space travelled is a third of that travelled in the second half: this latter theorem would be developed by another master of the Oxford school, William Colligham, with the proposition that the space traveled in successive equal times increases as a natural series of odd numbers.

While the school of Paris did not hesitate to argue against Aristotle, the Oxford school searched above all for a mathematical interpretation of his physics. Thomas Brawardine (1290-1349) was the most fortunate of this line of thinking. In his *Tractatus proportionum* of 1328, he attempts to give a mathematical formulation of Aristotle's law of motion, arriving at an expression that we would now term logarithmic. Although Brawardine's formula has not been accepted into classical physics, it does have the merit of having introduced the concept that the resistance of the medium rapidly increases with the velocity.

2.6 Diffusion in Italy of the dynamics of Paris and Oxford

As we have seen, with an important scrutiny of the manuscripts held in French libraries, Duhem was able to trace the history of physics in his country. That history is often tarnished by a myriad of redundant erudite comments and purely bibliographic information, but, overall, the guidelines of the development of thinking are well defined. There was no similar experience in Italy; many manuscripts held in Italian libraries are still to be discovered and printed works had been little studied, if at all. As a result, the development of mechanics in XIV and XV century in Italy is either badly or completely unknown.

We do know that Italian universities commentated the books of Aristotle, in particular *Fisica.* This obligatory teaching forced professors to explain, and then accept or refute, Aristotelian dynamics, to support or argue against the theories of Buridano, Alberto, Oresme and the Oxford School. Biagio of Parma, mentioned before, penned a comment on the works of Oresme in his *Quaestiones super tractatus de latitudinibus formarum*, printed in Venice in 1486 and 1505. In his *Summa totius philosophiae*, a scientific encyclopedia published in Milan in 1476 and repeatedly reprinted, Paolo Nicoletti (c. 1370-1429), known as Paolo Veneto, supported the theory of impetus, but then refuted it in the later *Expositio super octo libros physicorum Aristotelis*, written in 1409 and printed in 1499. One of his pupils, Gaetano da Thiene, born in Vicenza and died 1465, composed a commentary (*De tribus praedicamentis; Regulae solvendi sophismata; Recollectae super octo libros physicorum*) which follows the theory of impetus but does not apply it to the acceleration of falling bodies, that he explains using traditional Aristotelian theories. With regard to uniformly varied motion, he uses the graphic representations of Oresme and clarifies the concepts of instantaneous velocity (*latitudo motus*) and acceleration (*latitudo intensionis motus* or *velocitatio motus)*.

In summary, also from the documents examined thus far, it may be claimed that around the mid-15th century, the theories of the Paris and Oxford Schools were well known in Italy, and some aspects were clarified and enlarged.

The lively humanistic activities in Italy, favoured by the numerous libraries full of manuscripts, and especially the many typographies (at the end of the 15th century, of the 70 in Europe, 50 were to be found in Venice), brought to light new Greek scientific works. More specifically, with regard to mechanics, it is interesting to note that the *Problemi meccanici* attributed to Aristotle (§ 1.3), almost certainly unknown in the Middle Age, appeared in 1497 with the Aldine "*editio princeps"* of the philosopher's works. They were translated by Vittore Fausto in 1517, and again in 1525 by Niccolò Leonico Tomeo (1456-1531) in an annotated edition complete with illustrations (missing from the previous edition).

The aforementioned fervour for studying explains why in the 16th century almost all progress in mechanics was made in Italy, as we shall see in the next chapter. With regard to dynamics, the most significant exception is the Spanish Dominican friar Domenico Soto (1494-1560) who, in a commentary on Aristotelian physics, admits, with no justification, that the motion of the falling bodies is uniformly variant and he gives a rule that is the same as the modern one for the distances covered by falling bodies.

OPTICS

2.7 Alhazen

The most brilliant period of Arab physics is undoubtedly that of Ibn al-Haythan, known in the West also as Alhazen, who flourished in Egypt at the same time as al-Biruni and died in Cairo in 1039. It is no exaggeration to claim that Alhazen, astronomer and mathematician, was also the greatest physicist of the Middle Ages, as well as being a commentator of Aristotle and Galenus.

The latter aspect is particularly important to our history. Among the many merits of Galenus, who lived between 130 and 201 A.C., is the consideration of the eye of one of the sensory organs of our organism: he described the structure and highlighted the function of the optic nerve in sight. In the theory of vision, Galenus basically followed Platonic theories, but gave greater importance to the external fluid emitted by the Sun and also stated that "eyelight", secreted by the brain, is transferred from the optic nerve to the retina and from there passed to the vitreous humour and the crystalline lens, in his opinion the organ of perception. In sum, with Galenus the structure of the sensory organ takes its place in the mechanism of vision.

Without doubt, Alhazen adopted Galenus's description of the eye, but rejected the "light of the eye" as an impediment; in his first fundamental proposal he states: "Natural light and illuminated colour damage the eyes",[19] and supports his argument with the observation that the eye feels pain in direct sunlight or sunlight reflected from a mirror, and cites other examples of dazzling. By "natural light" (*lux per se*), Alhazen intends the white light of the Sun and illuminated colour as the light emitted from a coloured object.

Therefore, in a series of well-conducted physical-physiological experiments, he demonstrates that the idea of light issuing from the eye to touch the object is unsustainable. In Chapter 4 he describes the anatomical structure of the eye, borrowing heavily from Galenus, also in the next Chapter he claims "Vision occurs due to rays emitted by the object towards the eye".[20]

This is not Euclid's luminous ray, but a sort of inverted ray of light: it does not pass from the eye to the object, but from the object to the eye. It is not Alhazen's most original idea, as the concept of the inversion of the luminous ray can be found in his contemporary Avicenna, who adds that if light is due to the emission of particles by the source, its velocity must be finite. In Euclid, as with all Greek physicists, vision was considered a global phenomenon: a sentient person immediately perceived, in a single process, the image of the entire observed object either because its covering infiltrated the pupil or because the light of the eyes sensed it in every part at the same time. Alhazen, in the contrary, broke down this global process into an infinity of elementary processes with an ingenious theory: to each point of the observed object there is a corresponding point impressed on the eye. But to explain that there are no special directions to see an object, it has to be admitted that infinite rays are emitted from each point and strike, infinite, the pupil. So, how can one point of the object correspond to a single imprinted point? Alhazen explained away the problem by stating that of all the rays penetrating the eye, the only efficacious one is that perpendicular to all the ocular tunics that he believes concentric. It therefore leaves an

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[19]*Opticae thesaurus Alhazeni Arabi libri septem*, edited by F. Risner, Basel, 1572, p. 1

[20]*Ibid.,* p. 7.

impression of the anterior surface of the crystalline lens, the seat, according to Alhazen, of the sensation; the ray that, starting from each point of the object observed, passes through the geometric centre of the eye. In this way, Alhazen establishes an exact correspondence between the points of the object and the points impressed on the anterior face of the crystalline lens, and can claim: "Sight occurs through a pyramid whose vertex is in the eye and the base in the object seen".[21]

What a difference between this proposition and Euclid! It maintains the classical prospective but overturns physics and, despite its incompleteness, represents a great step forward.

But why did Alhazen not prolong the luminous rays beyond the centre of the eye up to the retina, making this where the image is formed? Perhaps because he did not understand how the retina works, even though he knew its nervous structure? In fact, we have to wait until Ibn Rushd, known in the West as Averroes (1126-1198), to see explicitly recognized the functional role of the retina; all the same, the discovery was forgotten until the anatomist Felix Plater (1536-1614), which would prove that such a discovery was anything but pacific. In any case, it is strange that someone as perspicacious as Alhazen did not find the impression on the crystalline lens odd. We may, therefore, suppose that Alhazen asked the question without giving an answer, fearing the consequences. In fact, if the rays cross at the centre of the eye, where -up to Realdo Colombo (1516-1559), the crystalline lens was located- a reverse image is formed; and who had ever seen an upside-down world with his own eyes?

Besides elementary geometrical considerations, Alhazen knew from experience that inverted images would be formed on the retina. In fact, later in the above document, he experiments with a "dark room" to prove that rays of light issuing from different bodies may intersect without being altered. He placed some candles in front of a wall in a darkened room, with a hole, and, observing the facing wall, saw the light of all the candles, "and if you cover one candle, the corresponding light on the wall disappears; and if you remove the cover, the light returns. And this can be demonstrated at any moment; therefore, if light is mixed in air, also the air in the hole would be mixed, and would pass in mixed form through the hole, and after the hole they would not be distinguishable. But we find that this is not true, therefore the rays of light are not mixed".[22] It is immediately clear from this passage that Alhazen had frequently and diligently used a dark room; consequently, he must have observed the reversal of the images, even though he makes no mention of it here.

[21] *Ibid.,* p. 10.
[22] *Ibid.,* p 17

We have to wait until Leonardo da Vinci for a scientist with the genius and courage to use this heritage to deduct the mechanisms of sight. Leonardo describes the dark room in much greater detail and, by observing the reversal of the images, concludes “This is done inside the pupil”.[23] Six words, but what a discovery!

But let’s return to Alhazen, who pursued his theory of sight, without being convinced and continually changing and adapting it according to his optical experiments. It should be noted here that medieval geometrical optics were much more difficult than ours, because we see images on a screen, while in the Middle Ages observation used the eye, resulting in an also physiological optics rather than purely physical. After Book II, dedicated to a discussion of types of vision, the whole Book III deals with optical illusions, translated as *hallucinationes* or *deceptiones visus*. Despite the interesting comments on physiological optics, Alhazen’sAlhazen’s contribution to physics was rather negative. Because it was inspired, or supported, by the mystical trend, which we shall find still strong in the era of Galileo, that distrusted the perception of the senses, in particular what we see: it is very easy - as Alhazen also has it - “to mistake fireflies for candles”!

Books IV-VI deal with the experimental and geometric study of flat, spherical, cylindrical and conical mirrors. Proposition 39 of Book V lays out the famous problem over spherical mirrors called Alhazen’s problem: given a mirror, a luminous point and an eye, determine the point in the mirror where the refection occurs. Alhazen solves the problem, in a contorted and confused way that is still difficult to follow, by recourse to the intersection of a hyperbole with a circumference. The problem occupied mathematicians for many centuries and it was not solved until 1676 when Christiaan Huygens first gave a simple geometric answer, and in 1776 Abraham Kaestner (1719-1800) laid down the first analytic solution that leads to an equation of fourth degree. After numerous attempts, that often led him to wrongly suppose he had found the answer, and other times claiming it was unsolvable, Leonardo da Vinci finally explained it “due to an instrument”, by constructing a mechanical tool notable for the first use

[23] Leonardo da Vinci, ms D, f.8*r*, according to historical records of Leonardo’s manuscripts, in the Institut de France, and which Giovan Battista Venturi (the first to study them) listed from A to M. They were first published in facsimile with a “diplomatic” transcription (that is literal: as we know, Leonardo wrote from left to right, maybe to keep his secrets, or maybe just to be bizarre) and French translation by C. Ravaisson-Molien, *Les manuscrits de Leonard de Vinci*, 6 volumes, Paris, 1881-91. The Da Vinci Commission reprinted them. Manuscript D is one of the better compiled and complete; it is an original treatise on physiological optics that also, for the first time, mentions the dilatory changes in pupil depending on the intensity of light (*ms. D, f. 5v)*.

of a five-point articulated system. The instrument, reconstructed in 1929, is the property of the Institute of Physics of the University of Naples and the italian National Research Council.[24] The last book, number VII, of Alhazen Optics is entirely given over to refraction. Mention should be made of the perfecting of Ptolemy's instrument (§ 1.12) for the experimental study of the phenomenon and the more accurate measurements that resulted from it but that, however, did not allowed Alhazen to lay out the exact law. Nonetheless, a new concept is introduced in the treatment that would be used to great effect by Descartes (§ 5.5).

To interpret the phenomena of reflection and refraction, Alhazen established a parallel between the motion of projectiles and the motion of light: as a spherical body, thrown against a flat surface, is reflected at equal angles, so light, travelling at a very high speed, is reflected at equal angles when it meets the mirror. Alhazen explains the equality of the angles by breaking down the speed of the projectile into two components: one parallel and one normal to the reflecting surface, leaving the parallel component unchanged and inverting the normal. He also used mechanical models in treating refraction. If you throw an iron ball against thin plates, so as to perforate them, you can note the trajectory of the ball, after the perforation, approaching the normal. An analogous phenomenon occurs with light and Alhazen explains both the mechanical phenomenon and the light phenomenon, dividing the velocity into two components, one normal and one parallel to the refracting surfaces; he leaves the parallel component unchanged and varies the normal component, thereby obtaining the change in direction. To justify the change in the normal component, Alhazen states: "Lights propagated by diaphanous bodies are propagated at high speed, as a result of that speed, the senses do not perceive it. Therefore, their motion in thin bodies, that is very diaphanous ones, is faster than in thicker, less diaphanous, bodies. In fact, any diaphanous body, while the light passes through it, opposes a resistance that depends on its bulk (*secundum quod habet de grossitie*)".[25]

It may be deduced from this passage that, for Alhazen, the speed of light is lesser in denser mediums; but in this case, the ray of light, passing from a less dense medium to a denser one, should move away from the normal; that is, more or less the opposite of what Alhazen describes. In fact, he did not effectively performed speed breakdown; his considerations are simply intuitive. But what is important to note is not the deficiency of the technical part, but the novelty of the concept: the decomposition of the speed of light

[24] R. Marcolongo, *Memorie sulla geometria e la meccanica di Leonardo da Vinci*, Stabilimento industrie editoriali meridionali, Naples 1937, pp. 78-79
[25] O*pticae thesaurus, Alhazeni*, op. cit, p. 140

into its components, normal and parallel to the surfaces separating the two mediums.

2.8 Alhazen work in Western culture

Such an important treatise as Alhazen's, so new, so original and so well organised, was translated into Latin, probably by Gherardo of Cremona, or maybe in the following century by John Peckham (Bishop of Canterbury in 1279), and continued as a manuscript up to the first printed edition in 1572 by Friedrich Risner (d. 1580), that divided the work into books and chapters and improved it. In the Middle Ages, the treatise was more famous than known, and its author was referred to antonomastically as *Auctor perspectivae*. The name really should not be a surprise; as strange as it may seem to modern science: in the Middle Ages optics, prospective and meteorology were a single science.

We have said that the treatise was more famous than known, because the theory of vision, that is its most original part, met with little success in the Middle Ages. Throughout the 16th century, and beyond, the most common theory of vision remained the nebulous idea of simulacra or species, that depart from the objects and enter the pupils of the observer. It is not easy today to explain why Alhazen's theory was not fully accepted: perhaps his experimental treatment was too different to the usual philosophical treatments of the time and therefore appeared too difficult; perhaps the authority of ancient philosophers had the better on -in the Middle Ages- the authority of a recent writer, who was also an infidel; maybe the global process of sight, that may make us smile now, was of such immediate intuition to compensate for its serious theoretical insufficiency.

Much more widespread in the Middle Ages than Alhazen's treatise was Vitellione's treatise on optics, whose name we are not even sure of: Vitellione, Vitellio, Witelo? It would seem he was of Polish origin, stayed for a long time in Italy, studying in Padua approximately between 1262 and 1268 and then in Viterbo. Between 1270 and 1278, he wrote a treatise on optics in which, borrowing heavily from Euclid and Ptolemy, and above all Alhazen, he substantially set out the doctrine and methods of the Arab physician in better order.

There are two particular new elements compared to Alhazen's treatise: an accurate study of rainbows and a demonstration that parabolic mirrors have a single focus (the word *focus* in its modern sense in physics would be introduced by Kepler in *Ad Vitellonem paralipomena quibus astronomiae pars optica traditur*, 1604), a property containing, according to a fragment discovered in 1881 by a Greek writer, perhaps Artermios of Tralle (474-534 A.C.).

We have already seen (§ 1.3) how the rainbow, due to the magnificence and power of the phenomenon, had attracted the attention of the first Greek observers, but we have to wait until Descartes for the first satisfactory interpretation. Witelo (Witelonis/Vitellione) noted that a rainbow could not be explained as a simple reflection of light on water drops, but must be influenced by the refraction of solar rays in those rain drops.

2.9 Roger Bacon

Around the same time, to demonstrate that *nulla scientia potest sciri sine mathematica* (no science can be known without mathematics), Roger Bacon gave an accurate description of the phenomenon in 10 chapters of Part VI of his *Opus majus*. Bacon precisely traces the path of luminous rays and finds the height of a rainbow in 42°. But the modern reader will be very surprised to read in these pages that the colours of the rainbow are a subjective sensation, caused by humidity in the eye.

Roger Bacon, the most illustrious disciple of Robert Greathead and famous Franciscan monk, was born around 1214, perhaps in Ilchester in the Somerset levels (but some claim he was French) and died around 1292. His life and work are surrounded by legend, perhaps fed, through reaction, by a hatred of the Scholastics for his position against Albertus Magnus and Thomas Aquinas. One result of the legend is the various inventions attributed to him: gun-powder, lenses, the telescope or spyglass, the compass, the steam engine, aeroplanes, to name just the most common.

He was claimed as precursor of the experimental method and, in truth, Part VI of the *Opus majus* is entitled *De scientia sperimentali* and contains wonderful pages on the value of experiments. But for Bacon, the word has a much broader meaning than its modern one. He says: “Experiences are double: one through the external senses [...] but this experience is not enough for man, because it does not fully certify corporeal things and does not touch spiritual things. Therefore, human intellect requires something else and consequently the patriarchal saints and prophets, who first gave science to the world, were illuminated from within and did not trust only the senses”.[26]

From this quotation, we may also deduce that Bacon was first of all a theologian, ingenious and scrupulous, often of independent thought, but he remains tied to his time, with the credulity and limitations of the age.

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[26] R. Bacon, *Opus majus*, edited by J.H. Bridges, Williams and Norgate, London 1900, Vol. 2, p. 169

Part V of the *Opus majus* (together with the *Opus minus*, which is a sort of introduction and with the *Opus tertium* which constitutes a supplement) is, among the great number of works, most directly of interest to physics. It is completely given over to optics, "flower of all philosophy, through which, but not without which, all other sciences may be understood".[27] The treatise is completely based of the works of Alhazen, with minor additions and some applications.

It must be acknowledged that Bacon, besides supporting the idea of the finite speed of light, also sustains that it is not an emanation of particles, but the propagation of motion.

Of course, it is an exaggeration to say that this vague intuition anticipates the light wave theory. There is an interesting passage in Part IV of the *Opus majus* in which, after recalling that spherical mirrors pointing at the Sun may cause combustion, he adds: "But combustion does not occur with all the rays striking the mirror, but only those striking the circumference of a single circle around the axis of the mirror [...] and those striking another circumference are reflected in another point, and for a third in a third point, and so on for the infinite circles that may be imagined around the axis of the mirror".[28] Bacon is therefore credited with the beginning of the study of catacaustics, a phenomenon that complicated and hindered progress in optics and was the subject of heated argument among 18th-century mathematicians.

2.10 Lenses and spectacles

The following passage from Bacon is also a great historical interest: "If one looks at letters or other small things using a crystal or glass or another clear material held over the letter, and the smaller convex part of the sphere is towards the eye, and the eye is above it, the letters will be seen much clearer and will appear larger [...] Consequently, this tool is useful for old people and those with weak eyesight, because they see the letters, however small, as sufficiently large".[29] He adds that the instrument is less useful if the crystal, instead of being limited by a shell of smaller emi-sphere, is limited by a larger one.

This is one of the first, if not the first, historical document in which a scientist talks of lenses, and we know that Bacon used them in a number of experiments and sent an example to Pope Clement IV, inviting him to try

[27] *Ibid.*, p. 3
[28] *Ibid.*, Vol. I, p. 115
[29] *Ibid.*, Vol. 2, p. 157

them. But even if we had no other clues, a first reading of Bacon's work is enough to realise that he is talking of something already known in his time.

So, who was the inventor?

In spite of numerous studies down the centuries, justified by the importance of these swollen discs of glass in the advance of physics, up to now it has not been possible to establish neither the time or place of their invention. It has only been possible to establish that there are two historical questions: the use of a magnifying lens and its application to correcting presbyopia.

Apart from some sporadic observations that can be dated to the Classical period, the magnifying glass, as the subject of scientific investigation, dates to the high Middle Age and Alhazen had already begun to study magnification produced by glass spheres, presenting it as an optical illusion. Spectacles (reading glasses) appeared later, but that cannot be the product of theory as it is impossible that medieval theory of sight could include the idea of correcting defects of eyesight. The discovery was therefore accidental and it is logical to assume that it occurred in glass-working: a glass-worker in Murano, for example, busy making the glass discs that, backed by lead, were used for the glass in the windows of the houses of the rich, may have made a lucky discovery.

This artisan origin is also supported by the popular term "lens" ("lente" in Italian) that derives from "lentil" (*lenticchia*) that only 16th-century scholars had the courage to ennoble by making the name in latin. Bacon, as we have seen, shuns the specific term and uses *instrumentum.* In the middle of the 16th century, Girolamo Cardano, another pompous and sometimes obscure Latin writer, calls the lens *orbem e vitro*, an expression that his French translator either did not know how to render in his language or did not understand and simply translated as *rotondité faite du verre*.[30]

In the three hundred years after Bacon, it is extremely difficult to find in scholarly works any mention to "reading glasses for old people", as biconvex lenses were called, or to "glasses for young people", biconcave lenses to correct myopia that certainly came after the former and were also discovered accidentally by glass-workers; or, at most, followed a simple reasoning that if biconvex lenses helped the elderly, biconcave lenses would have the opposite effect and help the eyesight of the young. It is certain that in the mid XIV century, reading glasses were already widespread: a document dating to the 1300s (of the corporation of Venetian artisans) mentions *roidi da ogli* (eye discs); a fresco from 1325 depicts a monk with

[30] G. Cardano, *De subtilitate*, Lugdini 1551, p. 181 (first edition 1530), French traslation by R. Le Blance, Paris 1556, c.89*v*.

glasses (Fig. 2.1); in his letters to posterity, Petrarch (1304-1374) informs us that after reaching sixty years of age, he needed to use reading glasses.

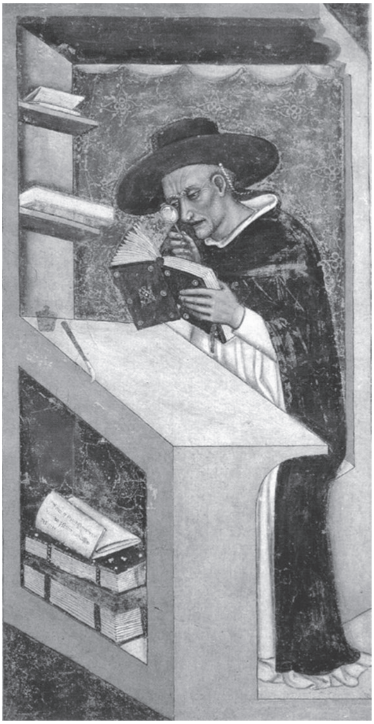

Fig. 2.1 - Detail of a 1325 fresco by Tommaso da Modena (Treviso, San Nicolò, *Capitolo* Room. The monk wearing glasses is Brother Ugo of Provence
Source: Alinari

MAGNETISM

2.11 The compass

Magnetism is the only branch of physics entirely founded in the Middle Ages. Classical antiquity kn ew only the minimum possible: the attraction of a piece of lodestone and a piece of iron, that, once the mineral was discovered, was impossible to ignore, even if one wanted to. On the other hand, the Greeks used great imagination in inventing popular theories and legends about magnetism, widespread in antiquity, that can be found in Medieval literature: we can quite happily ignore them.

Suddenly, in the XI century, a magnetic instrument appeared of the greatest importance: the sailor's compass. Where did it come from?

The question is still open. Throughout the XIX century, the almost unanimous theory was that the Chinese had known about magnetic polarity for twenty-seven centuries before the Christian era. But now, many historians argue that the first authenticated Chinese document to mention the directional properties of a magnetic needle date to 1100 A.D. and they attribute it to foreign sailors. All the same, it seems that Chinese divining arts included a tool constituted of a lodestone spoon that rotated to reveal the direction in which it stopped (Chinese spoons have a short handle and remain balanced when the convex part is placed on a horizontal plane): a bas-relief in the Museum of Zurich, dated 114, shows just such an instrument.

The first mention in the West of the use of a magnetic needle for navigational purposes is found in an 1180 work by the Englishman Alexander Neckam, who refers to it as already in use, and mentioned by various authors of the same period, for example Guyot de Provins and Toegues de Vitry. The primitive maritime use may have been introduced in the Mediterranean by the sailors of the Italian Maritime Republics (but some credit the Norwegians) who had traded actively with the East, as can be deduced from the fact that the word "calamita" (perhaps from "calamus" - rod) for a magnetic compass, passes from Italian to all Romance languages and the languages of the Mediterranean Slavic peoples.

The early primitive technology is described by an Arab scientist, Bayleck al-Qabajaqi (d. 1282) who, on a journey from Tripoli (Syria) to Alexandria in 1242, saw a compass being used by the ship's captain. The captain floated an iron needle inserted in a piece of cork in a vase full of water, he then positioned a magnet near the surface of the water and moved the water and the needle; then he removed the magnet and the (now magnetized) needle indicated North and South.

This rough technique was improved in the XIII and XIV centuries, at first using an instrument in which the floating needle, each time magnetised by induction, was replaced by a permanently magnetised mobile needle placed on the horizontal plane. Later, with an improvement that revolutionised nautical arts, the fixed wind rose was substituted by a mobile card (Fig. 2.2).

Both these refinements seem to have been made in Italy, because the instrument was known as the *bossola della calamita* (magnetic compass), later abbreviated to *bossolo, bussola* (from the Latin *buxia*, box-wood or wooden box) and the term passed from the Italian to Romance languages and Arab and Turkish sailing jargon.

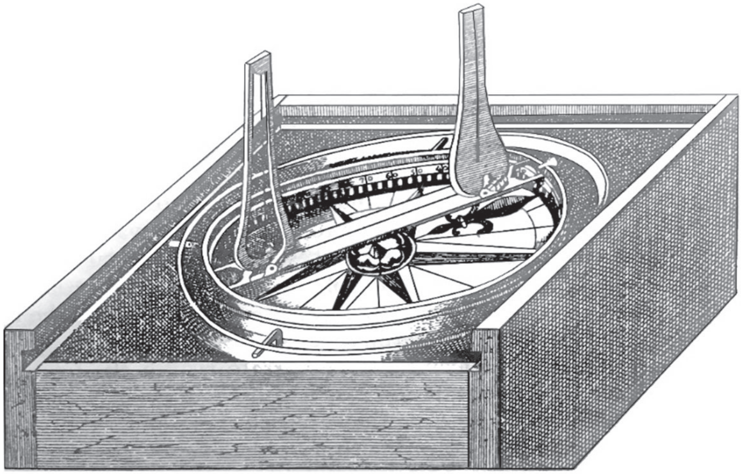

Fig. 2.2 - 17th-century mobile compass with mobile wind rose and gimbal. Sunset is indicated by a lily. *Source* R. Dudley, *Dell'arcano del mare*, Florence 1646

With regard to the advances made in this period, what we may claim with some certainty is that in 1380 the mobile compass card was in common use and believed of ancient origin, as Francesco da Buti, in his famous commentary to the *Divine Comedy* noted its first description in the verses "*Si mosse voce che l'ago a la stella/parer mi fece in volgermi al suo dove*" [A voice was heard, and I turned to it as a needle to its polar star] (*Paradiso,* XII, 29-30).

From the above brief description, it seems that certain Flavio Gioia, the inventor of the mobile compass card, never actually existed, notwithstanding a monument to him in Amalfi. This monument could be erected more truthfully in honour of the unknown inhabitant of Amalfi who, in the XII century, built the mobile compass card, because it is almost certainly true that the inventor was born in Italy and probably in Amalfi.

Gimbal suspension, known in Classical antiquity and the High Middle Ages, made up of a twin suspension that allowed the needle to remain more or less horizontal despite the rolling and pitching of the ship, was described in more recent times by Cardano, who does not claim to be the inventor but attributes it to a certain Iannello Turriano of Cremona who built it for an emperor's chair[31]. Suspension entered maritime use only in the first half of

[31] Cardano, *De subtilitate*, op. cit, p. 532

the XVI century (Christopher Columbus employed it), although three drawings by Leonardo da Vinci show its application to the compass (Fig. 2.3).

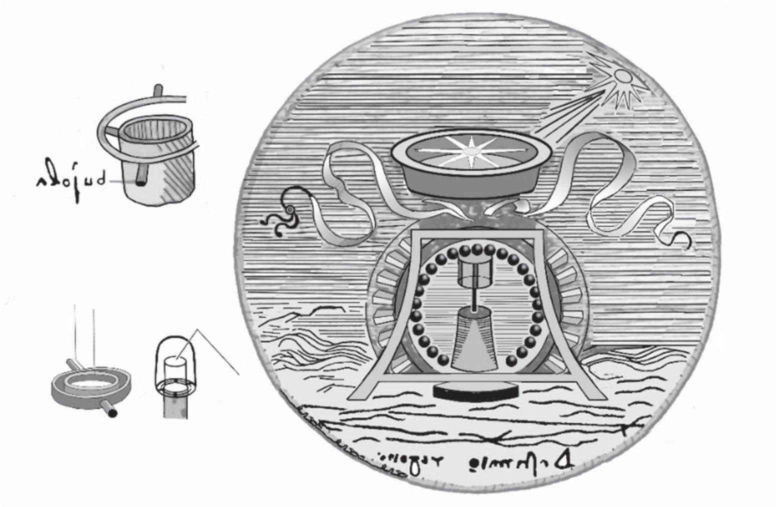

Fig. 2.3 - Gimbal suspension (left) and compass (right) in designs by Leonardo da Vinci

2.12 Pietro Peregrino

No less surprising than the compass sudden appearance is that of the first treatise on magnetism; according to surviving documents, this treatise is not preceded by separate observations, single experiments or partial attempts at connections. Although the author, Pietro Peregrino da Maricourt, proves to be an ingenious and expert experimenter, it is still difficult to claim that the entire treatise is all his own work.

We know little of Peregrino, if that is his real name (from "pilgrim" because of his frequent travels). He came from Picardy, was a contemporary of Bacon, who praised him and called him "a master of experiences"; he was part of Charles of Anjou's troops in the siege of Lucera (Foggia), during which he wrote *De Magnete* as a letter, dated 8th August 1269, to a Picard knight, Sigerus or Sigerius. That is about all we know of Peregrino.

The purpose of the treatise, then a manuscript and later printed in 1558, is to describe a perpetual motion engine. The aim should not make us laugh: it was until Sadi Carnot (1824) that science, after many centuries, admitted the impossibility of perpetual motion, even if it was claimed by Cardano and Simon Stevin in special cases. In the Middle Ages, the question of perpetual motion was scientifically legitimate; the efforts of hundreds of scientists (and impostors, who still exist today) were not in vain, because it was precisely their failure that produced the attitude that made Carnot's work a foundation of science. The substantial content of the principle of the impossibility of perpetual motion is simply historical: the ascertainment that it has never been achieved; it is also true that early rigidity was immediately weakened in the 20th century with the study of Brownian motion.

But to return to Peregrino's treatise, it is divided into three parts. The real scientific treatment begins in the third chapter of the first part, in which the four distinctive attributes of a good magnet are explained: colour, weight, capacity to attract, compact texture without bubbles. They are all characteristics (apart from specific weight) still deemed good indications of a magnet's quality.

The next chapter describes three experimental approaches to discover the magnet's polarity. Here it is important to note one element, that will be very important in the further study of magnetism: Peregrino does not use prismatic magnets but spherical ones, resulting in much more laborious experimental proofs, that Peregrino overcomes in a brilliant way. Having defined the polarity, Peregrino explains how to differentiate the north pole from the south, and observes the repulsion of similar poles, to magnetise the iron on contact; last, he describes the phenomenon of magnetic induction and split magnet in the same way schools of physics do today.

From this very tidy experimental treatment, Peregrino moves on in the ninth chapter, just as if he were a modern writer, to theoretical speculation, asking what is the cause of magnetic action. Refuting the theory of his age that attributed the direction of the needle to the huge lodestone mines in the northern regions of the Earth, Peregrino believed that the heavens influenced a magnet in such a way that each point in the sky induces a similar point in the magnetic sphere, as long as this *in se gerit similitudenem coeli;* it follows that a spherical magnet balanced along its polar axis will rotate along that axis according to the motion of the skies. This theory hints, in our opinion, at the astrological and magical origin of the study of magnetism.

The second part of the treatise deals with the technical applications of magnetic properties, describing a primitive magnetic graphometer that makes it possible to determine the angle of the Sun's azimuth or of a star on the horizon, and the pivoted compass mentioned before.

TECHNICS

2.13 Influence of Renaissance technics on the rebirth of physics

In terms of science, the vast and deep renewal known as the Renaissance, which we deal with in the next chapter, undoubtedly had its foundations and impulse from the renewed contact with the ancient world provided by the translation of Classical works, critiques of the schools, the cultural diffusion by the universities, and a literary rebirth.

But during the Middle Ages there was a growth of another element that gave rise to a renewal of physics in particular: the gradual extension and refinement of technics, that in part changed social conditions and human thinking and in part posed new scientific problems.

Our history seems closely tied to a phenomenon that appeared around the year 1000 and continued up to the XVI century, that historians call the second industrial revolution, a brief description of which is given hereafter.

The technical renaissance began in Italy with a collective instinct for defence and self- preservation against the Hungarian and Saracen invasions: villages become larger to accommodate the flow of the rural population seeking safety and freedom within the defensive walls; the price of land near the villages rose, a first sign of the capitalist organisation of society. Inside the walls of these primitive villages, that were becoming towns, Medieval craftsmanship was born, a fertile and ingenious activity in which living seems to be mixed with work and work acquires a new nobility, unknown in ancient times.

Already by the X century, agricultural re-awakening took a great step forward with the shoeing of draught animals, that allowed horses to be used in farm work and on rocky ground. In the XI century, the neck harness for horses and oxen was replaced by the shoulder harness that, no longer risking the animals being strangled, quadrupled the haulage power. It was only in this century that the concept of teaming draught animals was understood, resulting in a concentration of energy never seen before. This concentration of energy led to the introduction of a new type of plough, mounted on wheels and heavier than before with a share better adapted to go deeper into the earth and break up the clods. Land transport was improved not only by the introduction of greater animal power, but also through the improvement of roads: the heavy and expensive Roman paving, using large polygonal stone slabs, was replaced by a more flexible network of roads built of cobbles or flint.

This greater source of energy available in the countryside was reflected in new sources of power for artisan and industrial use. Around the XI century, the water mill, already used by the Alexandrians in the I century B.C., rapidly spread across the Western world in a variety of forms (powered by tides in Venice, floating in river areas, etc.) adapted to the environmental conditions. At the same time, the windmill became common, introduced in the Arab world and arriving in Europe through Morocco and Spain. Water and wind mills, even in their early forms of the XI-XII centuries were capable of producing 40-60 horsepower and governed all technology up to the XVIII century and conditioned the use of machinery.

This new source of energy gave rise, in the first decades of the XIII century, to a strong development in metallurgy. In ancient ovens, air was blown in through bellows moved by human force that did not create a high enough temperature to melt iron (over 1500 °C). In the XIII century, bellows were powered hydraulically creating very high temperatures, able to obtain flows of cast iron through an arrangement of alternating wood and ferrous material; in the XVI century, smelting furnaces were 6 metres high and cast iron was used to build a variety of things (cannons, bullets, ovens, pipes, cooking pots, metal sheets).

The flourishing new life was reflected in all working activities: the introduction of silk-working (around 1130 in Sicily), progress in spinning with the introduction of new machines, fulling-mills and looms, that from the end of the XIII century replaced spindles and distaffs; advances in distillation that led to the production of alcohol (Salerno, c. 1100), nitric acid (c. 1160), and shortly after sulphuric acid and then hydrochloric acid (XV century); the rebirth of glass working beginning in the X century with the invention of enamel tinted glass that, through a continuous process of learning and perfection gave rise to the masterpieces of Murano in the XV century; the invention of movable printing, whose first paper fragment dates to 1445; in the new architecture, turning its back on the monolithic Roman constructions in favour of lighter constructions - Romanesque, Gothic - that raised new problems of the science of statics; the use of modern firearms, that in turn created new questions of dynamics; in the grandiose hydraulic enterprises, begun in Holland to drain flooded land ("polders") using a variety of pumps; mining; sailing, with an increasing tonnage of ships and the advances in sails, the introduction of pilot's charts (XIII century), the compass and the invention of the vertical rudder (XII century) that meant the end of coastal navigation and navigating the open seas.

While the School shut itself into a lifeless contemplation of the world, navigators and sailors, architects and engineers, glass-makers, weavers, foundry workers, miners and artisans of all types took possession of nature's bounties and improved human life. Throughout the Middle Ages, alongside the learned movement, shut up in book learning, there was a parallel development of technology with a different view of the world, capable of creating a new concept of culture. When, in the Renaissance, the two currents met, influenced each other, and finally merged, the new science was born, with its new ideal of man that was not the lazy scholar or the ignorant empiricist, nor the man "*sine artificio sciens aut ignarus artifex*" as Giovan Battista Porta puts it in his first book on natural miracles (Fig. 2.4)[32], but the man who acts to know and used knowledge to act.

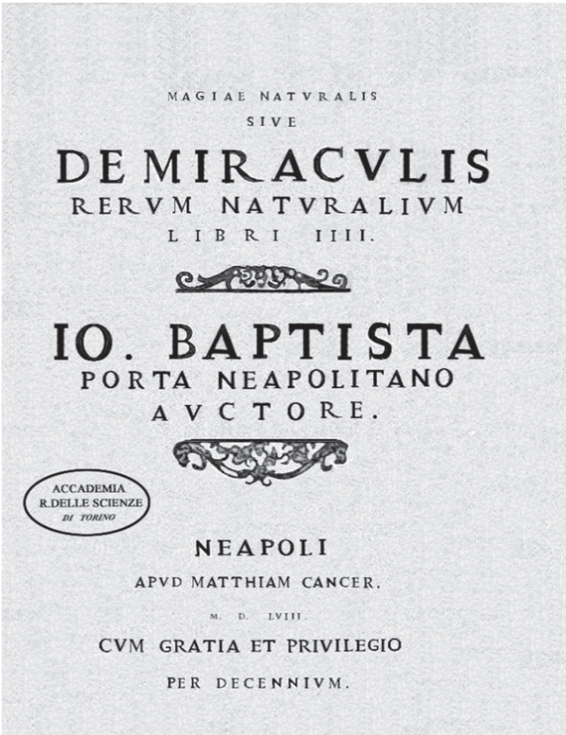

Fig.2.4 - Frontispiece of the first edition of Giovan Battista Porta's *Magia* (Naples 1558). The work is so rare that its very existence was in doubt. The example reproducing the fronti-spiece is owned by the library of the Turin Academy of Science.

[32] G.B. Porta, *Magiae naturalis sive de miraculis rerum naturalium*, Naples 1558, Book I, Chapt,2

The beneficial influence of the grafting of the technics on the aged trunk of science was completely understood by the promoters of the new science, so much so that the greatest of them all, Galileo Galilei, begins his masterwork, the work he took the most time and patience to produce, with a refined quotation from Filippo Salviati that cites the fervour of the Venetian arsenal: "*It appears to me that frequenting the famous Venice Arsenal, Your Lordships, offers much for reflection, for one concentrating on pure knowledge and rational thinking, particularly with reference to mechanics; as every type of machine is constructed with great ingenuity, thanks both the past experience and experience gained in the day-to-day constructions of these machines, we are sure that we will encounter well-prepared and able minds with whom to hold theoretical and refined discussions*". To which Giovan Francesco Sagredo replied. "*Sir, You are not mistaken; and I, being of a curious nature, frequently visit this place and study the works of those that we hold to be pre-eminent in their field and pray that they be protected; many meetings with them have often assisted me in the investigation of the reason for the marvellous but recondite effects that are still almost inconceivable*".[33]

[33] G. Galilei, *Discorsi e dimostrazioni matematiche intorno a due nuove scienze attinenti alla meccanica e i movimenti locali*, in Works, op. cit., Vol. 8, p. 49

3. The Renaissance

LEONARDO DA VINCI

3.1 Leonardo the inventor

The work of Leonardo da Vinci followed in the path of the technical tradition, untainted by the schools or blocked by the principle of authority, and which mark the beginning of the scientific renaissance.

Leonardo was born in Vinci on 15 April 1452 to Ser Piero and a certain Catherine; his early education was at home but in 1469 he moved to Florence where he was apprenticed under Andrea di Cione, called Verrocchio (1435-1488) to learn painting, sculpture and architecture. He soon became famous and in 1482 he moved to Milan and the court of Ludovico Sforza "il Moro" (the Moor). He remained there until January 1500 when, with the French occupation of the Milan, he moved back to Florence by way of Venice. He returned to Milan in 1506. In 1513 he moved to Rome which he left in 1516 to follow Francis I to France. He settled in Amboise, the favourite seat of the French king, where he died on 2 May 1519.

An intense, troubled and restless life that took him to Lombardy, Lazio, and France and gave him a variety of experience. Extremely curious by nature, and an acute, almost unbelievable, observer, no science of his time left him indifferent: from mathematics to physics, from anatomy to physiology, to biology, botany and geology.

He was of humble origins that did not allow him, in his early difficult youth, to study Latin texts or engage in long, boring and vague discussions of the writings of Aristotle, but rather led him a direct observation of nature, to test it and imitate it. And even when, later, with the help of his friend the mathematician Luca Pacioli (c. 1445-1517), he came to know Aristotle, Euclid and Archimedes and, driven by an insatiable curiosity to know more, the works of the Medieval scientists, which we will deal with later, he was never overwhelmed by tradition but always tried to reinvigorate ancient science through new experience, because experience was always his primary source of information: "Although, like them," he wrote, "without being able to quote the authors, it is far better to add experience to reading; experience is the teacher of these teachers. They are deflated and shown to be pompous, dressed and decorated by the labours of others, not theirs; and they do not recognize my labours; and if they scorn me as an inventor, they are no greater, not inventors but trumpeters and actors of other works and should be censured".[34]

Leonardo is THE inventor, and maybe Franz Feldhaus was right to call him the greatest engineer in history. But the depth of his thinking and thirst for universality drove him to move onward from mere technique to generalisation, immediate utility, typical of technics in every age, to deferred utility, peculiar to the science. Historians of technics have listed hundreds of Da Vinci's inventions, described in his detailed drawings, sometimes accompanied by brief captions, but often with no comments, as is if the urgency of his inventiveness stopped him from making verbal comments. Some drawings are frequently repeated, with modifications, often added years later, testimony to the serious commitment of a builder and not the caprice of an artist. We will cite only Da Vinci's most famous inventions: mechanisms to change or transmit movement, like the nuclear and cylindrical steel chains used today in bicycles; simple or complex belt transmissions; various types of gears (conical, spiral, stepped); anti-friction rollers; double joints now termed universal, or "cardanic", used in automobiles; machinery, such as precision instruments for the automatic production of files and percussion tools to produce gold ingots; instruments, once attribute to Cellini to making coinage more precise; a bench to test attrition; suspension of the axes on highly mobile wheels to decrease rotational attrition; a system rediscovered by George Atwood in the late XVIII century that led to modern ball-bearings and rollers; instruments to test the resistance of metal towing lines; numerous spinning machines, like the shearer, the twister, the carder; a mechanical loom and a wool spinner; instruments of war (the "beastly madness", as he defined it); a number of ingenious musical instruments.

3.2 Hydraulics and hydrostatics

Leonardo was a master of the ancient science of hydraulics. He contributed to the reclaiming of the Lomellina and the water systems in the Novara area. He studied the draining of the Pontine marshes and designed

[34] Leonardo da Vinci, *Codice atlantico* (Codex Atlanticus), edited by the Regia Accademia dei Lincei, Rome 1900-04, f. 117 *r.v*. The famous Codex (called Atlantic because of the abundance and variety of subjects) consists of 8 volumes; four written and four in drawings, with a diplomatic (literal) transcription. Students of Leonardo refer to it as *Cod. Atl.* f. 1171 *r.v*, where the letter after the *r* and *v* helps to find the passage, that is not always easy to do.

the deviation of the Arno above Pisa. He studied the regularisation of the Adda and the Martesana canal. Here also, he invented a number of machines: dredgers using chains, buckets, baskets, that are still in use today; he thought up machines to dig canals to make then navigable, and perfected basins. In basins, already used in his time, he replaced the early imperfect and easily broken sluice-gates with a double hinged gate that exploited water pressure for a perfect closing, and introduced the system of partitions that regulated the opening of the gates for the filling and emptying of the basins.

Passing from practical hydraulics to theoretical hydraulics, Leonardo discovered the principle of communicating vessels with liquids of different densities and the fundamental principle of hydrostatics, now known as "Pascal's principle", that, according to Duhem, reached the French philosopher from Leonardo through Giovan Battista Benedetti and Marin Mersenne. We owe Leonardo the wave theory of the sea; actually, extending this theory to the most universal concept of physics that is attributed to him ("motion is the root of all life"), he saw, anticipating history, wave motion as the most common natural motion. For Leonardo, light, sound, colour, smell, magnetism and even thoughts propagate in waves.

3.3 Human flight

The most amazing discovery of Leonardo the inventor was without doubt human flight. He studied and described, with miraculous precision, bird flight and realised that the compression of air under the wings produced the force that we now call supportive; he studied the anatomy of the organs of flight, air resistance and the dynamic importance of the centre of gravity.

He set himself a study plan: "In order to talk of this matter, one must first define the nature of air resistance; then the anatomy of birds and their feathers; thirdly, understand the functions of the feathers in the various movements; and last the contribution of wings and tails".[35]

This conscious approach to scientific research is Leonardo's greatest merit; studies are completely different to the attempts at human flight that, in legend and reality, predate him. We need only mention Giovan Battista Danti who, it is said, flew across the lake of Perugia in the early 15th century, or the construction of flying birds attributed to Giovanni Regiomontano (pseudonym of Johannes Muller), the new Archita, something that Giorgio Vasari attributes also to Leonardo who, during his country walks, amused

[35] Ms. F.f. 41*v*. For this type of reference, see above, Note 6.

himself by making special little wax birds which he then filled with air and threw up in the air.

After long and careful study of bird flight, in his first stay in Milan (1490) Leonardo designed and perhaps built his first machine fitted with bat wings with which, using the force of arms and legs, a man would be able to fly. We know now that the problem could not be solved in this way as a man cannot produce sufficient energy to fly.

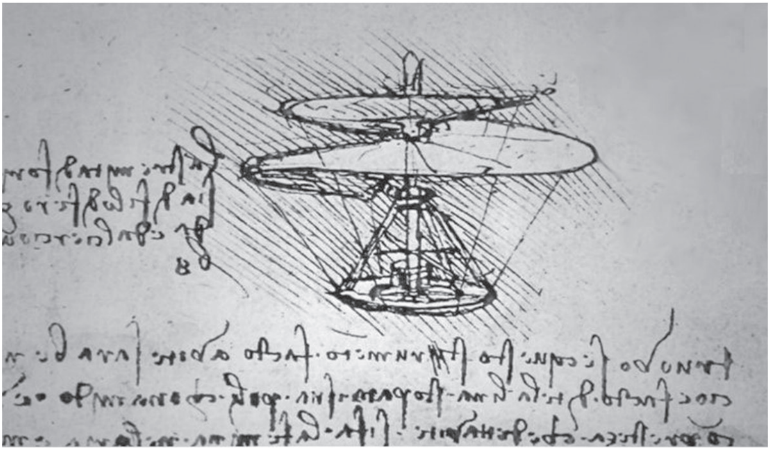

Fig. 3.1 - Design for a helicopter drawn by Leonardo da Vinci (ms. B)

Whether or not he understood this, after fifteen years Leonardo started studying flight again, while he was in Fiesole, and though wind-assisted flight (gliding), rightly observing that this would require less effort for sustaining and progressing. In Manuscript K, he modified his original plans and set out the arguments of the four treatises on flight, one of the many scientific treatises planned and never written: "Divide the treatise on birds into four books; the first on their flight and wing beats; the second on flying without beating wings using the wind; the third on flight similar to birds - bats, fish, animals, insects; the last assisted flight".[36]

The *Codice Atlantico* includes the oldest known design of a parachute, annotated: "If a man has canopy 12 arms wide and 12 arms high, he may launch himself from any height without danger".[37] Manuscript B contains the design of a helicopter (Fig. 3.1) whose main propulsion comes from the

[36] Ms. K, f. 3*r*.
[37] Ms. B, f. 83 *v*.

propeller, “a screwed instrument that when used with speed uses the air as female screw and rises into the air”.[38] After such a lot of work, it is an act of faith, more than a prophesy contained in the famous hendecasyllable: “The great bird will take its first flight from the back of Monte Ceceri, filling the Universe with wonder, filling all writings with his fame, and bestowing eternal glory to the nest where he was born”.[39]

Leonardo probably never tested his “big bird”, but it is likely that these researches into flight, that he persevered with for 25 years, from 1489 to 1513, more than any other accidental reason, gave rise to his fame among his contemporaries as a magician or a lunatic. We should remember that, despite four hundred years of constant progress, the first airmen at the end of the 19th century were laughed at or ridiculed for their madness.

3.4 Centres of gravity

As we said at the beginning, a great engineer always passes from the particular to the general, from the concrete to the abstract, from the contingent to the permanent: in a word, from the technique to the science. As it was with Archimedes, so it will be with Sadi Carnot. The studies of mechanics and prospective led Leonardo to questions of geometry (algebra, which began to flourish in his time, was almost unknown to him) and mechanics.

The most lasting and perhaps conspicuous result was the study of the centres of gravity of planes and solids, already begun by another two great engineers, Archimedes and Hero, whom Leonardo knew through the works of Albert of Saxony and the Scholastics. Just as Archimedes had found the centre of gravity of a triangle, Leonardo discovered the centre of gravity of a tetrahedron (and therefore of any pyramid); moreover, he added another elegant theorem to this discovery: the conjunctions of the vertices of a tetrahedron with the centres of gravity of the opposite faces pass through the same point, the centre of gravity of the tetrahedron, that divides each conjunction into two parts, the one towards the top being three times the other. This is the first contribution that modern science adds to the barycentric studies of Archimedes.

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[38] Ms. B, f 83 *v*.

[39] Leonardo da Vinci, *Codex on the Flight of Birds and other subjects*”, published by T. Sabachnikoff, transcript and notes by G. Piumati, French translation by C. Ravaisson-Mollien, Rouveyre, Paris 1893, first cover page.

3.5 Statics

But with centres of gravity, we are at the borders between mathematics, practical mechanics and theoretical mechanics, handed down from Aristotle, Archimedes, Hero and throughout the Middles Ages explained by Arab and Western commentators who analysed the subject, criticised it, modified it and widened it. Leonardo certainly had read a number of books on mechanics, as can be seen from his infrequent quotations and the more numerous unaccredited transcriptions and annotations. Besides the works of Aristotle, Archimedes and Hero, he knew those by, or attributed to, Euclid, the works of Thabit ben Qurra (826-901), the mysterious Giordano Nemorario (Jordanus Nemorarius), Biagio Pelacani, Leon Battista Alberti (1404-1472), and Nicola Cusano. He came into direct or indirect contact with the kinetic and dynamic theories of the Oxford School, and especially the Paris School.

From these sources, Leonardo learned the science of mechanics of his age, he assimilated it, applied it with increasing accuracy, and went beyond it. He surpassed Nemorario and Pelacani, expanding the concept of the "moment" of a force with respect to a point, discovering in two particular cases the theory of the composition of moments and applying it, with rare ability, to solve problems on the composition and decomposition of the forces; answers that had been sought in vain for centuries and would only be fully resolved a hundred years or so later by Stevin and Galileo. From Nemorario and perhaps also, as Duhem suggests, Albert of Saxony, he learned the conditions of balance of a body resting on an inclined plane, but he went further than them discovering, perhaps from a reflection on Italy's leaning towers (Pisa, Bologna), the theorem that we now call the support polygon: a body resting on a horizontal plane is balanced if the foot of the vertical conduit through its barycentre lies within the supporting base. He could not demonstrate it, but he justifies it with admirable good sense. And by applying the scientific results to the technical aspects, Leonardo was the first to attempt a theory of the arch ("a strength resulting from two weaknesses; as arches in buildings make up 2 quarters of the circle, each of these is very weak and would collapse but, opposing to the ruin each of the other, the two weaknesses are converted in a single strength").[40] He was the first to study resistance to pressure and the flection of beams; the first to study attrition and note its influence on equilibrium.

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[40] Ms. A, f. 50 *r*

3.6 Dynamics

Leonardo's contributions to dynamics are more doubtful. From the numerous notes on dynamics in the manuscripts, with the usual disorder of all his other reflections, it is doubtful that, as is often claimed, he glimpsed the principle of inertia. An often quoted, and sometimes changing the last phrase, comment is "Every motion is sustained by itself, that is, each moved body always moves according to the impulse that it is given".[41] The first two phrases, if complete, would express, with the typical incisiveness of Da Vinci's prose (that in this case recall the precision of a Cartesian phrase in Latin: "*quod in vacuo movetur, semper moveri*"), the principle of inertia. But it should be recognised that the last phrase, an integral part of the idea, greatly reduces the generality of the preceding affirmation and seems to lead Leonardo's ideas to Buridano's theory of impetus. Another brief note, only recently published, conforms this conclusion: "No inanimate body accidentally will maintain its motion".[42] Basically, da Vinci's dynamics is Aristotelian, although influenced by the theory of impetus. The relation between force and motion and the consequent proportionality between weight and velocity of the bodies fall is particularly Aristotelian (although in this case there is a claim for independence). Fundamentally Aristotelian, too, is the denial of perpetual motion, which in his youth he admitted: "Against perpetual motion - No inanimate thing moves by itself, it therefore moves by unequal forces, that is of differing time or movement, or unequal weight, and when the force of the first motor ceases, so does the second".[43]

On the contrary, there is no reason to doubt that Leonardo realised the principle of action and reaction in some particular cases, without rising to the general declarations we find in Newton. Proof can be found in a small number of citations from the *Codice Atlantico*, but they are not the only ones: "With regard to the motion of water: moving an oar in still water is the same as moving water against an immobile oar".[44] "The force of an object against air is the same as the air against the object".[45] "The movement of air against an immobile object is the same as the motion of a moving object against immobile air".[46]

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[41] Leonardo da Vinci, *Codex on the Flight of Birds*, op. cit., p. 106 (ms. F, *r*)
[42] I *manoscritti e i disegni di Leonardo da Vinci,* Vol. 7, *Disegni di varia epoca*, edited by A. Venturi. Libreria dello Stato, Rome, 1951, tab. 311
[43] Ms. A, f. 22*v*
[44] *Cod. Atl*. f. 175 *r.c*.
[45] *Ibid.*, f. 381 *v.a.*
[46] *Ibid.*, f. 395 *r.b.*

The preceding suppositions, especially the last, demonstrate that, together with the principle of action and reaction, Leonardo realised also the relativity of motion. But there are some acute critics who interpret these passages as simple propositions of dynamic reciprocation.

A better idea of his need for research into dynamics can be found, if we follow him through the numerous attempts to clarify and define the concept of force, experimenting, classifying, and using analogies, that led him, finally, to burst out in a famous passage of robust Italian prose, often mutilated, without explicable reason by compilers of anthologies: “I say force is a spiritual virtue, an invisible power, that through accidental external violence is caused by motion and located and infused in the objects that are by natural use retracted and bent giving active life of marvellous power; forcing all created things to change form and place, it rushes headlong to the desired death and changes according to its causes. Slowness makes it great and speed makes it weak; it is born out of violence and dies for freedom, and the greater it is, the sooner it is spent. It crushes with fury anything that stands against it; it desires victory, to kill its cause, what contrasts it and, winning, kills itself; it is stronger when it encounters greater contrast. All things flee from their death, but being forced, everything compels. Nothing moves without it. The body where it arises increases neither in weight nor in form”.[47]

Leonardo’s dynamics are enriched by some particular intuitions that anticipate much later discoveries: he observes, for example, and it is impossible to understand how he could make such a difficult experimental observation, that the movement of weights along an arc of circle is faster than along the underlying string; he hypothesized (influenced by Cusano?) the daily movement of the Earth, maybe observing or maybe guessing the deviation of a falling weight from the vertical, that only in 1640 would Father Vincenzo Ranieri (1606-1648) prove from the Tower of Pisa.

3.7 Pneumatics, optics and acoustics

Leonardo’s long and diligent experience of balances led him to discover the weight of air - when traditional learning dating back to Simplicius taught that air was weightless - but also the variation in atmospheric pressure that brought him to build a type of balanced barometer, or, as others believe, a hygrometer “to understand the quality and weight of air and when it is going to rain”.

[47] Ms. A, f. 34 *v*.

A master painter, he was intensely interested in the science of optics. Around 1490, he studies the optics of Vitellione and gave the first description of the *camera oscura*, already invented by the Arabs and was the first to apply it to the theory of vision. He suggested making glasses "to see the moon larger", and perhaps built parabolic mirrors. Using this instrument, as we have seen (§ 2.7), he solved the difficult problem set by al-Haytham (Alhazen). He discovered the phenomenon of the persistence of images and noted that each eye sees a different image of the object observed. He listed photometric properties and raised questions. He was the first to affirm that the pale light of the Moon - that Galileo called "lunar whiteness" - is the light coming from the Earth and reflected by the Moon. He attempted to interpret, through accurate observation supported by experimentation, the blue of the sky and the blue of a thin layer of smoke viewed in a dark background.

It is an easy step from optics to acoustics because of the many analogies between the two phenomena. Leonardo made many observations on acoustics: he understood the law of the reflection of sound and the consequent phenomenon of an echo; he knew that sound travels over time and wanted to exploit that to calculate the distance of a thunderclap; he experimented with resonance, using a balance to measure the vibrations of a nearby chord "similar to a sonata".[48] He recognised that the ripples produced by throwing a stone into still water are transverse and the two systems of ripples produced "on a still watery surface" intersect without breaking[49]: for Leonardo, sound (and light) waves are transverse like water waves.

3.8 The Method

It is usual to claim Leonardo as the inventor of the experimental approach. This search for the thaumaturgic founder of the experimental method seems to us a little simplistic. It appears clear, also from what we have seen before, that recourse to experience is as old as physics and continued throughout the Middle Ages. Many learned Medieval teachers did not used experimentation not because they disdained it but because they believed superfluous, as Aristotle had made all the experiments possible. The awareness of the experimental approach (distinct from recourse to experimentation) was formed slowly with the gradual liberation from the principle of authority and the consequent confluence of the tradition of the learned and artisan practices.

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[48] *Ms. A, f. 22 v.*
[49] *Ibid., f. 61 r.*

Leonardo occupies a prominent place among the men who accelerated this process of synthesis. He claimed to be, with self-satisfied exaggeration, “an unlettered man”, that is free for prejudice, closer to nature. He had a high opinion of experience and supported its universal value - “knowledge is the child of experience” - and he used it widely, convinced that “all our understanding derives from the senses”, therefore it is necessary to “limit reason to experience”, and not extend it beyond experience. But experience *per se* is a crude datum and it is the job of reason to place it in the concept of the world and demonstrate why “that experience is forced to operate in that way”.[50] The observation in the *Codex on bird flight* - “a bird is an instrument that works by the laws of mathematics” - has a universal character, in the sense that for Leonardo - and this concept of the experimental approach links him to Galileo - all nature is woven into the laws of mathematics, therefore “no human investigation may be considered true science, if it does not go through mathematical demonstrations, and there no certainty if mathematical science cannot be applied”.[51] With this concept, experience *per se* never fails, “only our judgement fails, finding in the experiments effects that these did not cause”, and therefore “men are wrong to complain about the experiments and, with bitter reproaches, they accuse the experiment to be a failure. But do not accuse experience and turn your complaints on your ignorance that makes you blind and leads you, driven by vain and unrealised desires, to lament things that are beyond its capacity, saying that the experience is false”.

The above comments and quotations are sufficient to demonstrate that the fundamental philosophical idea guiding scientific research from the XVII onwards lies with Leonardo: nature can be understood through mathematics. Not even Duhem, with his mania to find precursors for everything and everybody, could find precursors of the Leonardo’s concept. Who inspired Leonardo? May the fervent and fertile times in which he lived, with artisans, engineers, architects, mariners and tradesmen, get used to measures and calculation. Sadly, Leonardo was not a scientist-philosopher and his intuition about the fundamental position of mathematics in the study of the physical world did not lead to an organic work and did not impede the not infrequent contradictions (the near impossibility of dating the writings impedes knowing to what extent they constitute an evolution of thinking), nor do they change the personality of a genius curious about nature.

[50] M*s. E, f. 55 r.*
[51] M*s. G*, f. 96 *v*

THE 16TH CENTURY

3.9 The cultural environment

Leonardo's influence on the development of science is the subject of much discussion. Some refute it on the basis that the manuscripts of Leonardo were buried and forgotten until publication in the famous pamphlet by Giovan Battista Venturi;[52] others, especially Duhem, support the diffusion of Leonardo's thinking in the Italian scientific world up to Galileo, through the oral tradition and direct consultation of the manuscripts.

Without entering into the argument, almost entirely inductive, we shall only say that objectively some of Leonardo's ideas can be found in three of Italy's greatest scientists of the 16th century: Nicolò Tartaglia, Girolamo Cardano, Giovan Battista Benedetti.

But before dealing with the work of these and other scientists, it should be remembered that the 16th century was a century of intense intellectual activity. Physicists and mathematicians read the works of the great Greek scientists, especially Archimedes, through translations, often commentated, by Tartaglia, Federico Commandino (1509-1575), Guidobaldo Del Monte (1543-1607), and Francesco Maurolico (1494-1575). The novelty of the scientific ideas forced translators into a praiseworthy philological effort to create a new terminology, that is at the foundation of our modern scientific language. Italian mathematics underwent its most flourishing and splendid period and the biological sciences were exceptionally fertile. And, if we look at broader horizons, we must remember that this was the century of the Reformation, the fight against the authority of the Church. It is the century of the Copernican revolution that had profound repercussions on the scientific mentality. It was a century of a renewal in philosophy, that through Bernardino Telesio of Cosenza (1509-1588) found the first firm opposer of Aristotle and in Giordano Bruno of Nola its first martyr. It was the century of the great geographical discoveries. Last, it was the century of the rebirth of occult sciences, as we shall see the next paragraph.

Progress in physics in the 16th century, taken as they stand, appear detached, almost accidental and of little importance, but when placed in the wider scientific environment from which they arose, they acquire a special relevance as the first conquests of a culture that was breaking the shackles of tradition and throwing off the centuries-old yoke of the principle of authority.

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[52] G.B. Venturi, *Essai sur les ouvrages physico-mathématiques de Leonard de Vinci avec des fragments tirés de ses manuscrits apporté d'Italie*, Paris 1797.

3.10 Occult sciences

An albeit brief mention to the occultism seems necessary to an understanding of the passage from Medieval to modern science, whose contribution, with the particular characteristics of the XVI century, is greater than we would like to admit.

The occult sciences, in their fundamental divisions (astrology, alchemy, magic) were a vibrant part of 16th-century culture and part of the cultural background of the time, taught in illustrious universities and ennobled by a centuries-old tradition that, reinvigorated by the animist and vitalist philosophy of the time, reached their greatest splendour with Paracelsus (Philipp Theophrast Bombast von Hohenheim). Astrological, alchemical and magical practices enriched the culture framework of observations, ideas, and technologies that, cleansed of the occult, became scientific knowledge in the following centuries. If examples are required, it is sufficient to remember the clearly astrological origin of magnetism; the supposed magical influence of the Moon on tides later proposed by Newton; the practices in alchemy of distillation, sublimation, crystallisation, numerous metallurgical, pharmaceutical, and tinting processes, as well as the discoveries through alchemy of alcohol and mineral acids (sulphuric, nitric, hydrochloric).

It is not therefore through eccentricity or weakness or submission to the mores of the time, as is often said, that mathematicians and astronomers such as Regiomontano, Paolo Da Pozzo Toscanelli or Luca Gaurico were also astrologers, or that philosophers like Bruno and Tommaso Campanella or technicians like Giorgio Agricola believed in magic, or that scientists like Cardano and Johann Rudolf Glauber were alchemists.

During the XVI century, “good” magic, as Bacon called it, confused in previous centuries with “demonic” magic and bitterly opposed by philosophers and theologians, postulated two ideas consonant with the vitalistic conceptions of the time: there exists between all bodies a universal connection that is manifested as sympathy or attraction between similar things or antipathy or repulsion among dissimilar things (*magia analogica*); when two bodies meet, forces and qualities are exchanged (*magia contagiosa)* giving rise to beliefs that today seem extravagant, but for the maguses of the time had no need of further empirical proof. For example: the cock is the enemy of serpents, then cock’s broth is an antidote to snake poison; the dog is man’s best friend and by applying it to the sick part of the human body will cure the sickness; crystal is similar to water, therefore with high fevers, holding one in the mouth will reduce thirst; the wolf terrorises sheep and therefore banging a drum made of wolf skin will frighten the sheep; and so on. This capacity of perception and reaction was accompanied by "magic" or "magnetic" power that Paracelsus identified in the power of the imagination, capable of altering the course of events or directly influencing nature.

The conception of magic, a central element to Renaissance culture, fed the aspiration to a cosmic religion and was a support in the fight against Aristotle, the champion of immutable things beyond the control of man. In the course of the century, "magic power" drew increasingly away from the horizons of magic, to be replaced by unspecified occult "virtues" emanating from the stars, metals and words. Magic was celebrated above all for its practical nature, for the possibilities it offered men to act on and dominate the forces of nature. In this sense, it has always been better defined as "natural magic", preconceived by Arnaldo di Villanova, Bacon, Alberto Magnus, as distinct from "black magic", vigorously opposed by the maguses of nature as cheating and lying. Freed from the demonic, the world of magic was secularised.

The true magus works with nature, not against it; he collaborates with it and does not violate it. In this sense, Cornelius Agrippa, one of the greatest writers about magic, is most explicit: "Those that place [magical works] above nature or against nature fool themselves, because they come from nature and are done according to nature"[53]; Cardano, too, claims he is "truly competent" in "natural magic", that is the study of the properties of bodies and similar questions, such the property of amber to conserve innate heat and its causes"[54] and he claimed to be the first "student to make practical use of the observation of natural phenomena".[55]

The natural magic of the 16th century is therefore increasingly linked to practical activities, while still preserving cabalistic, astrological, and alchemistic elements and a taste for the occult, the "secret", the fantastic analogy. While magic came closer to the mechanical arts, the extension and refinement of technics changed social conditions, posing new problems, changing artisans' ways of thinking, whose work was no longer the mere repetition of atavistic practices, but the attempt to dominate nature through experimentation, even if this was often crude. In the XVI century, in Italy the revived literary and artistic tradition met and amalgamated with occult techniques and science due to a number of favourable circumstances: the existence of a great number of libraries full of manuscripts, and the concentration of printers.

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[53] Agrippa, *Della vanità delle cose*, Italian translation L. Dominichi, Venice 1659, p. 57*v.* (the first Latin edition dates to 1531).
[54] G. Cardano, *Autobiografia*, edited by P. Franchetti, Einaudi, Turin 1945, p. 121.
[55] *Ibid.*, p. 156.

This happy synthesis of the old and the new, philosophy, magic and technics led to the discovery of the reality of nature per se, independent of the will of man, perhaps outside divine will. The new mental attitude naturally led to the first open opposition to Aristotle and the principle of authority and encouraged the study of details and the concrete. The leading exponents of this new approach in the century were Cardano and Porta, as we shall see better later.

3.11 Giovan Battista Porta

The most famous exponent of the renewed conception of magic was the Neapolitan Giovan Battista Della Porta, or Porta, as his contemporaries often referred to him, born in Naples between 3 October and 19 November 1535. Educated according the upper-class of the rich classes of his time and inducted to Humanistic studies by a learned uncle, Porta, at a young age, was immediately inflamed by all the marvellous and occult sciences: astrology, magic, alchemy. Using recent and ancient manuscripts in which he passionately studied the hidden and the arcane, and the less he understood, as he openly admitted, the greater was his desire to learn.[56] Fired by an obsession to excel, at a very young age in 1558 he published a *Magiae naturalis sive de miraculis rerum naturalium,* that was widely distributed, with 17 editions in Latin published up to 1588, and Italian translations (11 editions, up to 1628), French editions (15 up to 1678), and Dutch (3 editions, up to 1655).

This exceptional success convinced him that he was heading in the right direction, and a few years after publication he travelled to Italy, France and Spain with the principal purpose of meeting learned men, to consult ancient texts, learn new technics and discover new "secrets". He was denounced to the Inquisition - we do not know when or by whom - perhaps because he described the wonders of nature. In 1579, when perhaps the judicial case had been closed, he entered the service of the Cardinal d'Este, who, in 1580, sent him to Venice charged with the construction of a parabolic mirror and a pair of glasses.

The brief sojourn in the city, maybe two or three months, and certainly no more than five, had a profound influence on Porta's scientific activity: he visited the Arsenal and the glass-works in Murano, but, above all, he was fortunate to enjoy the friendship of Paolo Sarpi (1552-1623) who opened a whole new world of knowledge and method. Returning to Naples in 1581, he re-opened his house to friends and foreign travellers; he returned to his

[56] G.B. Porta, "*De furtivis literarum notis volgo de ziferis*", Naples 1563, *Praefatio*.

studies, chasing with ingenuous tenacity the ghosts of fantastic inventions, an easy victim of the many impostors of the time. Between 1589 and 1593, he published his first weighty writings on physiognomy, magic and agriculture. After a long interruption, due to new suspicions of the Inquisition and the tragic events of those years (Campanella imprisoned, Bruno burnt at the stake), in 1601, Porta returned with the publication of new works in which there is a clear sign of a change in his approach to analysis, marked by a reduced search for the spectacular, specific studies and greater attention to experimentation. This period includes the writings of greatest interest to physics: *Pneumaticorum libri* (Neapoli 1601), *De distillatione* (Romae 1608), *De aeris trasmutationibus* (Romae 1610), and *De telescopio* (of uncertain date and discovered only in 1940).

Striking up a friendship with a young Federico Cesi (1585-1630), who admired him greatly, in 1610 he was admitted to the Academy of the Lincei, the first after the four founders. He died in Rome on 4 February 1615.

The most important, widespread and talked about of Porta's works is the *Magiae naturalis*, which is not, as often claimed, a reworking of his youthful text, but a new work that has only the title in common with the first, and even that is incomplete as the author removed the sub-title *sive de miraculis rerum naturalium*, perhaps to avoid further persecution by the Inquisition. But its success was equal to the earlier work and from when it appeared for the first time in Naples in 1589, up to 1664 it was re-published 13 times in Latin and was reprinted several times translated into Italian, French, German, and English.

It is not easy to explain today why the two works met with such favour. Maybe it has less to do with their intrinsic value and more to do with the implicit revaluation of artisan work and the need of the times to get back to nature, even if still examined from a magical point of view.

A common element in both works is the informing spirit: the concept of magic. For Porta, natural magic is the richness and delight of the natural sciences, their quintessence. Works of magic are not marvellous because they go beyond the limits of natural possibility, but because the causes remain unknown to the common observer. The magus has made no deal with the devil, but he knows what to do and does it, and does to understand, in the belief that "with practice, the intellect better understands the theory".[57] In summary, Porta believed that the marvellous may be obtained without the intervention of angels and demons but by questioning nature and provoking its actions. "Natural magic" becomes the "science of

[57] G.B. Porta, *Della magia naturale*, edited by P. Sarnelli, Naples 1677, Book 5, Chapter 3.

miracles" in the Cartesian sense, perhaps led to coin the phrase[58] by a recollection of the sub-title of the juvenile work of Porta. In conclusion, Porta wanted to bring magic back to the study of natural things, to a practice imitator and dominator of nature. "Natural magic", wrote Tommaso Campanella (1568-1639), "is a practical art that uses the active and passive properties of natural things to produce stupefying and unusual effects, the causes and processes of which are not commonly understood".[59] It is a concept that downgrades the occultism of Paracelsus to the level of mere utilitarian curiosity, and therefore brings it closer to science as it would be intended in the coming centuries. To study natural things, but not the common and obvious things the cause and art of which we already know, because here there is no magic.[60] Campanella says: "Until the art is not understood, we call it magic: after it becomes science. The invention of gunpowder and printing was therefore magical, as well the use of the magnet, but now that everyone understands the art, it is common knowledge".[61] There are therefore two characteristics of natural magic, for both Porta and Campanella: natural actions; ignorance of the spectator of the causes, and therefore the spectacular, the stupefying, the unexpected and the mysterious. Did Porta really adhere to this new idea of magic? "Only historically, without explanation",[62] is Campanella's answer; that is, in modern terms, only by describing the magical actions of nature, without investigating the causes. And this is the difference between Campanella and Porta:[63] the latter sustained that magic is the pure description of the work of nature and does not seek the cause (Porta, therefore, was an advocate of experimental physics, as interpreted in the following centuries); Campanella, on the other hand, tended towards a philosophical physics, or a physical philosophy, that precedes the Cartesian scientific ideal.

But Porta's historical conception was one thing, scientific practice a completely different one. A balanced judgement of the experimental approach of Porta and the physicists of his time requires comprehension. In general, historians judge experimental the proposition established after

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[58] R. Descartes, *Oeuvres*, edited by C. Adam and P. Tannery, Cerf, Paris 1897-1910, Vol. I, p. 21; Vol. 6, pp. 343-44.

[59] T. Campanella, *Magia e grazia, inediti. Theologicorum liber XIV*, edited by F. Amerio, Bocca, Rome 1975, p. 165.

[60] G.B. Porta, *Della magia naturale*, op. cit, Book I, Chapter 2.

[61] T. Campanella, *Del senso delle cose e della magia*, edited by A. Bruers, Laterza, Bari 1925, p. 142

[62] *Ibid.*, p. 222

[63] Campanella points out (*De libris propriis*, I) that he wrote the *De rerum* after reading Porta's *Humana Physiognomonia* and discussing the concept of magic with him.

personal observation and experimentation. It is a very restrictive criterion, so much so as to render experimentation strictly subjective, with the consequent impossibility of accumulating knowledge acquired (and accepted) by preceding generations that makes scientific progress possible. Porta had a different conception of experimentation. His meaning is similar to the non-univocal meaning attributed by Pliny: experimentation means both common experience and witnessed and proven experience; it also covers medical prescriptions and it is also the common meaning.

We will describe later the not inconsiderable technical contributions of the Neapolitan magus to optics (§ 3.17), magnetism (§ 3.19) and pneumatics (§5.21), leaving aside minor contributions and those relating to other sciences. From what we have described so far, and that which is to come, we believe we may express a positive overall opinion of Porta's scientific standing.

During his life, his fame was enormous. Huge numbers of learned men and princes, Neapolitan and foreigners, flocked to his house like to a Sibylline cavern and treated what he said as the words of an oracle. Then, after his death, his fame rapidly declined and the following centuries conserve but a vague memory of a student of books rather than nature, of an acritical polygraph and an impenitent dupe.

The dawn of new times was the fundamental reason for this fall. But the opinions of Galileo's circle influenced, and still influence, his later esteem. The judgement of Sagredo is one of the most incisive, and sums them all up: after having begun reading the optics of Porta and Kepler, he wrote to Galileo expressing his dissatisfaction "with the style of writing of both and they seem to depart without need from the mathematical style and embrace that of philosophy"; in a later letter, he specified that Porta's book seemed "as awkward as possible" and among the mathematicians Kepler "may be termed peripatetic and enigmatic, as Porta occupies the same place among esteemed doctors as bells among musical instruments".[64] On his part, Galileo, without mentioning him by name, shrewdly describes Porta in the first day of the *Dialogue* as one who wanted to sell him "a secret about magnetic needles",[65] an obvious allusion to the *Proemio* of book VII of the twenty-volume *Magia*.

The hostility of the Galilean circle, exacerbated by the Portian claim of priority in the invention of the spy-glass (§ 3.17) was inevitable, because Galileo's mechanicist conception could not but refuse any idea of occult, sympathetic, magical or astrological causes.

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[64] Sagredo's letters to Galileo of 18 August and 22 September 1612, in Galilei, *Works*, op. cit., Vol. II, pp. 378 and 398.
[65] *Ibid.*, Vol. 7, p. 120

Nonetheless, Galileo's library contained both the four-volume and the twenty-volume *Magia*, the *De refractione*, the *De distillatione*, and the *De aeris transmutationibus*:[66] clear evidence that even the Pisan philosopher do not believe that Porta's science was all rubbish. And it is simple to point out that in some cases (balances, thermoscope, elasticity of air, spy-glass), the attention of scientists was drawn to the same objects of research, even if the investigative approaches used and the results obtained were always very different.

Alongside the doubts of Galileo and the hostility of his school, we must remember the esteem and friendship of Sarpi and Campanella, the excessive praise of Kepler, and the admiration of Cesi and Nicolas Fabri de Peiresc.[67]

To conclude, also from the evidence of the diverse esteem of his contemporaries, Porta appears as a figure of transition, half scientist, half magus, enamoured of natural phenomena, more inclined to foresee than to see, but attentive to the changes of the times that he accepted and for which he was partly responsible.

MECHANICS

3.12 Nicolò Tartaglia

The introduction of firearms posed new problems of scientific dynamics, and Nicolò initiated its study. He was nicknamed "*Tartaglia*" (the stutterer) by his companions due to a speech defect caused by five head wounds suffered during the sack of Brescia in 1512. He was born in Brescia around 1500 (a not completely reliable document says 1499) to a humble family, perhaps called Fontana. The family's economic conditions forced him to leave school after having learnt only half the alphabet. However, with an admirable show of character, he taught himself the sciences, particularly mathematics, and acquired such a vast and deep knowledge that he was called to Venice in 1534 to conduct public lessons. He held the post continuously, except for a brief interruption in 1548-49, until his death on 13 December 1557.

Famous as a great mathematician, Tartaglia's beginnings was a very minor work entitled *La nova scientia*, published in 1537 in three books of the five announced. The first two books expounded the new science, that is

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[66] A. Favaro, *La libreria di Galileo Galilei descritta e illustrata*, in "Bullettino di bibliografia e storia delle scienze matematiche e fisiche", 19, 1886, pp. 262-63, 267.
[67] Peiresc met Porta in Naples in 1601 and intended to write his biography.

the motion of projectiles; the third deals with the measurement of terrestrial distances, necessary for the correct use of artillery. The work begins with the proclamation of its greatest discovery: the maximum range of an artillery projectile is obtained when the weapon is inclined at 45° to the horizon. Tartaglia reports that in 1531, a close friend asked "how to aim an artillery piece to fire the furthest" and that he, who had never fired a cannon or a gun, "after much thought", concluded and demonstrated that "under natural and mathematical reasoning" the weapon should be inclined at 45°. Faced with his friend's questioning that the inclination was excessive "with some particular experiments, it proved to be absolutely right".[68] But if we look at the eighth proposition of the second book, where the theorem is set out canonically, concerning what these "natural and mathematical reasons" actually consist of, we can see that Tartaglia, in the two long and closely-written pages giving an alleged demonstration, vacillates between digressions and strange discourses that basically boil down to that, between two contrasting things, there is always a middle way. And he could know even less of the "clearly evident reasons" why "an artillery piece that had two different levels (that is elevations) could have the same fire-power", a fact that was put down to 'execution' (that had never been thought of or considered in ancient or modern times)".[69]

It is an exaggeration to say that Tartaglia discovered the theorem that range is equal to elevations of 45°±〈. The idea imparted by these passages and a reading of all the work is that the dynamics of artillery projectiles are the conclusion of how they would seem to an acute observer who had friendly conversations with expert bombers. From this, or through experiments with their cooperation, Tartaglia would have learned that the same target may be hit using two different elevations, but it is difficult to claim that he understood the mathematical relation, to which he makes no reference in this work, nor in the subsequent and broader *Quesiti et inventioni diverse*, also because it is difficult to claim that the artillery of the time was capable of guaranteeing conditions (weight of the projectile, explosive charge, etc.) that were exactly the same in two firings so as to result in equal initial velocities of the projectiles.

Practically, the trajectory of projectiles is Aristotelian, but Tartaglia states that no part of its non-vertical motion can be rectilinear, in the geometric sense, "because of the gravity found in that body, that is continuously influencing it and drawing it towards the centre of the Earth".[70] And in the *Quesiti* he gives a geometric explanation, founded on two

[68] N. Tartaglia, *La nova scientia,* Venice 1558, pp. III-IV. n.n. First edition of 1637.
[69] *Ibid.*, p. VI, n.n.
[70] *Ibid.*, c. II *r*.

axioms: a non-vertical trajectory moves the closer to a straight line the greater the speed of the projectile; in violent motion, velocity gradually decreases. If therefore *AB* is the rectilinear part of the trajectory and *E* an intermediate point, part *AE* would have travelled at a greater velocity that part *EB*. *AE* would therefore be straighter than *EB*, which is absurd because all *AB* is presumed straight.[71]

The *Quesiti et inventioni diverse*, appearing in 1546 as a continuation and development of the *Nova scientia*, is a vivacious work, also due to the almost completely dialogue construction, that would be copied and immortalised by Galileo: ordinary people, technicians and upper class "signori" discuss matter and raise practical questions, which Tartaglia raises to the level of scientific considerations. He realised the originality of this form of treatment and in a caudate sonnet *Alli lettori* (To the Readers), the introduction to the work, promises …

> *new inventions*
> *that do not belong to Plato or Plotinus,*
> *nor any other Greek or Latin, but derive*
> *only from Art, measurement and Reasoning.*

The work is divided into nine books. The first two are dedicated to artillery firing, and the others, respectively, to gunpowder, military arts, the use of the compass in topographic operations, fortifications, Aristotle's mechanics, and the science of weights; book IX, the most famous, deals with issues of mathematics.

The first two books broaden, without introducing anything new, the dynamics of the *Nova scientia*. The most notable of the others is book VII, in whose first problem claims, against Aristotle, that the real balances of short bars are more exact that those of long bars: the history and theory of Dmitri Mendeleev on scales showed that Tartaglia was right.

But book VIII is famous in the history of physics, even if it did not achieve the same celebrity in the history of mathematics as the much more original book IX. Book VIII contains a series of conversations, or rather a cycle of lessons, on the science of weights that Tartaglia gave to the ambassador to Venice, don Diego Hurtado de Mendoza (1503-1575). The

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[71] N. Tartaglia, *Quesiti et inventioni diverse*, Venice 1554, c. 11 *v.* This work was first published in 1546 and was reprinted four times (1546, 1554, 1562, 1606). There followed translations into German (1547, partial), French (1556, book VI), English (1588, the first three books), German (1778, book VI), French (1845-46, first three books). The 1554 edition is the most complete and has therefore been reproduced in facsimile, with an introduction by A. Mossotti in the annotated work *Nicolò Tartaglia*, Ateneo di Brescia, Brescia 1959.

text is the pamphlet *De ponderositate* by Nemorarius, that was discovered among Tartaglia's papers and published after his death by his editor Curzio Troiano.[72] Nemorarius is never mentioned in the lessons, something that should not be used as a moral judgement of Tartaglia, as many claim, but rather an example of the usage of the time and maybe also proof that the diffusion of Nemorarius' works (§ 2.3) was such that they did not require a reference, exactly as we do today for common knowledge.

Following Nemorarius, Tartaglia gave an exact definition of *gravitas secundum situm o peso accidentale*: the two bodies are "of equal weight according to their situation, or position, when their descent from those positions is equally oblique"; on the contrary, weight is greater for a body "without an oblique fall".[73] Translated into modern language, the proposition means that the accidental weight - that is, falling down inclined planes, the component of the weight of the body in the direction of the inclined plane - is proportional to the weight of the body times the projection along the vertical of the length of the inclined plane or of a unitary part of it. It follows that along the same inclined plane or equal inclined planes, the accidental weight remains the same irrespective of the point in which the body is placed, as can immediately be seen from the fact that the equal segments of the same line, or of two equally inclined lines have the same projections over the vertical. This proposition established by Tartaglia in problem 41 preludes to the jewel that closes book VIII in which Tartaglia, in an almost literal translation of *quaestio decima* of *De ponderositate,* sets out the theorem: the accidental weights of two bodies placed in two inclined planes of equal height, are the same if their absolute weights have the same relation as the lengths of the respective inclined planes.

Tartaglia's demonstration is also identical to Nemorarius's (even the letters of the figures are the same) and this should be mentioned as it was the first printed information on equilibrium on an inclined plane.[74]

Therefore, *DC, DA* are the lines of different inclinations and *DB* the common vertical projection (Fig. 3.2). Take *BK=BC*, therefore *DK* has the same inclination as *DC*; let *PP'G* be parallel to *CBK;* the bodies are in

[72] N. Tartaglia, *Jordani opusculum de ponderositate*, Venice 1565.
[73] Tartaglia, *Quesiti et invenioni diverse,* op. cit., c. 86 *v*.
[74] The first edition of the small work of Nemorarius, edited by P. Apiano (*Liber Jordani Nemorari [...] de ponderibus propositiones: XIII et earundem demonstationes*, Nuremberg 1533) does not include this theorem. This and other differences between the two editions led Duhem to suppose that the author of the treatise published by Tartaglia was not the author of the treatise published by Apiano, but another he calls "the precursor of Leonardo".

P,P',G using the same letters to indicate their weights; let *P*=*G*. Let's assume that

$$P : P' = DC : DA$$

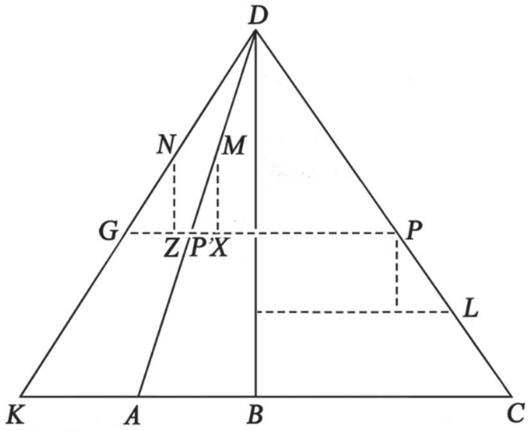

Fig. 3.2

Suppose that the accidental weight of *P* is greater than that of *P'*; therefore, if *P* and *P'* were connected by a wire, *P* would move of a segment *PL*, dragging *P'* of an equal segment *P'M*. Furthermore, take *GN* = *P'M* = *PL* and execute the other constructions indicated in the figure. Due to the similitude of the resulting triangles, the following proportions may be derived:

$$MX : MP' = DB : DA$$
$$NZ : NG = DB : DK$$

leading to

$$MX : NZ = DK : DA = G : P'$$

therefore, the two weights *G* and *P'* are in inverse ratio of the respective differences in height and have the same accidental weight. As a result, if *P* would drag *P'*, it would also drag *G*, but that is absurd as *P* and *G* are equal and positioned in equal inclinations.

It is briefly noted that if *DA* has the vertical position *DB*, Tartaglia's explanation coincides with that given by modern writers.

3.13 Girolamo Cardano

Cardano, the great rival of Tartaglia, also studied the equilibrium on an inclined plane, only marginally in *De subtilitate*, published in 1550, four years after *Quesiti*, but in more detail in *Opus novum de proportionibus*, published in 1570, five years after the appearance of Nemorarius's *De ponderositate* in Tartaglia's edition. And if this last booklet was unknown to Cardano, he had certainly read book VIII of *Quesiti*, because in the first "placard of mathematical challenges", distributed either on his initiative, or at least, with his consent, by his pupil and son-in-law Ludovico Ferrari (1522-1565), he accuses Tartaglia of plagiarising Nemorarius and making a claim for his own demonstrations "that are in many cases inconclusive".[75] Convinced of the inconclusiveness of Tartaglia's demonstration, that Cardano did not realise was actually Nemorarius's, in both the mentioned works he states that the accidental weight is proportional to the angle of inclination of the inclined plane on which the object is placed, instead of the sine of the angle, as Tartaglia had demonstrated. But was this demonstration really inconclusive? On re-examination, the reader will agree with us that Cardano's opinion was unjust and spiteful.

Cardano was born illegitimate in Pavia on 24 September 1501 (made legitimate in 1524) to Fazio and Chiara Micheri, an ignorant and irascible woman. He was taught by his father, a man of encyclopedic knowledge and a friend of Leonardo da Vinci (a coincidence mentioned by the supporters of the diffusion of the science of Leonardo, see § 3.9). In 1520, he entered the University of Pavia and in 1526 graduated in medicine in Padua. He practised with great success in Saccolongo (Padua), Milan, and Gallarate, and in 1543 he accepted the chair of medicine at the University of Pavia, where he taught, with a sole interruption between 1552 and 1559, until 1560 when he was struck by the greatest sadness of his life: his favourite first-born son was executed in the Pavia prison accused of having poisoned his wife. Broken by the shock, and also distraught at the dissolute life of his other son, he requested and obtained a chair at the University of Bologna. In 1570, he was imprisoned by the Court of the Inquisition, accused of heresy, particularly for having drawn up the horoscope of Jesus Christ and

[75] L. Ferrari, *I sei cartelli di matematica disfida*, edited by E. Giordani, Milan, 1876, placard I. These famous placards and the arguments between Tartaglia and Cardano on the priority of the solution of third-degree equations are a much-discussed subject in the history of mathematics.

attributing astral influences to events in his life. After some months in jail, in 1571 he moved to Rome where he was able to find favour with the Pope who awarded him an annuity that continued until his death on 21 September 1576. In Rome, during his final year, he wrote *De propria vita*, a highly interesting autobiography or auto-apology, translated into French, English, German, and, several times, Italian. It recounts an exalted and visionary life, mixture of wisdom and madness, of medieval superstitions and inspired intuitions. Cesare Lombroso (1835-1909) will take Cardano's life as an example of his theory on genius and madness.

Insensitive only to some artistic expressions (painting, sculpture, architecture), Cardano possessed a universal mind that excluded no branch of knowledge.

Of the over two hundred works he produced, two are of particular interest to the physicist: *De subtilitate*, already mentioned, and *De rerum varietate*, published in 1557, that together make up the most wide-ranging encyclopedia of 16th-century physical and natural sciences. They contain a bit of everything: from cosmology to the construction of machines, that sometimes recall those of Leonardo; from the usefulness of the science of nature to the evil influence of demons. They are a mine of true and imaginary facts, news on the state of science, beliefs, superstitions, technics, alchemistic manipulations, and the magical, astrological and chiromantic practices of the time.

The diverse subjects also include the development of dynamics that waver between the Aristotelian and the Medieval concept of impetus. Cardano believed that a projectile put in motion by a projector skill, that remains impressed like heat in water, but that the agitation of the air, with a modest effect at the beginning, accelerates the projectile when it reaches a certain velocity. The trajectory of the projectile is Aristotelic, but the central part is not the arc of a circle, as Leonardo and Tartaglia believed, but a line "*quae parabolae ferme imitatur*"[76]: it is an unfounded affirmation because, as Cardano adds, an exact measurement of violent motion is impossible, and the trajectory may be understood only "by conjecture".

Moving on from the study of trajectories, Cardano considers why objects hanging from a thread move more easily than those placed on a horizontal plane. The study resulted in interesting observations of the movement of pendulums; he noted that in successive oscillations, pendulums reached

[76] Cardano, *De subtilitate*, op cit, p. 98. The first edition is, as we have said, dated 1550. The phrase is oddly translated by Le Blanc (French trans. op. cit. p. 49) as "representing approximately the fourth part of a circle" and this is the translation accepted by P. Duhem, *Etudes sur Leonard da Vinci*, Hermann, Paris 19, Vol. 3, p. 181.

almost exactly the same height from which they descended; furthermore, he explained that a longer pendulum moves more slowly than a short one, because, with an equal length of the arc, its vertical rise is smaller.

The preceding modest and not always original contributions are enlivened by a profound intuition: the impossibility of perpetual motion. Cardano notes that the motion of the sky is perpetual, as is the motion of water that flows down rivers and drives mills. He therefore asks whether it is possible to build a machine in perpetual motion. First of all, he observes that the motion of river water is not an example of perpetual motion because the water moves in succession, according to its natural direction; on the contrary, a perpetual motion machine must contain its own cause of continuity "in the second generation", as if in a mechanical clock the weight, after falling, was spontaneously raised again. Basically, Cardano adds, "for the movement to be perpetual, it is necessary that what is moved returns to its original position at the end [*quod fert cum in fine est, denuo referatur*]".[77] But it cannot return to its original position without an effort; therefore, continuity of movement may be obtained as it is according its nature, and perpetual motion cannot be achieved.

Cardano also made important contributions to hydrodynamics. Against contemporary thinking, he observed that in a conduit of flowing water, the water does not return to the height from which it descended, but to a lesser height, and that height is less the longer the conduit. He studied the flow of rivers and believed it to be proportional to section and speed; all the same, he recognised that not all water strata in a river travel at the same speed, but that surface strata are faster, whereas his contemporaries believed the deeper levels to be quicker, distracted by the fact that the lower part of a clapboard immersed vertically in a river bends according the direction of the current.

Cardano's works were widely published: in the XVI century alone, five editions of *De rerum varietate* appeared, eight of *De subtilitate*, the first gradually emended and augmented by the author, and at least seven editions of its French translation, published in 1556 by Le Blanc and used as a textbook throughout the XVII century in French schools, especially for the study of statics and hydrostatics. The editorial success and the frequent references by writers of the second half of the 16th century demonstrate Cardano's considerable influence and the pedagogic influence of his works, especially as a stimulus for the study of the particular and the concrete, in a renewed conception of scientific research that was slowly freeing itself from the principle of authority.

[77] G. Cardano, *De subtilitate*, Lugduni 1559, pp. 610-11. This passage from book XVII is missing from the first edition and was introduced in the second (Basel 1554), on which the quoted 1556 French translation was based (p. 339 *r*).

3.14 Giovan Battista Benedetti

The exhibitionism of Cardano is contrasted by the reserve of Benedetti in revealing details of himself and his life, which is why we know very little about him. He was born into a patrician family in Venice on 14 August 1530; he had no tutors except for Tartaglia, who, in teaching him the first four books of Euclid, instilled in him a love for science. Aged 23, the self-taught Benedetti published in Venice his first small work that was to become an historical document on dynamics, as we shall see later. In 1558, Benedetti was summoned to the court of the dukes of Parma as a reader in philosophy and mathematics. He stayed there until early 1567 when he moved to Turin on the invitation of Emanuele Filiberto, duke of Savoy, who wanted him as a teacher of mathematics at the university and, at the same time, a builder of mathematical instruments (solar clocks, nautical armillas, and so on). In Turin, becoming intimate with the Savoys, Benedetti received honours and was ennobled. He died there on 20 January 1590.

Benedetti was without doubt the most original mechanical engineer of the XVI century. His greatest contribution to dynamics can be found in the dedication-preface to his first printed work, a collection of geometric problems solved using only fixed aperture compasses. There is a curious preface of 22 pages in a text of about one hundred pages of a disorganised and somewhat contradictory collection of disparate subjects but containing, almost as if forcibly, a long discourse on motion. The digression is justified by Benedetti by the fear that some plagiarist might, as had already happened to many others in his century, appropriate his discovery, defrauding him of the "oil and the work": the memory of the misfortune of his teacher Tartaglia hung over the young man.

Benedetti's discovery consists in a demonstration, diligently conducted, to prove, against the claims of Aristotle, that "two bodies of equal form and the same type, equal or unequal, in the same space and the same medium, move in the same time".[78] What was most original was not so much the statement but the demonstration, re-elaborated in a very rare pamphlet published in two editions, both in 1554.[79] Benedetti considered two

[78] G.B. Benedetti, *Resolutio omnium Euclidis problematum*, Venetiis 1553, preface, p. XVII, *n.n*.

[79] G.B. Benedetti, *Demostratio propotionum motuum localium contra Aristotelem et omnes philosophos*, Venetiis 1554. The dating follows Venetian usage, and therefore the treatise was published on the Ides of February 1555: it is therefore believed that this is the second edition of the pamphlet, only one copy of which has so far been discovered, held in the University Library of Padua. Two copies are known of the first edition, respectively owned by the Apostolic Vatican Library and the Berio

homogeneous spheres having their centres the same distance from the centre of the Earth and one four times larger than the other. Supposing the division of the larger sphere into four equal spheres and observing that each of these moves in the same time to that of the smaller sphere, and continuing the subtle reasoning concludes with the referred proportion. Thirty-two years later, in his major work, Benedetti returns to the reasoning and simplifies it, supposing a single body, that ideally divided into two equal parts, each of which should move at the same speed as the whole body: therefore, the bodies fall at equal velocity. All the same, it should be noted that the conclusion is valid for bodies "of the same type", not for any bodies: although no explicit explanation is to be found in the works in Benedetti, a reading of his writings leads to the impression that, in his opinion, the bodies, under equal conditions, and in the same medium, fall at a velocity proportional to their density.

Benedetti's reasoning was taken up by Cardano in proposition CX of the *Opus novum* (1570), copied by Jean Taisner (1509 - after 1562), repeated by Stevin, and appropriated by Galileo who removed the restriction of the equality of material: "I can imagine - the Pisan scientist wrote - two bodies equal in size and weight, as if they were, for example, two bricks falling at the same time from the same height [...] but if we imagine the bricks to be united and joined together during the descent, which one would, by the addition of the impetus of the other, double its velocity, given that the velocity cannot be increased by addition of a mobile if it does not move with a greater velocity?"[80]

But after Galileo came the erudite scholars who sometimes acted like the country parson who starts his history of the village with Adam. Historians, therefore, were amazed that such a simple reasoning as Benedetti's had to wait almost two thousand years to emerge. It is common experience that scientific reasoning, once discovered, seems simple: who has never had the feeling that, in repeating a classical demonstration, he has discovered it for himself? In any case, historians found precursors also of Benedetti: the most illustrious, but not the only one, must be the man of letters Benedetto Varchi (1503-1565) whose work written in 1544 but published only in 1827 would have affirmed that bodies fall at equal velocities. It may be that the proposition was made earlier, as we have also recalled, but the merit for the

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Civic Library in Genoa: see C.Macagni, *Contributi alla bibliografia di Giovanni Battista Benedetti*", in "Physis, 9, 1967, pp. 337-64.

[80] G. Galilei, *Postille alle Esercitazioni filosofiche di Antonio Rocco*, in Id., *Works*, Vol. 8, p. 731. The notes were drafted during 1634, but in the juvenile *De motu* (ibid., Vol. I, p. 265), Galileo expounds the same argument which was therefore present throughout his scientific career.

mathematical demonstration goes to Benedetti. He should also be recognised as one of Galileo's most authoritative predecessors in the discovery of the law of inertia. He mentions it in several passages of his masterwork where, dealing with rotary movement, he proposes the concept that "any heavy body, moved by natural causes or by violence, naturally tends to move in a straight line [*rectitudinem itineris naturaliter appetit*].[81] A refinement of this concept led him to explain the acceleration of the motion of a body under the continuous action of a constant force, therefore in falling bodies the progressive increase in velocity is due to the accumulation of the effects produced by the cause of the motion itself, and not the progressive increase in weight, as Aristotle claimed (§ 1.3). He applied the same ideas to rotational movement, recognising that the tendency of parts of the rotating body to move away from the axis of rotation is not an intrinsic characteristic of this motion, as was believed since ancient Greece, but a consequence of the propensity of each body moved to proceed in a rectilinear direction.

One problem, mentioned in the works of Leonardo, had become the subject of common dispute in the humanistic environment of Renaissance: according the peripatetics, when a weight falling in a channel diametric to the Earth arrived at the centre it would suddenly stop. But Francesco Maurolico, Cardano, and Tartaglia believed that opinion to be absurd. Benedetti resolved the problem in the most rational way: he thought that, similarly to pendulous movement, the weight would move in damped oscillatory motion and after many oscillations would stop at the centre.[82]

All the rest of Benedetti's contributions to physics are to be found in the above-mentioned masterwork that, divided into six parts, deals with theories of arithmetic and elementary algebra, perspective, mechanics and proportions. It also contains arguments and letters on mathematics and physics. It is a highly important anti-peripatetic work, as it can easily be shown by the complete adhesion to the heliocentric system "according to the beautiful opinion of Aristarchus of Samos, eminently expressed by Nicolaus Copernicus, the arguments against which of Aristotle and Ptolemy are useless".[83]

We will mention Benedetti's other contributions to physics as the occasion arises. For the moment, we will limit ourselves to recalling that his study of the equilibrium of a liquid in two communicating vertical tubes of differing section gave rise to his statement of the "hydrostatic paradox" (that is the equal pressure, at equal heights, exercised on the liquids at the bottom,

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[81] G.B. Benedetti, *Diversarum speculationum mathematicarum et physicarum liber*, Taurini 1585, p. 287.
[82] *Ibid.*, pp. 368-69.
[83] *Ibid.*, p 195.

independent of the form of the container), touching on the invention of the hydraulic press that would be invented by Marin Mersenne in the next century and spread by Pascal.

3.15 Simon Stevin

A year after the publication of Benedetti's *Diversarum speculationum*, the hydrostatic paradox was also set out by Stevin, one of the most original scientists of the second half of the 16th century. Stevin was born in Bruges in 1548 and after travelling widely in his youth through the countries of northern Europe, he became a teacher of mathematics in Leyden University. Appointed Quartermaster General of the Dutch army in 1593, he continued his studies of mathematics, mechanics, hydrostatics, navigation, geology and technology, and in 1600 he organised the teaching of mathematics in the school of engineering attached to the university of Leyden. He died in 1620, perhaps in The Hague.

It may be that the hydrostatic paradox was discovered by Stevin independently of Benedetti's work. In any case, his enunciation is much clearer and more explicit. We also owe to Stevin the introduction of the concept of *metacentre,* important in the study of the equilibrium of floating bodies, defined precisely and named only in 1746 by Pierre Bouguer (1698-1758).

Stevin's greatest merit in the field of physics is the original demonstration of the law of equilibrium of a body resting on an inclined plane. The reasoning is based on the consideration of the equilibrium of a type of rosary wrapped around two inclined planes whose section is a right-angled triangle with a horizontal hypotenuse and one cathetus double the other (Fig. 3.3). The rosary contains 14 equal and equidistant beads; two beads are situated on the smaller cathetus and four on the larger. If the accidental gravity of the latter were to be greater than the accidental gravity of the first two, the rosary would move from the lesser cathetus to the greater; once the movement starts, when a bead assumes the position of the next one, the preceding disposition is reproduced, therefore the motion continues: "This motion would never end, which is absurd", Stevin concludes.

An immediate result of the demonstration is the equilibrium of a body resting on an inclined body, already publicised by Tartaglia (§ 3.12), from which Stevin was inspired for his ingenious demonstration that marked a milestone in the history of physics. In fact, the impossibility of perpetual motion, postulated by Cardano (§ 3.13), had remained without applications and it was Stevin who, for the first time, made it a scientific principle: a scientific creation not as obvious at it may seem today, to prove which it is enough to remember that even after Stevin, and up to Sadi Carnot and later, the attempts to build perpetual motion machines multiplied.

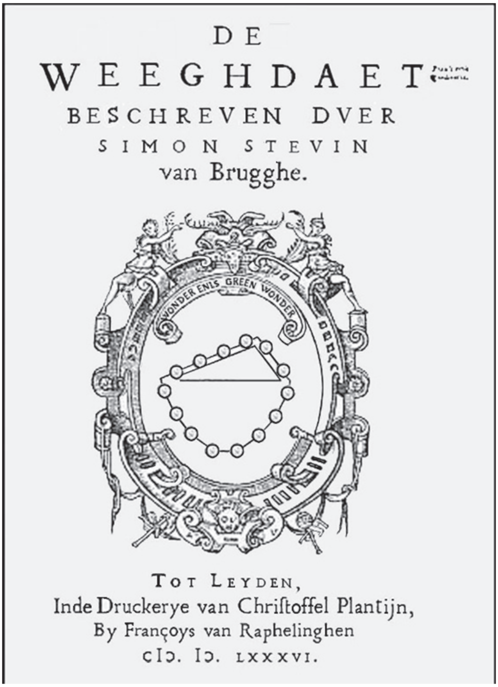

Fig. 3.3 - Frontispiece of Stevin's pamphlet on statics with a drawing of a rosary wrapped on two inclined planes (Leyden 1586).

Joseph-Louis Lagrange, expounding in the introduction to his *Mécanique analytique* the Stevin's "*très ingenieuse"* demonstration, made it famous and obfuscated that it was made possible by a knowledge of Nemorarius's theorem, publicised by Tartaglia.

From the consideration of the equilibrium of the rosary, Stevin deduced the law of the composition of concurrent forces and the principle of the decomposition of a force into two components perpendicular to each other. Both these laws, however, are limited to the specific case of three forces represented in size and direction by the three sides of a right-angled triangle and only refer to static effects.

Stevin made numerous mechanical inventions (relating to locks, windmills, road transportations) and his work in mathematics is well known. All the same, his influence was minor, a little because he, convinced that Dutch was the most suitable of all the ancient and modern languages to express scientific questions, wrote in his native language and his work was translated into Latin and French only in the first decade of the XVII century, and a little because his two most important works were published many years after his death.

OPTICS

3.16 Francesco Maurolico

Even minor, even insignificant, is the influence of the original writings on mechanics and optics by Maurolico. The son of a Byzantine doctor, he was born in Messina on 19 November 1494. He entered the Benedictine Order, was part of the state government, translated from the Greek several mathematical texts, and left writings, largely forgotten or lost, on every branch of knowledge of his time. He died in his native city on 21 July 1575.

The *Admirandi Archimedis monumenta omnia mathematica quae extant ex traditione F. Maurolici* contains his principal contributions to mechanics, but the work came to light in Palermo, after many vicissitudes, only in 1585, too late to be used, as it could have been, by contemporary scientists.

Perhaps less sterile were his speculations on optics, contained in a small volume that is the most original treatise on optics written in the three centuries after Witelo (§ 2.8). Actually, after Witelo, there are vague treatments of optics to be found here and there, but no other organic treatment, or one that is worth mentioning for novelty or clarity. In fact, the historian must recognise that there was a great confusion of ideas and, above all, there was a widespread diffidence about the perceptions of the eye, held to be the most deceiving of the senses. Writers delighted in wasting their time over describing *hallucinationes*, that is optical illusions.

Maurolico, on the other hand, did not indulge in the prejudices of the age and created a highly original work, divided into two parts, to which he added some questions on perspective and the rainbow.[84] The first part was completed in 1521, the second in 1554, almost thirty years after it was begun, as the author confirms. The questions about rainbows conclude with a mention of a book finished in 1567. The chronological dates are testimony to the author's constant and lifelong interest in optics, while the two later editions that followed in only six years and the attention paid by famous historians (Joseph Priestley, Gugliemo Libri, Baldassarre Boncompagni,

[84] F. Maurolico, *Photismi de lumine et umbra,* Neapoli 1611, followed by a second edition annotated by Cristoforo Clavio and with a slight change in the title (Lugduni 16). We have used the reprint of this second edition: *Theoremata de lumine et umbra*, Lugduni 1627.

Raffaello Caverni, and others) are testament to the originality and significance of the work.

The treatment of *Photismi*, that is *radiations* according to the meaning given by the author,[85] is founded on some postulations, the first of which states "Each point of a luminous body radiates in a straight line".[86] The proposition may seem identical to that of al-Haytham or Witelo, and Cristoforo Clavio interprets it as such in a note, but it is actually different. Al-Haytham made a distinction between *lux* and *lumen* and claimed *lumen* to be physical, as its rays were miniscule shreds or shadows that broke away from each point of the luminous body and dispersed in all directions. Maurolico, on the other hand, identified lux and lumen and took away the physical attributes of rays; his rays were simply geometric lines. In conclusion, Maurolico's conception is more abstract and geometric than al-Haytham's. Although it departs from physical reality, making optics geometrical simplified the problems and made progress possible.

But if the rays are geometrically straight, then the second postulation does not make sense. He says: "The densest rays give more intense illumination, while equally dense rays give equal illumination".[87] But how can a star (in geometric terms) be more or less dense of rays? Maurolico does not answer the question, for whom the concept of density easily fitted into his explanation of some simple photometric theorems. The first is: illumination produced by a light beam on a plane is greatest when the plane is normal to the beam and decreases with the decrease of the inclination of the plane with respect to the beam, because the part of the plane illuminated by the beam of light increases with the decrease in the inclination, therefore the density of the incidental rays decreases. The second states: illumination decreases with the increase of the distance of the puntiform source of light from the illuminated surface, because with an increase in distance, the number of incident rays decreases. This simplistic photometric criterion immediately betrays the scientist and makes him make a mistake. In fact, to demonstrate that equal sources may equally illuminate the same screen positioned at different distances, he considers a circumference chord as the screen and two points of the same circumference as the sources; it follows that the two angles of the circumference underlying the chord-screen are

[85] In a letter dated 8 August 1556 to the viceroy Giovanni De Vega, in which he gives a report of his studies. The handwritten letter is held in the National Library of Paris (Fonds latins, n. 7473) and was first published by F. Napoli, *Scritti inediti di Francesco Maurolico*, in "Bullettino di bibliografia e storia delle scienze matematiche e fisiche", 9, 1876, p. 35 of the extract.
[86] Maurolico, *Theoremata de lumine et umbra*, op. cit, p. 1
[87] *Ibid.*

equal, contain the same number of rays and therefore illuminate the screen equally.[88]

Maurolico was a great mathematician; so why was he so easily led to apply the concepts of equality and inequality to the geometric infinite? We can only give one answer: he had realised that puntiform sources of light may not all be considered equal geometric points but are differentiated by something else that he called the "density of rays" and that perhaps, in his intuition, corresponded to our luminous flux.

Maurolico moved on from photometry to the study of shadows and penumbrae produced by puntiform, spherical or flat sources of light. He gave a notable explanation of the round image of the Sun given by holes of any shape. The explanation, taken up and improved by Kepler, is based on the superimposition, at a certain distance from the hole, of images formed by luminous rays emitted from each point of the luminous body."[89] It should be noted here that this explanation, as in general for the creation of images, is supported by the geometric concept of rays: without recourse to simulacra or other supports, images are interpreted as the place of superimposition of luminous rays. Maurolico then deals with the question of images produced by the reflection of flat mirrors, that were a mystery in all Medieval theories. Maurolico explains that the ray originating from a point-object and reflected by a flat mirror has an extension that passes by the symmetric of the point-object with respect to the mirror and therefore it is as if it originates from this point through the mirror *tanquam per foramen*. It is an explanation that seems to be correct, without, however, achieving the clarity of Kepler's explanation which introduces the intervention of the eye.

There follows the treatment of spherical, concave and convex mirrors. The *Photismi* ends with a short page, written in 1555, on faults in mirrors and briefly mentions cylindrical and conical mirrors, whose effects "can be easily demonstrated and proved the experiment".[90]

The second part of the work, entitled *Diaphaneon seu transparentium*, divided into three books, opens with traditional postulations, especially the proposition that the angle of refraction is proportional to the angle of incidence and the image of a point-object, viewed by refraction, is formed on the point of intersection of the visual ray with the normal to the refracting surface. This last postulate does not allow the scientist to give a correct interpretation of the experiment of the broken stick.

Two propositions in the first book of the *Diaphaneon* attract our attention, first of all because they are the beginnings of the scientific study

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[88] *Ibid.*, pp. 1-7.
[89] *Ibid.*, p. 22.
[90] *Ibid.* p. 35.

of lenses. The first, in theorem XVIII, states that of two parallel rays at unequal distances from the centre of a transparent sphere, the furthest intersects the parallel diameter of the sphere at the closest point; the second, perhaps more intriguing, is in the corollary to theorem XXIII, in which he notes that the inverted image produced by bi-convex lenses (*conspicilla)* is clearer (*espressior*) than that using a crystalline sphere, because "all the rays emanated from one point meet more or less in a single point, so the single points form a clear image".[91] A little earlier, he demonstrates that from a sheet with flat and parallel faces, rays are emitted parallel to the incident rays and shifted laterally.

The second book of this part is dedicated to the study of the iris: there is a notable affirmation that there are seven colours of the rainbow,[92] and not the traditional four. He even adds, returning to the argument at the end of the text, that poets who attributed a thousand colours to the iris may have been right, perhaps because colours melt little by little into each other in a "infinite" variety.

The third book of the *Diapheneon* is without doubt the most original. Dedicated to the theory of sight and the form of lenses, the book opens with an anatomical description of the eye, as given by Andrea Vesalio (1514-1564). The greater understanding of anatomy allowed Maurolico to go beyond al-Haytham's theory of vision, while still taking this as his model. Maurolico recognised the crystalline lens as the most important component of the eye, so much so that "its form dictates the quality of sight, both short and long".[93] Rays are refracted in the crystalline and strike the retina on which the image is formed. The crystalline therefore functions as a lens. This is a fundamental new concept in the theory of vision, that clearly marks the Sicilian's idea as a break from that of all his predecessors.

In the crystalline, refraction occurs according to its shape, giving rise to "as the form changes, so does the angle of refraction and therefore the position of the visual rays changes and their paths necessarily merge and diverge. And, the smaller the transparent globe, the smaller the distance at which the rays meet; it may be said that those who have a narrower pupil [*conglobatior*] are short-sighted [....], which is why some people are very short-sighted".[94] The cause of presbyopia is the opposite. Therefore, Maurolico concludes, "convex lenses correct an excess of long-sight and

[91] *Ibid.*, p. 55.
[92] *Ibid.*, p. 93.
[93] *Ibid.*, p. 55.
[94] *Ibid.*, p. 85.

concave lenses correct short-sight".[95] But the lens must be adapted to the "grade" of the defect: the greater the myopia, the more concave the lens.

There! We finally have a scientist able to understand how spectacles work, after three centuries of uninterrupted use.

But if the crystalline functions as a convergent lens, images should be reproduced on the retina upside-down. Maurolico saw the embarrassing consequence, but rejected it with a series of cavils; the force of prejudices is great even in the highest wits!

The work curiously closes with a description of a phenomenon of diffraction, assimilated to the phenomenon of the rainbow: "If you look though a white feather of a dove or another bird placed in front of your eyes in the light of a not-too-distant candle, you will see between the ribbing and the fissures of the feather a sort of cross painted in a marvellous variety of colours, which is what one sees in the rainbow".[96]

Maurolico's booklet is short, just 84 pages (in addition to the first six unnumbered ones) in the first edition, but it is a major work, and it is a shame that it appeared fifty-seven years after the drafting of the second part!

3.17 The invention of the spyglass

The works of Maurolico were unknown even to Porta (§ 3.11), a fervent student of manuscripts and "secrets", whose writings contain the first systematic printed Renaissance treatises on optics.

Besides the *De refractione optices parte*, Porta dedicates book IV of the first edition of the *Magia* and book XVII of the second edition to optics. In the preface to the latter, he promises marvels: "Although venerated antiquity invented many and great things, we have discovered greater, more real and more illustrious things, that will please those who look with pleasure to the future, and noble and sublime minds will have occasion to invent an infinite number of others".[97]

But it is not all boasting: the pages of this book XVII and book VII, which we will deal with later (§ 3.19), are notable in the history of the origins of our science and serve as a model for Porta's "natural magic", even though he did not always, actually rarely, realise it. A first observation is clear even to the most distracted of readers: Porta had a long practical experience of optics, made more concrete by his visits to the glass-works in

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[95] *Ibid.*, p. 86.
[96] *Ibid.*, p. 94.
[97] Porta, *Della magia naturale*, op. cit. The first edition in Latin appeared, as we have said, in 1589. We have compared the quoted passages with the Latin edition to ensure their basic correspondence.

Murano, as we have mentioned (§ 3.11), and expanded by the conversations with Sarpi.

In his youth, friar Paolo studied natural sciences and mathematics and was particularly fascinated by the naturalistic and geometric nature of optics. But he did not limit himself to the classics - Euclid, Ptolemy, al-Haytham - but carried out original research which he freely discussed with his great many scientist friends. One case is worthy of special mention, relating to the historical period and the argument we are dealing with. In one of his "reflections", written around 1578 but published in 1882, Sarpi, recognising that the apparent size of objects depends on the angle in which they are observed, adds that this is the result of the "eyeglasses and other lenses that enlarge or reduce the object, simply by making the angle bigger or smaller".[98] Now, this theorem of the angular enlargement of lenses appears for the first time in a text by Marco Antonio De Dominis (1560-1624), *De radiis visus et lucis* (Venetiis, 1611). De Dominis was a great friend of Sarpi and is known for taking part in the political and religious struggles of the time that led him to be incarcerated in Castel Sant'Angelo until his death. De Dominis's treatise has no particular scientific originality: we may mention, apart from the above theorem, the observation that a rainbow is caused by the reflection of light on the posterior surfaces of water droplets. It may be easily be inferred from this that he learned from Sarpi the theory of the angular enlargement of lenses. Other examples of the liberal thinking of friar Paolo will be seen in this chapter and in § 3.19.

Returning to Porta, we believe that in writing book XVII of the *Magia*, he made public Sarpi's science, in his specific teaching and methods. Chapters 1-3 of the book deal with plane mirrors, with angular placings and irregular surfaces to obtain diverse illusions, that are even more spectacular in the theatrical mirror, that from Euclid onward was a marvel of optics. Chapter 4 concerns the concave mirror, determining the "point of inversion", that is the focus, and instruments are described that give amazing results. Chapter 5 deals with the effects obtained through combinations of flat and concave mirrors. Chapters 6, 7, and 8 describe the *camera oscura* with all the marvels that it offered to the eye of the beholder (Fig. 3.4). Following on from paragraph 2.7, it might be useless to add that Porta did not invent the *camera oscura*, as some have claimed, nor is he the precursor of the modern camera because he applied a lens to the hole of the *camera oscura*. This innovation dates to before 1550, as Cardano mentions in *De subtilitat*e, without any attribution, and Daniele Barbaro in a book on perspective

[98] P.Cassani, *Paolo Sarpi e le scienze naturali,* in "L'ateneo veneto", 6th s, 1882 , p. 308. This text Cassani publishes for the first time Sarpi's "reflections" on natural sciences, drawn from a manuscript housed in the Marciana in Venice.

published in 1567 not only describes the *camera oscura* with a lens but also, in a lovely passage, notes spherical aberration and lays down the method to correct it - a method still used today: that is, to “diaphragm” the lens. In 1573, Egnatio Danti’s translation and commentary of Euclid’s optics describes the *camera oscura* and advises using a flat mirror to straighten the image. Last, in 1585, Benedetti describes a *camera oscura* with a lens, leading us to believe that he was also its inventor.

Fig. 3.4 - Porta’s application of the improved *camera oscura*: *AB* is a mirror inclined at 45° to the horizon; *E* is a convergent lens that projects the image of an external object on to a sheet of drawing paper. *Source*: A. Saverien, *Dictionnaire universel de mathématique et de physique*, Paris 1754.

So, what can we claim for Porta?

He achieved a lot. The first is the straightening of images using concave mirrors as he demonstrated in the 1558 edition of the *Magia*. He was also responsible for the application of the *camera oscura* in drawing. He had the idea of using the *camera oscura* to project drawings placed in front of the hole illuminated by the sun and candlelight which gave rise to the magic lanterns of Athanasius Kircher (1601-1680; *Ars magna luci et umbrae*, Romae 1646). Of greater scientific importance is the fact that in the 1558 edition, Porta uses the *camera oscura* to explain the theory of sight. After describing the experiment, Porta continues: “It is from here that philosophers and medical doctors showed in what region of the eye vision is formed, and has answered the difficult question of intromission, and that no other argument over the question could be more efficiently demonstrated: if we introduce a small image through the pupil, like a window, the small part of the large globe positioned at the base of the eye takes the place of the mirror”.[99]

It may be that this application of the *camera oscura* came down to Porta from Leonardo through Cardano, but this is certainly the first printed version we have. It is, however, a shame that Porta makes no mention in neither this version nor the later 1589 edition of the inversion of images in the sentient part of the eye.

Porta’s first eight chapters are followed by the ninth that deals with cylindrical and pyramidal mirrors. The tenth is dedicated to lenses in a complex experimental and varied theoretical manner that is both interesting and extremely organic. The title of the eleventh chapter announces: “Eyeglasses by which a man may see at a distance, that advances every thought”: a modern reader would say that we have here the spyglass, and would not be surprised because all the preceding treatises would have led directly to the spyglass, at least a telescopic spyglass, with a parabolic mirror and lens. But a reading of the chapter, that in its times tested speculative thinkers beginning with Kepler, is disappointing. It is a totally incomprehensible description of lenses, mirrors, parabolas, and letters to be read from a distance: all ingredients to be found in the following “reflection” by Sarpi: “One or more mirrors may help a man see far beyond himself, or see much closer, the same as with glasses. It enables him to read letters as far as 50 paces. I have tested it with a sphere, or with a lens, but it is better with a parabola and a lens, and reading away from the light”.[100] And almost in a commentary on this reflection, Sarpi himself, in a letter dated 6 February 1609 to De l’Isle Groslot announces the appearance of the spyglass in Venice. He writes that as a young man he also imagined a similar thing, but, he adds, “I did not confirm it, nor did I think to repeat the experiment. I do not know whether I remembered that artifice or whether the idea grew, as often happens, over time”.[101] We may suppose that in the famous eleventh chapter of book XVII of the *Magia*, Porta had made an obscure attempt to announce the telescopic device: parabolic mirror / magnifying lens, that friar Paolo had imagined and that Porta, after working

[99] Porta, *Magiae naturalis*, op. cit., p. 144.
[100] Cassani, *Paolo Sarpi e le scienze naturali,* op. cit., p. 230.
[101] P. Sarpi, *Lettere,* Barbera, Florence 1863, Vol. I, è. 181.

on it many years, had not been able to construct. Porta always refused to explain this chapter eleven and merely stated that friar Paolo had understood it; it was only after the instrument was used by Galileo that he claimed his priority and declared it a "nonsense".

A "nonsense" that was studied for centuries because the hope was as old as lenses and is almost natural in anyone who has used a magnifying glass, to ask how is it possible to multiply the magnification *ad libitum*. And it is this confusion between hope and truth that has led may historians to attribute the invention to various people: to Bacon who wished to create lenses that would make a man seem as big as a mountain; Leonardo da Vinci who wanted to make spectacles to see a huge moon; Girolamo Fracastoro who, in 1538, wrote that by looking through two superimposed glasses everything would appear bigger and closer; Leonardo Digges who, in 1571, published a book on how to pair concave and convex lenses; Sarpi; and Porta.

Huygens, who was no stranger to optics, wrote in his *Diottrica* that a man capable of inventing a spyglass, based only on a theory, without the intervention of chance, would be a superhuman genius. 16th century theories of optics did not lead to the telescope, actually the opposite. Proof can be found in the greatest treatise on optics of the century, the already-cited *De refractione optices parte* by Porta, published in Naples in 1593. But this is not the Porta of the *Magia*; maybe times were changing; maybe the experience of his life of studies, with the disappointments by the marvellous discoveries that he so innocently believed in, maybe because of the lessons of friar Paolo; whatever, the fact remains that in *De refractione*, Porta demonstrates a new scientific commitment, critical sense and seriousness of intent and method.

He followed the classical theory of sight, so incapable of explaining refraction to induce the reader to the conviction that refraction was a lie, a trick of nature. For example, one of the first propositions, the fifth of Book I, states that "a refracted image strikes the eye in straight lines",[102] that is an incomprehensible proposition and a contradiction in terms. It shows that Porta was totally unable to explain the most common proofs of refraction - the broken stick - which he dealt with in Book I. Book II is even worse, where, following the unfortunate Medieval usage, he deals with refraction in the *pila crystallina*, that is a glass ball. In this case, the aberrations, augmented by Porta's physical-psychological observations, mask the main phenomenon. After five books dedicated to the anatomy of the eye and the theory of sight, in the eighth, Porta deals with the most interesting and innovative argument: lenses. It contains some notable observations: consideration of the axes of the lenses, representation of real and virtual

[102] G.B. Porta, *De refractione optices parte*, Neapoli 1593, p. 12.

images, physiological observations, such as the beautiful description of the dilation of the pupil according to the intensity of the light it receives,[103] a phenomenon that Galileo considered, "among thousand [....] there are not two, nor even one that I saw".[104] But, despite all this, and other considerable contributions, the reader is left with the fundamental impression that lenses are not to be trusted. They enlarge and reduce; they give one double vision; they make one see colours that do not exist. No, one cannot trust lenses.

How could the spyglass be created in such a scientific environment?

In fact, all the documents currently at our disposal point to the fact that the spyglass was not created in learned circles but in the artisan workshops, the glass-workers who, in particular, made spectacles, a trade so widespread by then that a specialist industry had arisen.

There is no doubt, especially after the recent publication of the inconclusive *De telescopio*, that Porta did not invent the spyglass. However, it may be claimed that the twenty volume *Magia*, in its numerous editions and translations, publicised Sarpi's theories of optics that, detailing and perfecting them, "like fame through travel", led to the first construction of the instrument.

Despite the mass of documents, historians still cannot tell us exactly where and when spyglasses first appeared. Many claims and names have been put forward. We know for sure that in 1604 many people were gazing through spyglass and we may believe from a later document of 1634 that in 1604, in Middelburg, Holland, Zacharias Janssen built spyglasses according to a model that he said came from Italy and that was dated "1590".

If this is the date of birth of the spyglass, it had a gestation of 18 years, until 1608, without attracting anyone's attention, not least of scholars. In 1608, some soldiers began to take an interest in the instrument, but without enthusiasm, justified by the limited quality of these early spyglasses that did not go beyond three magnifications and produced blurred images due to the poor processing of the lenses. They were treated as show-ground bagatelles.

In the spring of 1609, news of the instrument arrived in Venice and came to the notice of Galileo. Ten months later, the *Sidereus nuncius* appeared, the heavenly message (or *Avviso astronomico*, as Galileo more modestly called it), that announced new times, as we will describe more extensively in the next chapter.

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[103] I*bid*, p. 74.

[104] G.Galilei, *Dialogo sui massimi sistemi,* in Id, op. cit., Vol. 8, p. 390.

MAGNETISM AND ELECTRICITY

3.18 Magnetic declination and inclination

A modern reader of Peregrinus (§ 2.12), after admiring the clarity of, and systematic approach to, the demonstration, can only agree that the author was not a compiler, but an acute experimenter. He does not refer to what he had read or heard, but to what he had personally proved. Consequently, when Peregrinus states in the seventh chapter, and repeats in the tenth, that a needle points to the North Pole, it must be concluded that this is the exact position of the magnetic needle he discovered. Moreover, there are other clues to believing that in Peregrinus's time, magnetic declination - that is the angle that the magnetic meridian forms with the geographic meridian of the point of observation – was zero in Italy. Therefore, the two notes found only in one of Peregrinus's letters, held in the university library of Leyden, in which he mentions a declination of 5°, should be taken as later interpolations.

So, who discovered magnetic declination? The question is still unanswered. Up to the XIX century, there was almost universal agreement on Christopher Columbus (1451-1506), during his first voyage to the Americas (1492). His son Ferdinando (1488-1539) made the claim in the biography of his father and it seemed totally natural, as, while it would have been difficult to understand magnetic variation while navigation was limited to the Mediterranean, the phenomenon could not escape the notice of sailors of the oceans. But in 1905, a German scholar, August Wolkenhauer, demonstrated that around the 1400s solar clocks were being constructed in Germany (there are still examples in German museums) that showed the angle that the direction of a magnetic needle forms with the midday gnomon. But this fact alone - the only proven one among all the other claims - cannot dissuade historians from the old belief that it was Columbus who discovered declination, because it seems very strange that the phenomenon was discovered on dry land and was unknown by the sailors who were more practised and interested in the uses of the compass.

Even if we do not know who discovered it first, we can set a date for when sailors understood the phenomenon: the early XVI century.

Navigators soon became aware that magnetic declination varies from place to place. Moreover, even though they were ignorant of the variation in the same place according to the weather (discovery of which naturally required more prolonged observation and was only made in 1634 by Henry Gellybrand, 1597-1636). For all of the XVIII century, they believed that an understanding of it in any position would solve the other great problem of navigation, the determination of longitude, that they thought was connected to declination in such a way that understanding one would deduce the other. This mistaken belief gave rise to the first *carta magnetica* that the Jesuit missionary Cristoforo Borri (born in Milan at an uncertain date and died in Rome in 1632) published in the *De arte navigandi*, drawing up a geographical map with lines linking places which, according to the data available to him, had the same magnetic declination. Magnetic maps spread after they were published in 1701 by Edmund Halley, who is usually given credit for them.

The phenomenon of magnetic inclination - that is that a magnetic needle free to rotate around a horizontal axis indicates, in the northern hemisphere, the north pole pointing downwards - is one of the questions that demand a coordinated series of experiments. At the same time, using a needle free to rotate on a vertical tip, the angle of inclination must be smaller, resulting in the phenomenon being harder to detect. With a floating needle, or a needle set in a pivoted cylinder, or with a floating magnetic sphere, the phenomenon is not observed. And yet, when the phenomenon is observed using a needle supported by a vertical tip, it is easy to attribute the cause to a dissymetry in the construction that inclines the north pole downwards. In order to discover the phenomenon, one needs to make an iron needle, balance it precisely on a vertical point so that it is horizontal, then magnetise it and verify that, placed in the vertical point, is does not stay horizontal. And it is this exact series of experiments carried out in 1544 by the German Georg Hartmann (1489-1564), the discoverer of the phenomenon, who found an angle of inclination of 9°, a measure too small precisely because the instrument used was good for revealing magnetic declination but not inclination. Later, in 1576, the Englishman Robert Norman explained how to make the needle free to rotate along a horizontal axis, constructing the first inclinometer.

3.19 The first Italian treatise on magnetism

While Norman was experimenting in England, in Italy Porta was busy working on every arcane, magical phenomenon. And what could more arcane for him than magnetism? And it is clear that, looking for the marvellous, he often saw one thing for another; he believed rather than proved, imagined rather than constructed.

But although this is the worst accusation against him, it must be recognised that book VII of the *Magia*, already cited in paragraph 3.17, is the first Italian treatise on the science of magnetism. A considerable contribution came from Sarpi, as Porta himself acknowledged in the preface: “When I was in Vineggia I realised that the same speculation had been posited by the Reverend Father Paolo Venetiano of the Order of the Servants, at one time a provincial friar and now a dignified Procurator in Rome, from whom I have learned many things and I am not ashamed to admit it. On the contrary, it gives me immeasurable joy to have known, in my life and in all the places I have visited in the world, and those who visited me in Naples, the most ingenious and learned of men, born only to devour all the sciences. He is an honour, splendour and ornament of all that is sublime, not only for the Republic of Venice, but for all of Italy".[105]

Book VII of the *Magia* can best be divided into three parts, discarding the last part made up the final chapter, 59, that recounts all the myths and yarns handed down over the centuries regarding the magical qualities of the magnet. The first contains an experimental explanation of known magnetic phenomena. The second part criticises and refutes ancient false beliefs, with a notable independent spirit that borders on an impatience of the principle of authority. The third part, which interests us most, gives an original intake to the science of magnetism. We will mention the more significant contributions, starting with the observation of the greater attraction of the magnet for iron filings over magnetic shavings. The following experiment is new and beautiful: place iron filings in a paper bag and put the magnet alongside; the filings will be magnetically charged as if they were a single iron; then, pour out the filings and mix them, then put them back in the bag; the force will be confused and dispersed. The experiment was repeated by William Gilbert to prove his theory, and then by Francesco Maria Grimaldi who used it as the basis of an ingenious theory that preludes to that of James Ewin from the late XIX century. We owe to Porta also the experiment of the hairs of iron filings that thicken at the poles of a magnet and that can be considered the first observation of magnetic spectra. Two other authentic and important discoveries should also be mentioned. The first, described in chapter 16 is the magnetic shield of iron sheets; the second, described in chapters 53 and 54, is the experimental observation that a magnet heated to high temperatures loses its magnetic qualities: a phenomenon studied in the XIX century by a great number of brilliant physicists, from Michael Faraday up to Pierre Curie (who gave his name to the *effect*).

3.20 William Gilbert

The most open and proudly polemical protagonist - perhaps explainable by the general conditions of his country - in the history of magnetism was the Englishman William Gilbert, born 24 May 1544 in Colchester, Sussex.

[105] Porta, *Della magia naturale*, op. cit. Lb. VII, *Proemio.*

After graduating in medicine in 1578, he visited the continent where he spent four years, it appears mostly in Italy, meeting Sarpi, probably in Mantua. Returning home, he practised as a doctor in London so successfully that in 1601 he was appointed the queen's personal physician. But on 30 November 1603 he died of the plague.

Gilbert's masterpiece, *De magnete*, inspired both the philosophy and the verbal violence of Francis Bacon. The work opens with a vicious philippic against contemporary philosophers: "Why [....] should I submit this noble philosophy, that for the things never pronounced is almost new and inadmissible, to the judgement of men who swear on the opinions of others, the worst corrupters of the arts, the learned buffoons, grammarians, sophists, declaimers, and the stubbornness of common people, so that this philosophy is condemned and covered in insults? Just to you, true philosophers, honest men, who not only in books, but in the very things you try to understand, I have dedicated these fundamentals of the science of magnetism, treated with the new approach to thinking".[106]

This new way of thinking consisted in a search for understanding not only in books, but also in the objects themselves, subjecting them to long and patient experimentation. And the long experimentation, that lasted eighteen years, is the real value of Gilbert who describes more than six hundred experiments and arrives at a concept of great scientific and philosophical significance.

Inspired by Peregrinus, Gilbert constructed a spherical magnet, the *terrella*. Then, with a small pivoted magnetic needle positioned on the surface, he studied the magnetic properties of the *terrella* and found that they corresponded to the magnetic properties of the Earth, the "big magnet - *gran calamita*". He therefore concluded that with regard to magnetic actions, size is the only difference between the Earth and the *terrella*.

This concept -judged to be "stupendous" by Galileo- had an importance that went well beyond its purely technical idea: it was the first time that someone had the temerity to compare a phenomenon tested in a mere human laboratory to a cosmic phenomenon. A severe blow was thus inflicted on the age-old myth that set the sub-lunar world against the heavens, because, in the last analysis, Gilbert's conception stated that cosmic phenomena could be studied using the same methods as for phenomena of human scale.

But the part of the work that appeared most revolutionary at the time was the last, book VI, where Gilbert not only gives his full support to the Copernican system, but also attempts to demonstrate the rotation of the Earth around its own axis using the arguments of magnetism. While

[106] W. Gilbert, *De magnete, magneticisque corporibus et de magno magnete tellure physiologia nova*, Londini 1600, preface, p. 11 n.n.

doubting that Peregrinus's *terrella* (§ 2.12), precisely balanced on its poles, completes a rotation of itself in 24 hours, he takes it as a model and proof of the diurnal rotation of the Earth. Gilbert's resolutely Copernican outlook had a profound influence of the formation of many of his contemporaries - we need only mention Galileo and Kepler. At the same time, it provided a weapon for followers of Ptolemy for whom it was easy to respond that as the *terrella* balanced on its polar axis does not rotate perpetually but remains perpetually immobile, so the Earth remains immobile.

In addition to the above stupendous idea and a personal reworking of the understanding of magnetism handed down over the centuries, Gilbert's work also contains some new experimental facts, such as, for example, that an iron wire extended over the magnetic meridian, hammered and flattened, assumes magnetic polarity, or the idea to increase, through accurate working of the surface and using an "armature" (it seems that Gilbert first introduced the term in a technical sense), that is an iron cawling, the power of the magnet, a technique that was hugely improved by Galileo who did not "fill" the armature, as the Englishman had done, but flattened it (calling it *ancora* - anchor), as he opened the suspended iron part to which it should adhere.

But when Gilbert attempted a theory of magnetism, after long and obscure disquisitions, he concluded that "Thales's opinion that a magnet has a soul is not absurd".[107] He must have been joking: going back to Thales was a bit much! It was also a bit much because Porta, managing on this occasion to free himself from the magical concepts of sympathy and antipathy, had introduced the more acceptable hypothesis of a magnetic fluid.

3.21 The birth of electrology

Gilbert originated the science of electricity, practically unchanged from Thales until the 17th century, that is the belief that amber - and perhaps also a hypothetical body known as "lincurio" - when rubbed attracts straws. We may ask why such a commonly known property had been attributed only to amber for so many centuries. One of the fundamental explanations must be that electrification by rubbing together two or more bodies is so weak as to hide the effect, without the aid of a sensitive tool. Basically, in modern parlance, it was necessary to go beyond the *threshold* of the phenomenon.

Perhaps the famous scientific poet, Girolamo Fracastoro (1483-1553), arrived at this conclusion. In 1550, in his *De sympathia et antipathia rerum*, he described a machine made up of a small bar suspended over a point

[107] *Ibid.*, p. 68.

representing a magnetic needle, by which he argued that amber does not only attract gold particles and shavings, but also silver.

Although Fracastoro did not pursue his experiments, Gilbert, understanding the support he would get his device, certainly claimed the tool to be of his own invention, giving it the name of *versori*o and using it in his systematic research, described in the second chapter of book II of *De magnete*.

Through the use of this first electroscope, Gilbert proved that not only rubbed amber, but also rubbed diamond, sapphire, carbuncle, opal, amethyst, beryl, crystal, glass, belemnites, sulphur, sealing-wax, rock-salt, firestone (selenite), and alum attract. He named each of these substances an "electric body", the abstract "electricity" appeared in 1650. Gilbert also proved that each of these elements attracts not only particles and shavings, but also "all metals, woods, leaves, sods, and even water and oil, and everything known to our senses".[108] On the other hand, it appeared to him that it did not attract other bodies, such as metals and some types of wood and stones, and he astutely observed that flame cancel the faculty of attraction of the rubbed bodies.

After such extensive experimentation, Gilbert attempted a theory of the attraction of electric bodies. He argues against the two 16th-century explanations of the attraction of amber. One stated that heat has the property of attraction and that amber attracts because it is heated by friction. But Benedetti had already shown that the property of heat is rarefying and condensation, not attraction. Gilbert repeats Benedetti's conclusions, adding that if the property of heat is attraction, all heated bodies would be attracted, not just amber. The other theory has an illustrious history dating back to Lucretius: the effluvia from rubbed amber produce a rarefaction of the air, therefore the shavings are driven by denser air into the partial vacuum produced by the effluvia. But if this is true, the English scientist noted, also heated bodies and flames should attract and an electrified body should attract the flame of a candle placed nearby, while not only it does not bend, it but also loses its power.

Gilbert's observation is undoubtedly astute, but his theory is no more believable than those against whom he was arguing. According to Gilbert, all bodies derive from two prime elements: water and earth. Those deriving from water have the power to attract because water emits special discharges that "like widespread arms" capture the body and drag it to their source, and, having penetrated it and almost hooked it, continue to embrace it until they weaken and, when nerveless, abandon their prey, and so on and so forth in similar arguments. It cannot be said that Gilbert's theories were better

[108] *Ibid.*, p. 48.

than Cardano's or Porta's, and it is surprising that the English scientist refers to an electrical fluid while refuting a magnetic one (§ 3.20).

In establishing a distinction between magnetic and electrical attraction (already posited by Cardano, whereas previously the two phenomena were believed to be the same), Gilbert noted another important fact: it is difficult to electrify damp bodies by rubbing them, while humidity does not impede magnetic attraction.

We will pass over the other distinctive characteristics of the two phenomena to conclude that under Gilbert the science of electricity, previously limited to a single curiosity, was enriched by a number of new phenomena, priceless observations and an instrumental technique that is in itself a new chapter in science: Gilbert deserves the title of "father of electricity".

3.22 Francis Bacon

While praising the use of experimentation in the study of natural philosophy, Bacon accused Gilbert of having deduced from many experiments in one subject only a general philosophy, an imaginative and senseless result:[109] the judgement is a clue to Bacon's mental attitude.

Born in London on 22 January 1561 (new calendar), Francis Bacon completed his studies at Trinity College, Cambridge, and in 1576 he was sent to Paris in the company of the British ambassador. He returned around 1579 to begin the forensic practice. He obtained a modest post at court, and in 1593 began his political career in parliament, accelerated by the accession of James I (VI of Scotland) in 1603. Through of series of intrigues that do not leave his moral figure unblemished, in 1618 he was appointed chancellor and baron Verulam; in 1620 he was created Viscount St. Alban. But in the same year, accused of corruption, charges that he admitted, he was forbidden from holding public office, deprived of his seat in parliament and imprisoned for a few days. Forced to lead a private life, and with the failure of his repeated attempts to return to his former post, he intensified his philosophical and literary studies that he had cultivated throughout his life as far as the political troubles allowed. He died on 9 April 1626.

From his early years, Bacon was fascinated by the progressive nature of science and, especially, mechanical arts, as opposed to immobility of philosophy which had abandoned itself to the error, even the madness, of believing that truth appears to the human mind from within and not through the senses. He also understood, and this is the most innovative aspect of his

[109] F. Bacon, *Novum organum scientiarum,* Venetiis 1762, pp. 54, 64.

thinking, that scientific research has a social importance and must work towards changing human conditions. In the *De dignitate et augmentis scientiarum,* first drafted in English in 1605 and hugely enlarged in the Latin edition of 1623, Bacon outlines all previous science and then asks what is still missing for science to make more rapid progress and achieve a wider and more secure possession of nature. According to Bacon, one of the main things missing was a *novum organum*, that is, a new logic or method for the study of natural philosophy in place of Aristotle's *Organon,* become inadequate. He provided one himself in the *Novum organum scientiarum,* published incomplete in 1620.

Every treatise on the history of philosophy contains an extensive description of Baconian logics and methods. It remains for us, therefore, to recall the essential elements. Cancelling out the prejudices, or *idola,* as Bacon calls them in his imaginative language, the scientist must collect the observed data in tables giving the presence, absence, level (that is, intensity) from which he will try to find the "form", a term of obscure meaning that may indicate the essence of things: for example, an inductive search by Bacon on the nature of heat leads him to conclude that movement is the form of heat. From the form, the scientist could proceed to the *vindemiatio prima,* or first harvest, that is more or less what we would call a working hypothesis. The first harvest must be subjected to various experiments that prove or disprove it.

It may be that Bacon did not intend his new logic to establish a method for particular scientific studies but rather the ordering of all knowledge in a single encyclopedia of the sciences. Notwithstanding his unitary ideal, he had a lively sense of the concrete, the particular, the borderline case. It is, in fact, consideration of the borderline case that led him to the theory, with regard to the experiments to prove *vindemiatio prima*, of the *experimentum crucis* - a picturesque expression suggested to him by road signs that places a cross at the intersection of roads to indicate the direction of the two paths[110] believed possible in physics up to the XIX century, despite Bacon's fallacious application to tides and the world system.

No physicist has ever applied the Baconian method, not even Bacon himself, and the experimentation was extremely limited and scientific interest almost non-existent, except for some rare examples such as experiments on the incompressibility of water[111] and on vibrant cords.[112] On

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[110] *Ibid.,* p. 34.
[111] *Ibid.*, p. 45.
[112] F. Bacon, *Sylva sylvarum, sive historia naturalis,* Lugduni Batavorum 1648. The work, appearing posthumously in 1627, contains ten *centuriae* of experiments. The second *centuria* and most of the third are given over to acoustics.

the contrary, in comparison to the science of the time, Bacon appears as a conservative. For example, he says that Copernican beliefs may be confuted by the principles of natural philosophy *recte positis*, and by proposing them *recte* he concludes that the Earth is still; he believes the Galilean astronomical observations to be a little doubtful due to the limited number of discoveries obtained with a telescope; he affirms the tendency of bodies to move upwards or downwards on the texture of the bodies and their sympathy for other bodies. And so on so forth, always ready to grasp the chance to demolish or ridicule the science of preceding centuries.

Bacon had no influence on contemporary scientists: Galileo and even Newton ignored him, and Descartes barely mentions him. His fame grew in the second half of the XVII century, with Robert Boyle, Robert Hooke, Christopher Wren and the Royal Society that was established in 1660 with the clearly stated intention of continuing Bacon's work. But it was above all French Illuminism - Jean-Baptiste D'Alembert, Denis Diderot, Voltaire - who feted the "chancellor of England and nature" in a hyperbole that no modern historian of science could justify, even in part.

Bacon was certainly not a scientist, he was a philosopher, a broadcaster of science and a herald of its value to social and human liberation; he promoted scientific progress not by the example of original research, nor an analytical tool, but with the eloquence of a fascinating writer who shouted: Study nature!

4. Galileo Galilei

GALILEO AT THE PISAN *STUDIUM*

4.1 Early years, isochronism of pendular oscillations

Galileo Galilei (Fig. 4.1) was born in Pisa on 15 February 1564, the first of seven children to Vincenzo (1520-1591) of Florence, a musician by vocation and a trader through necessity.

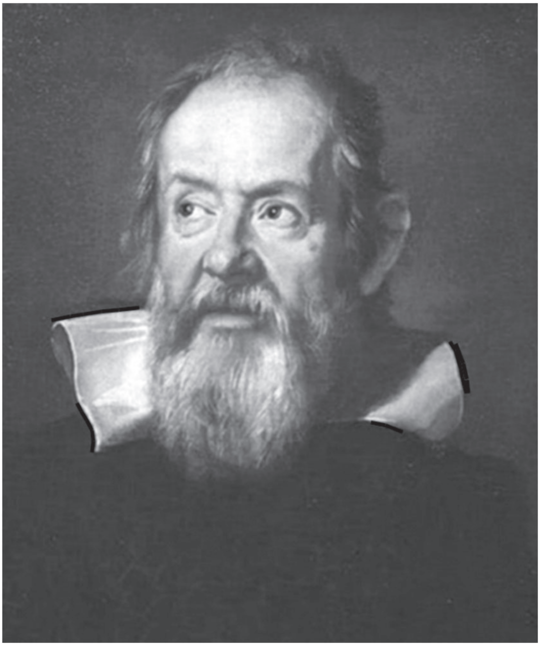

Fig. 4.1 - Galileo Galilei. Portrait by Justus Sustermans (Florence, Uffizi Galleries). *Source*: Alinari

When Galileo was ten years old, the family moved to Florence where he continued his early humanist education. He returned to Pisa in 1581 as a medical student, but in 1583 the mathematician Ostilio Ricci, already a follower of Tartaglia and a friend of the Galilei family, initiated his studies of mathematics. It was revelation for the young man and he abandoned medicine to study mathematics.

In his *Racconto istorico della vita del Sig.r Galileo Accademico Linceo, Nobil fiorentino* (XIX, 597-932),[113] Vincenzo Viviani, a student who lived with the master in Galileo's final two years, says that in 1583 Galileo observed an oscillating lamp in Pisa Cathedral and discovered the law of the isochronism of pendular oscillations. Many believe Viviani's version to be a legend (Cardano had begun the study of pendular motion: § 3.13) and in fact all Viviani's history of Galileo's early years smacks of the legendary.

All the same, Viviani heard the tale from Galileo himself and, leaving aside any embellishments he may have introduced, it seems impossible that the story does not contain a modicum of truth because Galileo deals with the law of isochronism in both *Massimi sistemi* and *Nuove scienze*. Furthermore, in the latter dialogue, Salviati, who represents Galileo himself, recalls the oscillations of church lamps: "I have studied vibrations a thousand times, especially those of lamps hanging in churches from long cords inadvertently moved by someone" (VIII, 140).

4.2 Early works

In 1585, Galileo returned to Florence without having finished his medical studies. In the fertile cultural climate of the city, he furthered his literary, philosophical and mathematical learning. His invention of the *bilancetta* ("little balance") (§ 1.6) perhaps dates to 1586, and the discovery of some theories on centres of gravity are probably from the same period, or soon after. These are Archimedian arguments revealing Tartaglia's influence on the formation of the young scientist.

The two works, unpublished but circulated as manuscripts, brought Galileo to the attention of contemporary mathematicians and the particular esteem of Guidobaldo Del Monte, whose intercession procured Galileo's first position as chair of mathematics at the Studium of Pisa. The young man gave his opening lecture on 12 November 1589.

In this new environment, Galileo turned his studies to the central question of physics: motion. Some traces remain in the short treatise "*De motu*" written around 1590, and a dialogue in Latin between *Alexandre* and *Dominicus*. *De motu* is influenced by Benedetti (§ 3.14) but Galileo draws the theory of impetus, rejecting the Aristotelian theory of violent

[113] Hereafter we will cite, according to common usage, the national edition of the works of Galileo Galilei: the first number indicates the volume, the second the page.

movements. However, even in this first work, Galileo refutes that bodies have their own lightness that tends towards a state of rest, that air instead of resisting, contributes to movement. Ironically, he says that Aristotle's declaration that the velocity of the fall of bodies is proportional to their weight is "ridiculous", and supported by Benedetti's demonstration, that he simplified, he states that with the same medium, the weights of the same material, even if of different weights, fall at the same speed, making frequent reference to experiments *ex alta turri* (from a high tower). To conclude, the *De motu* provides research that unites Medieval thinking with the philosophy of Archimedes, the result of profound studies of the mathematician that Galileo admired more than anyone else.

The mention of the *ex alta turri* experiments seems to confirm Viviani's story that Galileo verified the equal velocity of falling bodies through experiments at the Tower of Pisa, carried out with great solemnity in the presence of Aristotelian colleagues and students.

These experiments, too, that probably date to 1590, have often been doubted since Alexandre Koyré, with his detailed studies of Galileo (1939), took it upon himself to demonstrate Galileo's Platonism. Now, it may be that Viviani exaggerated a little and made some mistakes over dates, writing the *Racconto* thirteen years after the death of Galileo, but there is no serious reason to doubt the facts. Besides mentioning it several times in *De motu*, Galileo was "convinced by reason" (VII, 730) of the result of the experiment, and finally Galileo, still alive, began almost a tradition of experiments of falling bodies from a tower. It is said that Stevin did the same in Delft perhaps in the last decade of the century, but it is certain that Giovan Battista Baliani repeated them from the top of the rock of Savona in 1611, as did Vincenzo Renieri, already a follower of Galileo in Pisa, in 1641 from the Pisan bell-tower, Giovan Battista Riccioli and Nicola Cabeo from the bell-tower of the Church of Gesù in Ferrara, Riccioli again, in the presence of Francesco Maria Grimaldi, from the Asinelli tower in Bologna in 1640, 1645, and 1648.

To conclude, the doubts of modern commentators about the historical truth of the Pisan experiments seem unfounded.

GALILEO IN THE STUDIUM OF PADUA

4.3 The science of mechanics

But the hostility of the Pisan academic world to the open-minded young professor, and his increased financial difficulties after the death of his father, forced Galileo to look for a better position, which he obtained, thanks to the help of Del Monte, with a chair at the *Studium* of Padua where he remained for eighteen years, from1592 to 1610, the most serene and productive years of his troubled life.

At the time, the *Studium* of Padua was divided into two universities: jurisprudence and arts. The latter, of which Galileo was a member, was comprised of theologians, philosophers, and medical doctors. The majority of Galileo's audience was made up of students of medicine who, after learning a little geometry, passed on to the study of astronomy, necessary to start astrology, a subject that each doctor, for his decorum, needed to know, or at least claim to know. From the few surviving scrolls of the *Studium*, we know that the public courses held by Galileo were on Euclid's *Elements*, Giovanni di Sacrobosco's (John of Holywood's) *Sphere*, Ptolemy's *Almagesto*, and Aristotle's *Mechanical problems.*

He read the last of these in the 1597-98 scholastic year. The title was traditional, but most probably, the scientist expounded the results of his own Pisan research and the new speculations that he added. In this period, perhaps to help his students, he wrote the treatise *Della scienza meccanica, e delle utilità che si traggono da gl'istromenti di quella* (On mechanical science and usefulness of its tools) a handwritten lecture published for the first time in 1634 in a free French translation by Mersenne entitled *Les mecaniques*. The treatise expounds the theory of simple machines.

By introducing the concept of "moment" of a force with respect to a point, Galileo began a treatment that, even without today's mathematical precision, allowed him to alter the traditional definition of the centre of gravity of solids with the more recent provided by Federico Commandino as the point around which there are parts of equal moment, therefore it may be supposed that "it is the seat of each impetus, of each weight, finally of each moment" (II, 159-60).

Galileo was not yet using the principle of the decomposition of forces and drew on the works of Del Monte to reduce the equilibrium of simple machines (levers, wheel axes, capstans, hoisting tackles, inclined planes, screws) to that of the balance, already known to Aristotle. But while Del Monte, copying Archimedes, gives prolix geometrical demonstrations, Galileo's treatment is new, brief and elegant.

More in detail, the short work sets out in explicit and accurate way, without generalising, one of the most prolific principles of modern mechanics, virtual works, already elaborated by previous writers like Cardanus. Galileo's statement had become almost proverbial: "As much force is gained by them [that is the mechanical instruments] the same is lost in time and velocity" (II, 185). On the contrary, his immediate predecessor, Del Monte, wrote "The easier it is to move the weight, the greater is the time, and when it is more difficult, the less the time, and vice versa".[114]

We can be sure that it was in the Pisan period that he wrote about the isochronism of pendular oscillations, studies of magnets (§ 3.20) and dynamic discoveries, which we will describe later.

We will not deal here with the mathematical or astronomical studies, without mentioning that they were basic to Galileo's scientific training. Possibly, the desire to find dynamic proofs in support of the Copernican system induced Galileo to study dynamics in depth.

We do not know when the scientist was converted to Copernican thinking. In 1597, in a letter to Kepler, he said that he had been a believer in Copernicus for many years. Consequently, some historians claim that in his Pisan years he was Copernican, perhaps because of the influence of Benedetti, a firm follower of Copernicus. All the same, in order to live a quiet life, Galileo continued to teach Ptolemaic astronomy. It was only in 1604 that he came out publicly in support of the Copernican system.

4.4 The thermoscopic experiment

Especial mention should be made of the thermoscopic experiment, also dating to the Paduan period around 1597. It is important not only for the long arguments that it caused over the priority of the invention of the thermometer, but for the new (anti-perypathetic) thinking that produced its conception and application.

Basically, the experiment dates back to Philo, and had recently been diligently described in chapters 32-33 of Benedetti's *Diversarum speculationum* and book XIX of Porta's *Magia*. Galileo presents it in this way: a carafe as big as an egg, with a long neck as narrow as a wheat stalk is warmed between the hands, and then the mouth is immersed in a vase full of water (Fig. 4.2); withdrawing the heat of the hands from the carafe, the water in the vase rises into the neck as the carafe loses heat. "Deriving from this effect -writes in 1638 Don Benedetto Castelli (1577-1644), a follower of Galileo- Master Galileo constructed an instrument to examine the degrees of heat and cold" (XVII, 377).

[114] G. Del Monte, *Mechanicorum libe*r, Pisauri, 1577, c. 105*r*

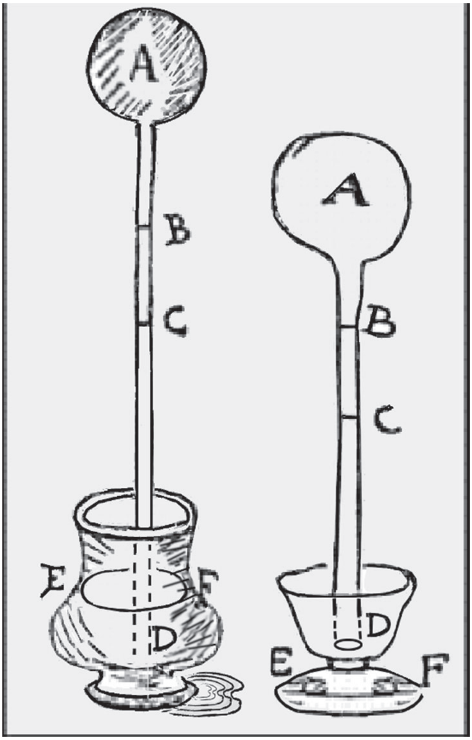

Fig. 4.2 -Representation of Galileo's thermoscopic experiment
Source: Galilei, *Works,* op. cit. Vol. 17.

Besides the degrees of hot and cold, the instrument was influenced by atmospheric pressure that was not understood at the time. But it is not this detail that is important, nor the question of priority. What is significant is that the experiment does not remain a simple curiosity, as with previous writers, but was used as a measuring tool. Galileo not only trusted in the new instrument but believed its readings to be more objective that our sensations. In summary, he teaches that physics needs tools, and this concept is truly revolutionary compared to the shared philosophy of the time.

4.5 The reinvention of the spy-glass

Around the end of 1608 and early 1609, rumours circulated in Venice of the invention of the spy-glass by someone from "across the Alps". The first examples began to arrive after a few months and they could be bought "for little money". In May 1609, Galileo was in Venice and was given the information by a former student from Paris. He discussed it with his Venetian friends and it may be, although he never admitted it, he procured an example.

Now, Galileo's preparation in the science of optics was modest, to say the least, limited to classical ideas, although in a letter of 24 August 1609 in which he presented the Doge Leonardo Donato with a spy-glass, he claimed to "have discovered it from the most hidden prospective speculations". In his ignorance, Galileo had the advantage of not having the same adversity to the instrument as the specialists, who really knew how many things the lenses would show that really do not exist.

In the first week of July 1609, Galileo constructed his own first spy-glass, as he famously recorded in the *Saggiatore* (The Assayer), remembering how he came to know about the instrument, the improvements he made to it and the subsequent successes. He continues boasting about the reasoning that led him to the re-invention: "That was therefore my discourse. This instrument may be made of a single glass, or several. A single glass does not work because it is either convex, that is larger in the middle, or concave, thinner in the middle, or contained in parallel surfaces: but this does not alter at all the visible objects by enlarging and decreasing them; concave decreases them, and convex enlarges them, but shows them as indistinct and unfocused; therefore, a single glass is not enough to produce the result. Using two, and understanding that a glass with parallel surfaces changes nothing, as we have said, I concluded that the result cannot be achieved without combining one with one of the other two. This forced me to experiment what would happen with the composition of the other two, that is the convex and the concave, and I saw that this gave me the result" (VI, 259).

Where are these "most recondite speculations on perspective"? There is only the ingenious man who believed in the possibility of artificially increasing the senses acuity, who possessed considerable manual skills, and was fortunate to live in a glass-making centre. All these circumstances allowed him to make rapid progress on the instrument with success after success that he eloquently described in the *Sidereus nuncius*: "I first prepared a lead tube and attached two glass lenses to each end, both with a flat face at one side, but one convex and one concave at the other; by looking through the concave glass I saw objects much larger and closer; in fact, they appeared three times closer and nine times larger than when observed with the naked eye. Shortly after, I constructed another more precise instrument that enlarged the objects observed more than sixty times. Finally, without sparing time or expense, I built such an excellent instrument that the observed objects were enlarged almost one thousand times and more than thirty times closer that when viewed with the naked eye" (III, 60-61).

This passage contains the fundamental merit of Galileo in the introduction of the spy-glass: the patient experiments, and the progressive technical improvements of the instrument that Galileo achieved through a precise working of the surfaces of the lenses, an art learned at the Murano glass-work of his friend Girolamo Magagnati. An almost pointless toy (§ 3.17) had been changed by Galileo into a scientific instrument.

The triumph of the spy-glass was not, however, simply a technical question. It also arose from Galileo's increasing belief in it and its veracity. He checked this in numerous experiments, as he wrote to a friend in May 1610, through "hundreds of improvements on thousands and thousands of objects, near and far, large and small, shiny and dark" (XI, 106).

Faith in the veracity of the spy-glass, and in general in the possibility of instruments to improve our perceptions marked a revolution in the scientific thinking of the time that was still strongly rooted in old Scholastic prejudices. And it is perhaps the importance of this new attitude to science to which Galileo alludes, in a polemic note that was not to be published, when he writes: "You say that everyone has eyes, and there are many spy-glasses, therefore a lot of people may observe etc. But you do not realise that so much more praise is due to me and the reproof of others, who would be pardoned only if I alone had been helped by eyes and spy-glasses" (VI, 383).

For perhaps twenty years, the spy-glass was forgotten; after ten months or less in the hands of Galileo it became the protagonist of modern science. Galileo was absolutely right to call it his "little son".

After looking at the skies through his spy-glass, and making some memorable astronomical observations, which we shall not dwell on here, on 30 January Galileo rushed to Venice to publish a pamphlet announcing his discoveries to fellow scientists: the *Sidereus nuncius* appeared on 12 March 1610.

GALILEO IN ARCETRI

4.6 Conditions of floating and the weight of air

The fame Galileo achieved with the *Sidereus nuncius* led him to be appointed professor of mathematics at the *Studium* in Pisa. He was required neither to be resident or give lectures: basically, it was a post in the court of the Grand Duke but without any special rank. The court mathematicians were more or less servants. Galileo took up residence in Arcetri, in the hills above Florence, where he continued his astronomical observations and studies in physics. It was here, also, in 1612, that he drafted his first essay with the *Discorso intorno alle cose che stanno in su l'acqua o che in quella si muovono* (Talk about things that float on water or move in it).

The *"Discourse"* is based around floating. From the observation of apparent floating, that we now know is due to surface tension (apparent floating of a knitting needle or a thin ebony tablet placed softly on the free surface of water), the peripatetics deduced that the floating or sinking of bodies in water depended not only on their specific weight, but also their form. On the contrary, Galileo adhered to the general validity of Archimedes' theory, arguing that the shape of the bodies had no influence on the phenomenon. Having understood that phenomena of apparent floating were singular, he interpreted them by positing that bodies supported on the surface of water were resting on a cushion of air, with this constituting an element specifically lighter than water. In this *Discourse,* the principle of virtual work reappears as an essential element of the treatise, that, amongst other things, allows Galileo to anticipate Pascal with the observation that a small mass of water contained in a "narrow straw" may balance a large mass of water in a "wide vase", connected with the straw (IV, 77-78), already explained by Benedetti (§ 3.2).

The *Discourse*, full of ingenious, clearly explained and brilliantly interpreted experiments, had a great effect and was followed by angry replies from Galileo's adversaries, to which he made answer his faithfull don Benedetto Castelli.

During this lively argument, Galileo was led to test the weight of air. In a part of *De Coelo*, Aristotle explicitly mentioned the weight of air, but Simplicious, the Greek philosopher's commentator, thought he should correct it: the peripatetics followed Simplicious and for centuries it was taught that "pure" air is weightless. As we have seen (§ 3.13), Cardano, and maybe Benedetti, rejected the traditional interpretation. In a letter of 1613, Galileo describes the first qualitative experiment: a weighted flask, with a long, narrow neck, sinks less in water when it has been heated to remove most of the air. In a Latin fragment of uncertain date (VIII, 636), the method was made quantitative, but in a letter of 1614, Galileo stated that "if he remembered well", he measured the specific weight of air as 1:460 with respect to water with another method, expounded many years after in the *Discourses*. This (and a similar one described in the same work) is based on a brilliant observation, precursor to the introduction of the concept of elasticity, that "air suffers from being very condensed"; consequently, the specific weight of air may be obtained when one measures on one hand the increase in weight of a flask into which more air has been forced in addition to that it naturally held, and on the other, the volume that the air introduced occupied in its ordinary condition (VIII, 122-26). The thermal method is no longer mentioned, perhaps because Galileo saw heating as a disturbing element that could falsify the results. Mersenne changed the thermal experiment into another that is still used in schools, verifying the decrease in weight of a heated balloon, or, as more easily done nowadays, by emptying the balloon of air using a pneumatic pump. The value of the ratio discovered by Galileo using compression was 1:400, almost double the real value (1:830). But after Galileo, and copying his methods, ratios were discovered that were less approximate than his: Mersenne gave two values, 1:255 and 1:879; Descartes, 1:145; the Academy of the Cimento, 1:1438; Giovan Battista Borelli, 1:1179. To define a better value than Galileo's, we have to wait until Boyle, who set it as 1:938. But Boyle was working in 1661, almost half a century after Galileo, and emptied his recipients using a pneumatic machine: hard work! The Galilean measure has the merit of being the first idea of the dimension of the specific weight of air, thereby originating the consideration of atmospheric pressure (§ 5.16).

4.7 Science and faith

As soon as the *Discourse* on floating objects appeared, Galileo was involved in another argument with the Jesuit priest Christoph Scheiner (1579-1650). In 1613, the Academy of the Lincei published three polemical letters by Galileo, collected in the single volume *Istoria e dimostrazioni matematiche intorno alle macchie solari e loro accidenti* (History and mathematical demonstrations of sunspots and their occurrences). The principal point of the new argument was sunspots. Scheiner sustained that they were planets orbiting around the sun (he later changed his opinion), thereby upholding the Aristotelian principle of the unchangeable nature of the skies. Galileo, on the contrary, believed them to be alterations, comparable to our clouds, that occurred on the surface of the Sun. The importance of the *Istoria* is not so much the above-mentioned study of sunspots and the numerous observations of scientific methodology (experimentation, need for definitions, the conventional but not arbitrary use of scientific language, refute of the principle of authority, etc.), but above all the open defence of the Copernican system, enriched with a new discovery. Starting from the apparent motion of the spots, Galileo deduced that the Sun rotated, in an orbit completed in a lunar month, around its own axis which was inclined on the ecliptic plane.

The open defence of the Copernican system attracted the attention of philosophers and, especially, theologians. Paradoxically, Galileo's discoveries fed and angered the opposers of Copernicus. In December 1613, there was a repercussion in the court of the Grand Duchy, at the time resident in Pisa. Galileo was informed of the discussion given by Castelli, to whom he addressed the famous letter of 26 December 1613 that was the primary cause of the storm that broke over his head. This was followed by the two letters to Don Piero Dini, dated 16 February and 21 March 1615 respectively, and the more famous one to Madama Cristina di Lorena, grand duchess of Tuscany, whose exact date is unsure, but certainly was written in 1615.

The handwritten letters, especially the first and fourth, had great diffusion and resonance. The fundamental argument of the letters is the boundaries between science and faith. By delimiting these boundaries, Galileo hoped to stop the condemnation of the Copernican system and give Copernican thinking increasingly wide acceptance by specialists, cultured people, and the ecclesiastic hierarchy.

Basically, the letters elaborate the theory, that was very dear to him, that nature is a manifestation of God, like the Scriptures, but the nature has unchangeable laws lain down by God, and the Scriptures are verbal expressions suited to man's common intelligence. Therefore, as no antagonism can exist between the two forms of disclosure, Scriptures must be interpreted in the light of natural laws.

The doctrine, that substantially gave preminence to scientific knowledge over any other knowledge, was bound not to please the vast majority of ecclesiastic opinion. In particular, it certainly did not please the Dominicans who vehemently attacked Galileo. He strongly defended himself, in the conviction that the Copernican system constituted the break with the Aristotelian tradition and in the hope of pushing the Church, for which he had a profound respect, towards a favourable attitude to new scientific horizons.

Denounced to the Inquisition, the Holy Office opened an inquest that concluded on 24 February 1616 with the condemnation of two propositions: that the Sun is the centre of the world and does not move, deemed to be formally heretical; that the Earth is not the centre of the world and is not immobile, judged to be against faith. Following the sentence, the Congregation of the Index prohibited books that taught the Copernican doctrine. Galileo was informed privately of the sentence by cardinal Roberto Bellarmino, who, on the orders of the Pope, advised him to abandon the censured opinion, adding - according to a document judged apocryphal by some historians and authentic by others - that he must abstain from defending or discussing or teaching in any way (*vel quovis modo docere*) the Copernican theory.

4.8 Il saggiatore (The Assayer); primary and secondary qualities

The sentence forced Galileo into silence, which he broke in a new argument with the Jesuit Father Orazio Grassi (1590-1654) over the appearance of three comets in 1618. Galileo replied to Grassi's bitter essay with *Il Saggiatore*, patiently prepared over three years and published in 1623 by the Academy of the Lincei.

The Assayer, a jewel of Italian polemics, was appreciated by many people, including the Pope. It is not an astronomic treatise as the nature of comets is not the basic argument, but only the starting point. Of all Galileo's masterworks it is the one with the least scientific content. All the same, its importance was enormous in the evolution of scientific thinking: it almost assumes the form of a "manifesto" for the new experimental mathematical approach, a declaration of war against the principle of authority.

The Assayer touches on virtually all of the questions of physical research of the time, such as the function of mathematics in research into the laws of physics, that gave rise to numerous different interpretations of Galilean philosophy; the magnification of spy-glasses; the need for exact definitions of terms to avoid uncertainty over some common words, where "it is assumed as absolute what can be considered without a relation" (e.g. "near" and "far", "large" and "small"); the concept of cause; the relation between the height of organ pipes to their sound or the strings of a harp and their length; the nature of heat; the distinction between primary and secondary qualities.

The distinction between primary and secondary qualities, as they will be classified by Locke, that some critics accused Galileo of originating dualist philosophies, is a fundamental and distinctive feature of Galilean physics, already proposed by Democritus (§ 1.2). The passage in *The Assayer* where Galileo repeats the Democritan idea is well known but bears repeating here: "I feel it necessary to state that I see a matter or a body in the sense that it is represented in this or that figure, that, in relation to others it is big or small, that it is here or there, in this time or another, that it moves or stays still, that it touches or does not touch another body, that it is single, few or many, and no force of imagination can separate it from these conditions; but should it be white or red, bitter or sweet, noisy or dumb, of a pleasant or evil odour, I do not have the strength of mind to assert that these conditions necessarily accompany it: on the contrary, if the senses were not to play their part, perhaps the imagination would never arrive at the conclusion. Therefore, I believe that these tastes, smells, colours, etc, are subjective and are nothing less than names and reside solely in the feeling body and, removed from the animal, these attributes are dimissed and cancelled" (VI, 347-48).

And to explain the concept better, Galileo immediately used examples of tactile sensations (that we feel and are not in the body we are touching); of smells, tastes, sounds, that "outside the living animal are nothing but names" and lastly also "heat" that is what modern terminology calls temperature, that Galileo believed to be a phantasm of the senses: "I'm more inclined to believe that heat is of this type and those materials that produce and make us feel heat, to which we give the generic name of fire, are a multitude of minimal bodies and, moved by more and more velocity, hitting our body, penetrate it because of their extreme smallness, and touching us, or passing through us, as we feel them, create the sense that we call heat, pleasant or harassing according to the greater or lesser number and velocity" (VI, 351).

We have not yet arrived at the kinetic theory of heat, because for Galileo "the minimal bodies" were the particles of fire, not the molecules of the elements, and this belief was the root of his diffidence to the thermal method of determining the weight of air. All the same, a first step had been taken to the idea of kinetics that would be taken up, as we shall see, by others and would be proved in the XIX century.

4.9 Dialogue Concerning the Two Chief World Systems

With the election of Pope Urban VIII (1623), who was his friend and supporter when he was Cardinal Maffeo Barberini, and after the publication of *Il Saggiatore*, Galileo believed the moment had come to take up the Copernican battle again. He started cautiously by writing a letter to Francesco Ingoli, in reply to the *Disputatio de situ et quiete Terrae*, that Ingoli had sent him in 1616. The handwritten letter, finished in September 1624, is a small Copernican treatise of great didactic value, in addition to being a fundamental document in Galilean dynamics, which we shall deal with later.

The warm acceptance of the letter, read and appreciated also by the Pope, and other favourable circumstances, gave Galileo the not unfounded hope of returning to his work on the chief world systems, begun by both the *Sidereus nuncius* and the *Discourse* on floating objects. He started work in 1624 but progressed slowly. In December 1629, the work was finished and after some difficulties was finally published in 1632 with the title "*Dialogue …Concerning the Two Chief Ptolemaic and Copernican World Systems"*.

The dialogue -a literary form preferred by Galileo because of the ease of introducing digressions- is supposed to have taken place in Venice many years earlier between three people: two friends of Galileo, already dead, Filippo Salviati and Giovan Francesco Sagredo, and Simplicio (Simplicius), an imaginary character who is named after one of the most famous commentators of Aristotle. Simplicius is the obstinate peripatetic who argues against the professors of the Paduan and Pisan *Studia*, a zealous defender of Aristotle to the point of ridicule and subjected to the brilliant irony of Salviati and Sagreto. Salviati impersonates Galileo and Sagredo is the voice of culture and common sense, called upon to act as judge between the two philosophies.

The *Dialogue* is divided in four days. The first contains some interesting digressions (the presumed perfection of the number 3, the infinite steps of velocities assumed by a body that begins to move; sun spots; the reflection and diffusion of light; moon light; human knowledge and divine knowledge) and rebuts the Aristotelian thesis of the impossibility to generate, change or alter celestial bodies, introducing the concept that the Earth is a moving body and orbits like the Moon, Jupiter, Venus, and the other planets. According to Galileo, the most obvious proof comes from new stars and sunspots. Simplicius replies with peripatetic arguments: sun spots do not originate from the surface of the Sun but are obscurities caused by opaque bodies orbiting around the Sun.

The mountains of the Moon demonstrate that the physical make-up of our satellite and therefore, by analogy, of other heavenly bodies, is similar to that of the Earth. But Simplicius will refute the mountains of the Moon, claiming that Moon shadows are caused by its more and less luminous parts.

In the second day, Galileo, using the laws of new mechanics (the principle of inertia, the composition of simultaneous motions, the principle of relativity, the laws of the falling of bodies) demonstrates that there is no basis for the Peripatetic arguments against the movement of the Earth (§ 4.14).

The third day opens with a long digression on the *stella nova* of 1604. The Dialogue continues with an exposition of the world systems, a truly masterly explanation, deliberately reduced to a basic idea - the Sun at the centre, the planets orbiting around it in a circular movement - that contributed, maybe more than any other picture, to the popularisation of the heliocentric system. The phases of Venus, Jupiter's moons, sunspots are fundamental arguments in support of the Copernican system.

The subject of the fourth day is the "*ebb and flow of the sea*", that is tides, the principal subject of a *Discourse* of 1616, disseminated as a manuscript. Galileo wrongly believed that tides were the solid proof of the Earth's motion. Let us take, he says, a tanker carrying fresh water to Venice. If the speed of the ship changes, because of inertia, the water will move aft or forward: Earth is like the tanker, the sea is like the water it is carrying; the non-uniformity in motion is due to the composition of Earth's daily and annual movements.

Even though Galileo was aware that recently De Dominis and Kepler, not to mention Porta, had suggested that tides were caused by the attraction of the Moon and the Sun, he declared the theories to be "frivolous". Before being astounded by, or condemning, Galileo, it is necessary to remember the period and understand his thinking. This action of the Moon and the Sun, this "*prensatio*" or "*vis prensandi*", described by Kepler, this "force" or "attraction" as Newton would later call it, seems to give back to the celestial bodies all the occult properties dear to the Peripatetics and so strongly opposed by Galileo.

But, coming back to tides, Galileo must have had some doubts about his theory, which he thought about for at least fifteen years, as Sagredo, at the end of the discussion in the Dialogue, confesses that his mind "is still blear by the novelties and difficulties" (VII, 487).

The publication of the Dialogue is a milestone in the history of human philosophy. It is not really an astronomical treatise, not an essay on physics, but a pedagogic work aimed against the teachings of Aristotle and destroying the authoritarian principle. It is a work of cultural propaganda in support of the new image of the world brought about by Copernican ideas, the framework for scientific research in the century.

4.10 The Second Trial

The publication of the *Dialogue* was met with the vast and enthusiastic consensus of the "free geniuses". Bonaventura Cavalieri devoured it with the same joy he had on reading *Orlando furioso* (XIV, 336); Castelli "could not stop praising it" (XIV, 361); Fulgenzio Micanzio immediately wrote to Galileo that he had "discovered the heart of nature" (XIV, 364); Campanella, even if of dubious Copernican beliefs, declared "These new versions of ancient truths, of new worlds, new stars, new systems, new nations, etc. are the foundation of the new century" (XIV, 367).

The reception by the Holy See was very different. Urban VIII had authorised Galileo to write a dialogue impartially setting out Ptolemaic and Copernican reasoning. Galileo might also go as far as proving the validity of the Copernican system, as long as it concluded that it would "limit and constrain divine power" if the Copernican system was pronounced certain, because God could have constructed the world "in many ways, even inconceivable to our intellects". Galileo had followed the Pope's directive. He submitted his *Dialogue* to the censors in 1613 and modified it according to their wishes and had received their approval. He had also added, at the end of the work, Urban VIII's argument on divine power, but he unfortunately made Simplicius set out the argument: a fatal error.

Galileo's enemies, for the most part those against the new science, were quick to take advantage. They convinced the Pope that Galileo had insolently made him look ridiculous by using the weak character of Semplicius. The old friendship of Cardinal Barberini changed into the ire of Urban VIII, who was also forced to give a show of strength in the difficult political situation of the Counter-reformation.

The Inquisition was mobilised and in October of the same year - 1632 - called Galileo to the Holy Office in Rome. Attempts to resist the injunction proved useless and Galileo's presence was strongly demanded, despite his advanced age, ill health and the presence of the plague. Faced with such severity, even the Grand Duke abandoned his "leading mathematician and philosopher" who had no choice but to obey the order. Galileo arrived in Rome on 13 February 1633.

But what was Galileo accused of since his *Dialogue* had appeared with the approval of the Church? Galileo was officially informed on 12 April, in the first interrogation by the commissioner priest of the Holy Office: he had betrayed the precept communicated to him in 1616 by Cardinal Bellarmino, who had since died, to abstain in future from dealing with *quovis modo*, the Copernican system and to have refrained from mentioning this in asking permission for the *Dialogue*. Galileo protested energetically that he did not recall having received any warning from Bellarmino.

But, tricked by the cunning of the inquisitors, Galileo began to make half admissions, to contradict himself, and to admit that some parts of the *Dialogue* appear Copernican, even while protesting that he had gone beyond his intentions, led by the pleasure everyone takes in seeming cleverer than others "also using false and ingenious propositions, and apparent arguments of probability" (XIX, 343). In the end, in the written disposition presented on 10 May, he confessed to having received Bellarmino's precept in 1616 but have completely forgotten the phrase *vel quovis modo docere.* as he believed himself "excused from not having notified the Master Father of the Holy Palace of a private precept" (XIX, 346).

On 22 May 1633, Galileo, kneeling before the Congregation of the Holy Office, as was the custom, had to "abjure, damn and swear against his errors and heresies". Sentenced to prison, he was first locked up in Rome, then in Siena at the home of his friend Archbishop Ascanio Piccolomini, and in late 1633 in his villa in Arcetri that would serve as his prison and where he was forbidden to receive unauthorised visitors.

But the torments of the aged scientist were not finished: in April 1634, his beloved first daughter, the sweet Sister Maria Celeste, who had lovingly comforted him during the trial, died. The harsh isolation in Arcetri - where the ban on receiving visitors was rigorously respected - was compounded by a malady that made the scientist completely blind.

4.11 The "Discourses and Mathematical Demonstrations Concerning the Two New Sciences"

In an incomparable show of fortitude, Galileo managed to rise above the spiritual depression into which he had fallen after the trial. Already during his stay in Siena, in the summer of 1633, he had started writing his most important work, promised at the end of the *Dialogue*, that would co-ordinate and unite in a single work the speculations on mechanics that had occupied his entire life.

Before even finishing the work, the scientist began negotiating its publication, which was difficult: the Papal Court still banned the printing of any work by Galileo, published or unpublished. After attempts in Venice, Austria, Germany and France, he managed to come to an agreement with Elzeviri in Leiden. Around mid-July 1638, the first copies started to appear under the title of *Discourses and Mathematical Demonstrations Concerning the Two New Sciences of Mechanics and Local Motion*. The long title was proposed by the publishers and Galileo was not happy with it, claiming it was "too vulgar, not to say plebeian" (XVII, 370).

The *Discourses* also take the form of a dialogue, again divided into four days. Galileo had the intention of adding another two of lesser importance: one, today known as the "sixth day" would have treated the "force of impact" and a draft dialogue has survived; the second, now called the "fifth day", dictated by Galileo to Evangelista Torricelli between October 1641 and his death, has also survived in draft form. The additional days were published only in 1718.

The participants in the *Discourses* are again Salviati, Sagredo and Semplicius (replaced in the sixth day by Paolo Aproino, a follower of Galileo). But Semplicius seems to have changed in character, he is more understanding, accommodating and more intelligent. As a result, these *Discourses* lack the polemics and sarcasm of the *Dialogue*.

The first two days are given over to the resistance of materials. An issue of engineering led Galileo to deal with the new science: if two machines are geometrically alike and built of the same material, the larger will be proportionally weaker than the smaller. Therefore, the robustness of the machine is influenced by something that is not purely geometric. Consequently, the first day is dedicated to a study of the coherence of solids, that is the structure of the matter: continuity and discontinuity, empty and full. Galileo is therefore provided with the opportunity of giving his full support to the Democritean atomism in some wonderful pages that constitute the birth of modern scientific atomism. There are several digressions in the first day and we will return to their particular relevance to physics in later paragraphs.

The second day is more technical, almost practical. Starting from classical theories of statics it moves away to the resistance of fixed or wedged beams. The science of the time lacked too many notions (moment of inertia, Hooke's law, modules of elasticity, etc.) to conform to modern results. But it is admirable that Galileo almost seems to have understood this problem by intuition and do not try to find the resistance of one beam but the ratio of the resistances of two beams of the same material, obtaining rules useful for practice and demonstrating an uncommon ability in applying geometry to practical problems.

The third day and the fourth deal with the second of the new sciences announced in the title: dynamics, that had already been the subject of a digression in the first day. There is a renewed and systematic analysis of the questions of dynamics that were used in the *Dialogue* to prove Copernican thinking. Consequently, the *Discourses* are also a Copernican work. In these days, Salviati reads a Latin treatise of "our academic"; the Italian dialogue is reduced to a minimum and it is not clear why Galileo decided to use the two languages. We will deal with the two days hereafter.

4.12 The speed of light

One of the digressions of the first day that has particular importance in physics is that related to the speed of light. The digression is part of how bodies may be divided. Salviati believes that the "minimums" of fluids are "indivisible", that is they are atoms, while no mechanical sub-division could result in "minimums" of solids: dissolution in minimums is obtained through the "indivisibles of fire or the rays of the Sun" (VIII, 86). This simple hint is enough to discuss on concave mirrors, and praise Cavalieri's recent book *Specchio ustorio* (Bologna 1632) and to deal with the question of the speed of light. In summary, the digression is introduced forcefully into the text, a clear indication of the importance Galileo gave to the question and his continuous studies of the problem.

Virtually all physicists thought that the speed of light was infinite, both because one first sees the lightning and then hears the thunder, or the firing of a cannon, or because "as soon as the Sun rises, we see its shine" (VIII, 87). After having demonstrated that this reasoning is not conclusive, Galileo, in the words of Salviati, describes the experiment that should have put the question beyond doubt.

It can be summarised like this: two scientists, A and B, each holding a lantern, stand apart. A opens his lamp and B, as agreed, opens his as soon as he sees A's light. Therefore, A sees B's signal after the lighting of his own lamp, twice the time between it takes for the light to travel between A and B.

When Galileo tried out the experiment over a short distance, "that is less than a mile", it failed, and could not succeed given the huge speed of light. But he asked the question in experimental terms, and this is his great scientific merit, independently of the result. It is also to his credit to have invented a brilliant experiment that would be used more than two hundred years later by Armand-Hippolyte-Louis Fizeau in the first terrestrial measurement of the speed of light. Actually, conceptually, Fizeau's approach was no different to Galileo's, but he replaced the Galileo's experimenter A with a geared wheel that periodically obscures the light emitted from the source, and scientist B with a flat mirror that immediately reflects the received light. The fact that Galileo had not thought that experimenter B could be substituted more simply and more efficiently with a mirror demonstrates how often it is difficult to invent experiments that appear so obvious to our modern mind.

Galileo probably discussed the finite speed of light and the possibility of measuring it by experiment with his friend Sarpi, who as a young man had theorised measuring the speed of light in a still primitive experiment that perhaps encouraged Galileo in his project. Sarpi writes "A flash of light would behave like a sound, as it would cease to be seen from close up, when seen further away, with a smaller difference, because light is faster"[115] But Sarpi's experiment would have required synchronised clocks (and enormous distances): maybe Galileo's amendment was aimed to obviate that problem.

4.13 Pendular motion; acoustics; last works

Another digression of particular interest to physics closes the first day of the *Discourses*: pendular motion and its application to acoustics.

In a letter of 1602 to Del Monte, Galileo had described the experiments he conducted to discover the isochronism of pendular oscillations in 1583 (§ 4.1), and its independence from the material of which the pendulum is made. These laws, repeated in the *Dialogue*, are enriched in the *Discourses*

[115] Cassani, *Paolo Sarpi e le scienze naturali,* op. cit. pp. 310-11.

with the discovery of the proportionality of the period of a pendulum to the square root of its length, and he goes as far as to affirm that "all the vibrations of the pendulum, maximum, average and minimum, occur at precisely equal times" (VIII, 139), while in the *Dialogue*, more prudently, he says that the "pendulum vibrates with the same, or almost imperceptibly different, frequency" (VII, 475).

It is not easy to say when Galileo had the idea of applying a pendulum to a clock, although it can be found in a letter of 1637 (XVIII, 101-03). In 1641 the mechanism, with a double escapement, curved and rest, was completely designed and Galileo, according to Viviani (XIX, 655-57), passed the plans to his son Vincenzo who constructed a model.

Let us return to the *Discourses*. Pendular motion allowed Galileo to make an accurate study of mechanical resonance, which he exemplified in the motion assumed by a pendulum "only blown on [...] and repeat the blowing but in the time congruent with its vibrations" (VIII, 141). This mechanical introduction allowed him, and it is still used in schools today, to arrive at an explanation, not dissimilar to that used nowadays, of acoustic resonance and to describe a beautiful experiment on the production of waves in a glass of water but rubbing the rim with a finger. It was exactly through this experiment that Galileo ascertained that the tone of a sound depends on the frequency of the vibrations: the greater the frequency, the higher the note.

In another ingenious experiment Galileo changed the definition of the musical interval, established in his time as the length ratio of strings of equal dimension, material and tension. He, accidentally, noted that if a chisel is scraped across a brass sheet, it sometimes produces a sort of whistling sound and a long line of parallel and equidistant marks are impressed on the sheet. After many attempts, he managed to produce two sounds in unison with two strings with a ratio of 2:3. Observing the marks left by both on the sheet, he observed that the space with 45 marks at the first mark contained 30 in the second, thereby proving that the ratio of the length of the strings was the inverse ratio of the respective frequencies, a well-known law of vibrating strings. The musical interval could therefore be expressed as the frequency ratio.

The pages dealing with acoustics that contain these "*novellizie*" (new concepts), as Galileo calls them, were praised even by Descartes who judged them to be "*le meilleure*" (the best) in his famous acrimonious letter against Galileo.[116]

[116] Letter of 11 October 1638 to Mersenne, in Descartes, *Oeuvres,* op. cit., Vol. 2. p. 380.

Towards the end of 1637, with the *Discourses* still unpublished, Galileo, who even in old age managed "to calm his restless mind" started work on the marvellous *Operazioni astronomiche* ("Astronomical operations") in which he bequeathed to future astronomers the huge review demanded by the use of two instruments that we may say were of his invention: the telescope and the pendulum, "thanks to invention of which the science of astronomy achieved certainties that previous instruments had not allowed" (XVII, 212).

The last completed scientific work by Galileo arose from an argument with Fortunio Liceti (1577-1657), then professor of philosophy and medicine in Bologna, who, in a treatise published in 1640, had criticised Galileo's interpretation of cenereous light, claiming, on the contrary, that it was caused by a weak phosphorescence in the Moon's atmosphere.

Provoked by Liceti and Prince Leopoldo de' Medici, the future founder and president of the Cimento Academy, Galileo answered the criticism in a letter of March 1640 addressed to Leopoldo and forwarded to Liceti in a second draft. It is very polemic, but calm and almost kindly and courteous. It contains a short treatise on cenerary light, full of astute methodological observations. It is also significant as a human document as its reveals the great spiritual peace that the aged scientist had reached.

Supported by the filial devotion of two pupils, Viviani and Torricelli, Galileo died on 8 January 1642.

GALILEAN DYNAMICS

4.14 The principle of relativity

More than in the purely physical works, described above, Galileo's scientific fame lies in his discoveries on dynamics, intimately linked to justifying the Copernican system.

By 1597, Galileo was already a convinced Copernican, demonstrating that he had already found the refutations of followers of Ptolemy against the daily motion of the Earth.

A peripatetic criticism of the Earth's motion, widely accepted by public opinion, was based on the fact that mechanical phenomena occurred on the Earth's surface as if the Earth was motionless: birds in flight are not behind the Earth beneath if it is rotating; cannon shots towards the west are no longer than eastward ones; weights fall vertically and not obliquely; and so on.

These peripatetic objections are brilliantly confuted in the second day of the Dialogue, that summarises in the classic principle of relativity, already set out by Galileo, in almost the same terms as the letter to Ingoli (§ 4.9): "Shut yourself with a friend in the largest cabin below deck of a big ship, and collect some flies, butterflies and similar flying beasts. Get a large vase of water with some fish in it and hang a bucket over it that drips water into a narrow-neck vessel underneath; the ship does not move. Carefully observe how the birds fly at the same speed all around the cabin; the fish will swim indifferently in all directions; all the drops will fall into the vase below; and you, throwing any object to your friend, will not need to launch it with greater force on one side or the other. And if you jump with your legs together, you will move equally in all directions. Having carefully observed all these things, there is no doubt that they should be like this while the ship does not move. Now make the ship move at any speed; provided that the motion is uniform and not fluctuating, you will not notice even the slightest change in these effects, nor understand from them whether the ship is moving or stands still. [....], this correspondence of all these effects is due to the fact that the ship is moving together with everything within it, including air" (VII, 212-13).

Nowadays, this piece by Galileo is summarised by saying that the internal mechanical phenomena of a system are identical in both a motionless system and one that moves in a uniform rectilinear manner; or that mechanical phenomena occur in the same way in two systems driven by uniform motion with respect to each other. Analytically, the laws expressed in one system can be expressed in another, by applying very simple formulas that are known as *Galilean transformations*. The principle of relativity can be expressed by declaring that the laws of mechanics are unchanged with respect to a Galilean transformation.

4.15 The principle of inertia

In a letter dated 1607, don Castelli wrote that he had learned from Galileo that "to start motion, you need a force, but for its continuation there must be no opposition" (X, 170). This early mention to the principle of inertia, that, as we know, goes against popular belief, is detailed in the *Dialogue* and further analysed in the *Discourses*. In the *Dialogue*, it is established by a mental exercise that recalls the demonstration *ad absurdum* of the mathematicians: the inclination of a plane causes the acceleration of a body travelling down it and the deceleration of a body moving back up; a body travelling on a limitless horizontal plane moves in a uniform manner, here being no cause for acceleration or delay.

The principle of inertia has a long history, as shown in previous paragraphs (§§ 1.3, 2.4. 3.6, 3.14), but no-one had set it down so clearly before. It is true, as many commentators have pointed out, that Galileo does not set a general rule for the principle (that can first be found in a minor work published in 1635 by Giuseppe Ballo of Palermo), and it is also true that in the *Dialogue*, Galileo seems to attribute inertia also to circular motion (VII, 174). But in the *Discourses*, Galileo correctly applies the principle to vertically rising motion (§ 4.17) and artillery fires, connecting it to the principle of relativity, even if the links do not appear as close as we recognise today. We may therefore conclude that Galileo, albeit slowly, came to understand the basics of the principle of inertia in all its generality. The fact that he believed weight to be an intrinsic property of bodies and not a force acting upon them does not lead to Koyré's conclusion of the impossibility of the general concept of inertia, but only to the observation that Galileo studied the world using his senses, as it is and not as it could be.

4.16 Free-falling bodies

The discovery of free fall of bodies, according to Lagrange who understood quite a lot about mechanics, required an "extraordinary genius". The exceptional intellectual effort also results from the tortuous affair of the discovery.

In 1602, Galileo understood the theorems of the fall of bodies along arches of circle and subtended chords (X, 98-100), that led us to suppose he knew the law of spaces proportional to the squares of time. But even if he had understood that, he had arrived at the conclusion in the wrong way, as shown in a remaining fragment (VIII, 383) in which he expresses the hypothesis that the velocity of the fall is proportionate to the space traveled, a hypothesis communicated to Sarpi in 1604. But in another piece, written in an imprecise period between 1604 and 1623, Galileo explains the reasons why he abandoned that idea. The same, unconvincing, reasoning is repeated in the *Discourses* (VIII, 203-04) together with an honest confession of his mistake. The reasoning applies the laws of uniform motion to uniformly accelerated motion: the same mistake Galileo made in deducing, as we have said, from the wrong hypothesis the exact conclusion of the space proportional to the square of time.

Having eliminated the proportionality between the velocity of falling and the space travelled, and guided by the principle of the simplicity of nature, Galileo adopted the hypothesis of velocity proportional to the fall time. But in the *Discourses*, this hypothesis becomes the definition of naturally accelerated motion, that is precisely that motion in which "equal times give equal indications of velocity" (VIII, 202).

Galileo goes on, constructing a time-velocity diagram and using a famous graphic integration, often still repeated in physical texts, to demonstrate that in a uniformly accelerated motion, the space travelled is equal to that travelled in the same time by a uniform motion whose velocity is half the final velocity of the accelerated motion: from which the proportionality of space to the square of time derives immediately.

But these are mathematical deductions: does Nature obey these laws? We must believe that it does, replies Galileo, introducing a new philosophical concept in physical research, particularly if the mathematical consequences are confirmed by experimentation. But experimental proof was impossible with the tools of the time as the phenomenon is too fast. Galileo then had a brilliant idea: slow down the motion without altering its nature. Along the length of a twelve-arms table, he cut a straight channel covered by the smootest parchment; he then rolled a hard bronze, round and polished ball down the channel from various positions. At the same time, he timed the descent in an ingenious way: a bucket with a small straw in its bottom, released a tiny stream of water that was collected in a glass; the ratios between the weights of the water were assumed to be equal to the ratios between the corresponding times.

These famous experiments, too, minutely described by Galileo, were questioned by his critics who particularly pointed out that the experiment was too delicate to be successful. Basically, for these critics, Galileo's experimental ability became a reason to ban him from experimenting. And yet, even recently, Galileo's experiments on the inclined plane were repeated with results not very different from Galileo's own.[117]

Galileo also makes another postulation: bodies that fall on different inclined planes of equal elevation acquire, at the end of the descent, equal velocities. The postulation, despite being made acceptable by ingenious experiments with a pendulum of variable length (where pendulums swinging in different directions from the same height return to the same height: Fig. 4.3), was difficult for the young Viviani to admit. Galileo, now completely blind, managed to give a demonstration, dictated to the young pupil and communicated (1639) to Castelli. The demonstration is based on a new postulate, that is another evidence of the old scientist genius: each mechanical system moves spontaneously so that its centre of gravity descends. The principle is now named after Torricelli, because it was published in 1644 by the scientist of Faenza, who ignored the Galileo's statement.

[117] T.B. Settle, *An Experiment in the History of Science*, in "Science" 3, 1961, pp. 19-23.

Using this postulation of the inclined plane, Galileo founded a completely new theory of the motion on inclined planes and the motion along chords of arcs of circle. In particular, he demonstrates that the motion along arcs less or equal to a quadrant is faster than along the underlying chords, but Galileo imprudently extrapolates the result, affirming that the circumference is the curve of maximum velocity, the "brachistochrone".

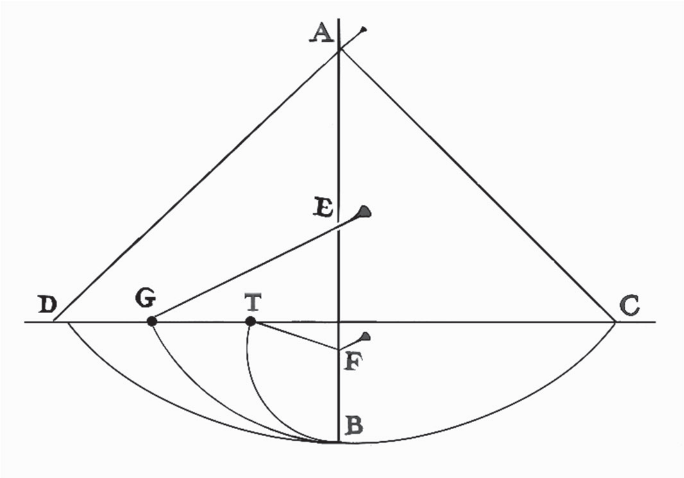

Fig. 4.3 - Diagram of Galileo's variable pendulum
Source: Galilei, *Works*, op. cit. Vol. 8.

The demonstrations, conducted *more geometrico*, are excessively prolix compared to the analytic approaches we are used to today, however appear so spontaneous and simple as to be more suggestive than ours.

In ascent and descent, both vertical and for inclined planes, the constant force applied to the body (that is, its weight or a component of its weight) results in a uniformly accelerated motion. It may therefore be said, as Newton believed (§ 6.6), that the second law of dynamics was discovered by Galileo, even if the general formulation of the law cannot be found in his writings.

4.17 Instantaneous velocity

It has often been observed (Giovanni Plana, Ernst Mach, Paul Tannery, Alexandre Koyré) that if Galileo had been able to set down in differential terms the concept of the proportionality of velocity to the space travelled and had known to then integrate the equation, he would have realised how absurd the theory was. But, independently of this possibility, if velocity were proportional to space, initially it would be null, as the space travelled is null; and so how did the motion start? Consequently, it appears to us that Galileo's repeated mistakes can be explained more by the deficiency of the mathematical tools of the time, with the intrinsic difficulty of the concepts in the cultural environment of the time, that today seem simple because we have been brought up in a different scientific climate.

Up until the middle of the XVII century, the law of the fall of the bodies was doubted. Mersenne thought it wrong; Descartes replaced it with a law in which the space travelled was proportional to time elevated to a bizarre exponent; Baliani, employing a complicated hypothesis that basically stated that the increase in velocity was in stops and starts, deduced that the spaces travelled in successive and equal intervals of time increased as the natural series of integers.

At the root of the incomprehension of contemporary scientist lies the difficulty of the notions of the infinite and the infinitesimal, that underlie the concept of instantaneous velocity, fundamental to Galilean dynamics.

If velocity is proportional to time, it must assume infinite values, as the moments contained in any interval of time are infinite. On the contrary, common experience would seem to indicate that a falling weight immediately reaches a great speed. Again, the mathematics of the time - that was a mathematics of the finite - did not contribute to an understanding of the problem.

In an interesting passage of the *Discourses* (VIII, 198-201), Galileo returns to, and further analyses the argument already treated in the *Dialogue*, and justifies the hypothesis with arguments that today seem acceptable, but were not appreciated at the time. Why does a body, starting from being motionless, immediately accelerate 10 degrees of velocity and not 4 and 2, "and, in sum, all minor numbers up to infinity"? In the ascent of weights, velocity decreases and passes through infinite values; movement would continue infinitely, as his opponents objected, if the moving object maintained the same velocity for some time, "but it passes over without stopping for more than a moment", so that infinite moments correspond to the infinite values of the velocity. If the moving object then maintains the velocity reached in a given moment for a certain time, it "would continue its uniform motion forever": a new allusion to the principle of inertia, not restricted to the horizontal motion (§ 4.15).

4.18 The composition of motions; motion of projectiles

Aristotle's contraposition of natural motion against violent motion naturally led to the concept of the impossibility of treating them equally. We have to wait until Bruno to discover an implicit admission of the independence of simultaneous motions. In a passage of the *Cena de le ceneri* ("The Ash Wednesday Supper") (1584), Bruno proposes two men, one on a moving boat, the other on the shore. At the same time each drop, from the same height, a stone: the stone dropped by the man on the shore falls behind, because the stone of the man on the boat participates not only in the motion of the fall but also the movement of the boat. But after Bruno, and even after Galileo, Gilles Personne de Roberval, influenced by Bernardino Baldi (1533-1617), according to Duhem, affirmed that the initial motion of the projectile almost entirely follows the violent movement, without a preponderating comparison of the natural motion of falling. Then, little by little, the violent motion weakens and the natural fall prevails.

With these precedents, it is easy to understand the novelty of the Latin treatment of the motion of projectiles contained in the fourth day of the *Discourses*: "I think of a moving object launched on a horizontal plane, without any impediment; we know that is motion will be uniform and perpetual along the same plane if this is prolonged to the infinite; if, on the other hand, we suppose a limited plane positioned higher, the moving object, that I presume to have gravity, as it arrives at the end of the plane and moving further forward, will add to the uniform and indelible motion a downward inclination acquired through its gravity and the result will be a movement composed of a uniform horizontal motion and a naturally accelerating vertical motion" (VIII, 268).

After a geometric digression, the treatise continues with the tracing by points of the trajectory.

In this way, without a general rule, but with the application to a concrete example, we have the introduction of the principle of independence of simultaneous motions, establishing a connection between the principle of inertia and the principle of relativity. "One cannot deny," we will say with Sagredo of the *Dialogue*, "that the argument is new, ingenious, and conclusive [...] and that in the mixing of these movements and their velocities, they are not altered, disturbed or impeded" (VIII, 273).

Given the rules of the composition of instantaneous velocities for two uniform orthogonal movements and for two orthogonal movements, one uniform and the other accelerated uniformly, Galileo illustrates the first ballistic theorems. Those most worthy of mention include: the maximum range is reached when the weapon is inclined at 45° to the horizon; the ranges are equal with an inclination of 45° $\pm\alpha$; the projectile, relaunched from the point of arrival at an inverse velocity will travel along the same trajectory in reverse, reaching the same speed at each point but with opposite sign. The latter important proposition, that a fragment of the *Discourses* (VIII, 446-47) attempts to prove, was also formulated for the motion of freefalling objects (VIII, 200).

We must go back to the period in Padua to find the famous discovery of the parabolic motion of projectiles (VIII, 428), spread so widely among Galileo's pupils that Cavalieri believed he could publish it in the *Specchio ustorio* of 1632 without crediting the author, known to everyone: which provoked some resentment on the part of the master. The claimed priority of the discovery by Del Monte, supported by Libri, has no credible foundations.

4.19 The method

The preceding pages have described some of Galileo's fundamental discoveries. The scientist's greatest merit lies not in his discoveries, but in the new mentality introduced into the study of nature. And when we say that Galileo was the founder of the experimental method, we should not mean that it is to him that we owe the introduction of experimentation as an analytical tool, because the practice of experimentation flowed down to him from classical antiquity, as we hope to have shown in this history. But with Galileo, experimentation was enriched by some particular aspects that make it appear new.

These are: repudiation of the principle of authority, because a single experiment is worth more than a thousand Aristotelian reasons; the study of phenomena, that is describing nature, with an analysis of *how* the phenomena occur, without asking *why*; abandoning every occult cause and anthropomorphic interpretation of nature; recognition of the singular importance of the quantitative aspect of the phenomena, because the book of nature "is written in mathematical language, and the characters are triangles, circles, and other geometrical figures, without which it is humanly impossible to understand any word; without these it is a vain wandering in a dark labyrinth" (VI, 232); last, faith in the simplicity of nature. Nature follows simple mathematical laws: this is the great new concept brought to the physical research of the time and through this concept the century is "Galilean".

Leaving aside these constants in his thinking, it is difficult to find in his works a conscious application of an analytical approach or unconditional belief in a philosophical system. There is no doubt that there is a Platonic component in his approach, but a person such as Galileo, so full of various impulses, with such an exuberant scientific imagination, so free from the ties of tradition, could not be bound by rigid schemes. That said, especially in the research on dynamics, the Galilean experimental approach followed, in an unwitting process that may in many cases be divided into four moments.

The first phase is the perception of the phenomenon, the *sensata esperienza*, as Galileo expresses it, that brings to mind the study of a particular group of phenomena, but does not give us a law of nature. That our brain supinely receives scientific knowledge from the external world, that is that experience is everything and contains everything, seems a concept foreign to Galilean thinking. From the "experience felt", we pass to the *axiom*, using Galileo's expression, that is the *hypothesis of work*, in modern terminology: this is the culmination of the discovery that gives rise to a critical examination of the experience through a creative process not dissimilar to artistic intuition. There follows the third phase, that Galileo calls *mathematical progress*, that is the logical deductions deriving from the chosen work hypothesis. But does the mathematical conclusion concur with the experience? "Because our discourses must relate to a sensible world and not a world made of paper" (VII, 139). This brings us to the fourth moment of Galilean experimentation: test by experiment, almost a Godly judgement of the entire discovery process. Sensing experience, work hypothesis, mathematical progress, experimental proof are the four phases in studying a natural phenomenon that begins with experience and ends with experience, but which cannot evolve without mathematics.

But is Galileo's mathematics merely instrumental or is metaphysical as with Plato? The question, which is also a question of Galileo's philosophy, was and is the subject of much debate. Galileo has been called Platonic, Kantian, Positivist, and so on. Without entering the argument, although our opinion has been expressed above, we shall close by recalling that the scientist wanted the collection of his works to be prefaced by: "These contain infinite examples of how mathematics may be useful in explaining natural propositions and how it is impossible to philosophise without the aid of geometry, in conformance with the truths expounded by Plato" (VIII, 613-14).

5. IN A NEW FURROW

THE SCIENTIFIC CLIMATE

5.1 The Galilean message

Giacomo Leopardi has Copernicus declare that the affirmation of the heliocentric system "will not be simply material pretends at first sight; and [...] its effects are not merely physical because they capsize the dignities of things, the order of entities; it will change the purposes of creatures and therefore will make great changes in metaphysics, even more in all that is the speculative part of knowledge. And the result will be that the men, even if they are capable of, or desire to, discourse sagely, will discover to be very different than they were before, or imagined to be".[118]

This upturning of mentality, so well decribed by Leopardi, clearly indicates that research in physics after Galileo has been at last removed from the centuries-old yoke of theology and teleology. It is true that there was no lack of opposers of the new approach to studies, especially zealous after Galileo's condemnation, but the majority of them had to reply to the observations with other observations, experiences with other experiences, mathematical demonstrations with other mathematical demonstrations. Constrained to question things and not Aristotle's texts, even the critics of Galileo, of which there were a great number in his century, contributed to the formation of a new scientific climate that, just in accepting the Galilean message, completed, extended and corrected the work of the Pisan scientist.

In Italy, too, where resistance was most strong, renewal was quite rapid. In 1651, the learned Jesuit Giovan Battista Riccioli (1598-1671), in his *Almagestum novum* was forced to recognize as unfounded all the criticisms about dynamics of the Copernican system and he put forward his own which he believed decisive: he set the motion of the fall of a body from a high tower in relation to the motion of the rotation of the Earth and concluded, due to the inexact knowledge of the laws of the new mechanics, that the effects of collision on a horizontal plan of a falling body must be almost independent of the height of the fall; the contradiction of this result with

[118] G. Leopardi, *Le operette morali,* edited by G. Chiarini, Vigo, Livorno q870, p. 314.

common experience was evident proof, according to Riccioli, of the immobility of the Earth.

But another two Galilean ecclesiastics, the Neapolitan Giovanni Alfonso Borelli and Stefano degli Angeli of Treviso (1623-1697), were able to demonstrate easily, although taking some verbal precautions to avoid the ever-vigilant Inquisition, the inconsistency of Riccioli's criticisms: both, independently, observed that Riccioli's reasoning was flawed because in calculating the effects of collision he did not take into account the direction of the velocity of the falling body. But the two Galileans argued among themselves over the different trajectory assigned to the falling weight and its deviation towards the east, admitted by Borelli and denied by degli Angeli. The argument between Riccioli, Borelli and degli Angeli is an important document in the history of the fortunes of the Copernican system and the new dynamics in Italy.[119]

Galilean dynamics took the upper hand not only because of its intrinsic merits but also because of the authority of scientist, glorified by martyrdom, and through the work of his disciples, led by Torricelli.

5.2 Evangelista Torricelli

In April 1641, Don Benedetto Castelli, reader in mathematics in the Studium of Rome and already a follower of Galileo, visited the master in Arcetri and asked him to examine a manuscript dealing with the motion of naturally falling bodies. The author was one of his student, Evangelista Torricelli (Fig. 5.I), born in Faenza on 5 October 1608, who Castelli suggested Galileo took into his house as an assistant to provide definitive support to his research into mechanics. The proposal was immediately accepted and Torricelli moved to Arcetri, in Galileo's house, in the first fortnight of October of the same year, but the collaboration lasted only three months. On the death of Galileo, the grand-duke nominated Torricelli his personal mathematician to occupy the vacant post. Unfortunately, the scientist of Faenza remained in the job for a little over five years; after a short and violent illness, Torricelli died on 25 October 1647 aged just 39.

The following paragraphs will describe Torricelli's contribution to aerostatics and optics. For the moment, we will limit ourselves to a brief analysis of his contribution to mechanics, contained in a single volume he printed in 1644, divided into three sections. The first and third are dedicated to geometry; the second, entitled *De motu gravium descendentium et proiectorum libri duo*, is basically the paper proposed by Castelli to Galileo.

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119 For greater historical and bibliographical details, see also M. Gliozzi, *Angeli, Stefano degli,* in *Dizionario biografico degli italiani,* Vol. 3, Rome 1961, pp. 205-06.

In the first book of *De Motu*, Torricelli sets out to demonstrate Galileo's postulate of the equal velocities of falling weights on inclined planes of equal height, and as, unknown to him, Galileo had already done (§ 4.16), he demonstrated it assuming as a postulate the principle now known as Torricelli's Law on the motion of centres of gravity. And so, he commentated: "When two weights are tied together so that the motion of one follows the motion of the other, they act as a single weight formed of two parts [...]: but such a weight will never be set in motion unless its centre of gravity goes down. Therefore, when it is in such conditions that its centre of gravity can in no way descend, the weight will certainly remain immobile in the position it occupies".[120]

Fig. 5.1 - Evangelista Torricelli
Source: Alinari

[120] E. *Torricelli, Works, edited by G. Loria and G. Vassura, Montanari, Faenza 1919, Vol. 2, p. 105.*

Torricelli also has the merit of refuting in the numerous applications of the principle (to the inclined plane, the lever, the motion of cords in a circle or a parabola), the theory of serious authors who upbraided Archimedes for having considered parallel, instead of concurrent to the centre of the Earth, the vertical directions of two strings weighed down to the Earth surfaces, and to have taught that the Archimedean concept was the most suitable for the study of theoretical physics.

The second book primarily deals with the motion of projectiles, generalising the doctrine contained in Galileo's *Discourses*. He had studied only the movement of projectiles launched horizontally; accidentally and without demonstration (§ 4.18), Galileo had affirmed the reversibility of motion. Torricelli, on the contrary, considered any oblique launch and by applying Galilean principles determined parabolic trajectory and the ballistic laws well-known today. In particular, by extending Galileo's observations, he noted that the motion of projectiles can be inverted.[121] The concept that dynamic phenomena are reversible, that time in Galilean mechanics is ordered but without any direction, is thanks to Galileo and Torricelli.

A chapter of the second book is dedicated to *De motu aquarum,* a subject that had had its immediate precursors in Benedetti and Castelli. Torricelli made such an important contribution to the study of the question that Mach declared him the founder of hydrodynamics. The purpose of Torricelli's treatise is the study of the outflow of water from holes made in the recipients.

The theory of vertical movements and the experiment suggested to him the fundamental hypothesis. If a liquid is spouted upwards from a small well-smoothed hole made in a recipient full of water, it will rise almost to the same level of the level in the container. The small difference is to be ascribed in part to the resistance of air and in part to the falling water that obstructs the movement of the new rising water, as can be deduced from the fact that if the exit hole is closed by a finger and then rapidly re-opened, the first spurt is higher than the following ones. The experiment works better with mercury. If all resistances to the motion of the liquid were null, the jet would reach the level of the liquid in the container: based on the laws of the motion of weights, the initial velocity of the jet would be equal to that which a body would have in free-fall from the level of the liquid to the exit hole. These experiments and these considerations led the scientist to express the fundamental theory (now known as "Torricelli's theory"): "Water exiting violently has, at the point of exit, the same velocity that any heavy body

[121] I*bid, pp. 160-61.*

would have, that is also a single drop of water, dropping naturally from the highest level of the water up to the exit hole".[122]

Doubts were immediately raised about this theory (a particular case of the conservation of energy). Mersenne interpreted them in a letter to Torricelli of 4 February 1645: how is it possible that the water exiting the hole, that is not the same found at the highest level of the tube, has a velocity corresponding to that which it would acquire falling from that level? And Torricelli replied without justification but relying on experience: the principle is true, provided that the exit hole is much smaller that the section of the recipient.[123]

The proof would be given by Newton in proposition XXXVI of the second book of the *Principles,* and by Pierre Varignon in a very similar way. Torricelli used the theorem, in conjunction with the results already obtained on the motion of projectiles, to demonstrate that if the exit hole is made on the wall towards the bottom of the container, the jet is parabolic. He also made some acute physical observations on the breaking up into droplets of the fluid stream and on the effects the resistance of air.

5.3 For and against Galilean dynamics

After dealing with the outflow of liquids, Torricelli continued the treatise by elaborating five tables; afraid that the experts to whom he addressed the problem did not understand Latin he quickly changed to the vernacular. The tables, similarly to those that Galileo had set out in the *Discourses,* are more properly trigonometric tables that, according to the angle of fire and given the initial velocity of the projectile, allow the characteristic elements of the trajectory to be calculated.

Galileo and Torricelli were mistaken in thinking that these tables could be of real use to the artillery. The dynamics of the two scientists was the dynamics of bodies moving in a vacuum, without attrition. Galileo had observed in the *Discourses* that the acceleration of a body falling through air gradually diminishes, "to that finally the velocity reaches at that state, and the resistance of the means such a size, that, balancing each other out, cancel acceleration and reduce the body to a uniform motion, in which it will continue forever" (VIII, 119).

Isaac Beeckman, also, admitted the existence of a limit to the speed for bodies falling through air, a concept first rejected by Descartes, then later accepted. The scenario, therefore, was prepared to propose a series of criticisms of Galilean dynamics. Descartes began the movement, followed

[122] *Ibid., p. 186.*
[123] *Ibid.,* pp 269 and 276.

by Roberval: the Galilean law of the fall of weights, he wrote to Torricelli, having read *De motu*, is valid for the fall from mediocre heights and dense bodies; but for light bodies and greater heights, the experiment belies itself because the bodies end up assuming a constant velocity.[124] Therefore, it is not true that a projectile launched vertically returns to the point of departure at the same speed, nor takes the same time to rise and fall. And then, if it was claimed that the theory does not consider the impediment of air, why construct the firing tables?

Torricelli, who had learned from his former pupil Michelangelo Ricci the criticisms of Roberval, replied to Ricci: "That the principles of *de motu* are true or false is of little import to me [...]. I pretend or suppose that any body or point moves up or down with the known proportion and horizontally with uniform motion. When this occurs, I say that it follows all that Galileo, and also I, declared. If then lead, iron, stone balls do not observe the supposed proportion, to its damage, we will say that we are not talking of them". In a letter of 7 July 1646, he replied to Roberval with the same consideration and added that the boards, the instruments etc, included in his *De motu,* are not used to measure the launch of the projectiles, but to trace geometrical lines.

The reply, the only reasonable one possible, should have satisfied the geometricans (Descartes, Mersenne, Roberval) who had contested the theoretical foundations of the doctrine (§§ 4.17-18), but could not be appreciated by the practicians. A group of Genoese gunners wanted to test the precision of the tables and remained "surprised" that the theory "responded so badly to practice", so that in their eyes the theory of their fellow citizen Baliani (§ 4.17) was more believable. This concluded that the motion of the projectiles was not parabolic. When Giovan Battista Renieri informed him of the failure of the experiment, Torricelli replied that his deductions were mathematical and were not confirmed by the experiment due to the impediment of air, and above all in the way the experiment had been conducted: imperfect levelling of the piece, lack of horizontal placing of the launching board, moving of the cannon at the moment of firing that raised the mouth. Torricelli asked for minute details on how the experiment had been executed, demonstrating his desire to believe in the practical value of his reference tables for bullet trajectories.[125]

In fact, the ballistics of Galileo and Torricelli were not practical, but the presuppositions of practical ballistics would become reality when Huygens and Newton understood how to combine the theoretical dynamics of Galileo with the study of the resistance of the medium.

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[124] *Ibid.,* Vol. 3, pp. 352-53.
[125] *Ibid.,* pp. 461-66.

5.4 Galileo's scientific thought in France

The contribution of many French scientists to the spread of Galilean thinking was no less than his Italian followers. It is true that the faculty of theology at the Sorbonne, on the wishes of the powerful cardinal Richelieu, condemned as false the doctrine of the Earth's motion. But Peiresc, Elia Diodati, Pierre de Carcavi, Mersenne, and Gassendi, the duke of Noailles, rebelled against the condemnation and all assumed the responsibility of spreading the new science in France, and therefore the cultivated world of the time.

Of particular merit was the work of Marin Mersenne, born in Oize (Sarthe) on 8 September 1588; he entered the Order of the Minors and, after some years teaching in Nevers, moved definitively to Paris where he died on 1 September 1648.

Anti-Copernican in his youth, he later became a firm follower and made several attempts to establish correspondence with Galileo but it seems that the Italian scientist had only a vague idea of who he was. In 1634, as we have already mentioned, Mersenne translated Galileo's *Mechanics;* in the same year, when reprinting and translations of the *Maximum systems* were prohibited, he compiled for his countrymen a summary of the work; in 1639 he distributed in France the main points of the *Discourses* in a volume entitled *Les nouvelles pensées de Galilée.*

This sincere admiration for the Italian scientist notwithstanding, Mersenne declared his own free spirit and after the appearance of Descartes' theory of subtle matter, a co-disciple of whom he had been at La Flèche, he advanced some doubts over the Galilean theory of the fall of weights, basing his views, as we have seen, on experience, in which he had limitless faith.

Although it is pointless to look for originality in the works of Father Mersenne, he did play an essential role in the diffusion of the new science: giving information on other values, explaining them, summarising them, at times publishing them in their entirety, his books and correspondence that were published only in the XX century, are a mine of information on the tumultuous period in which he lived. A tireless correspondent with the major scientists of the time, Mersenne gave and received information, proposed questions, raised objections, thereby carrying out that work of connection, clarification and diffusion now entrusted to the leading international scientific journals. A laborious man, but also upright and free, he admired Galileo and Descartes without be dominated by either.

Another admirer of Galileo was Pierre Gassend (1592-1655), known as Gassendi, philosopher, scientist and provost of Digne cathedral, he was later professor of mathematics in Paris. His work to affirm the new dynamics and, above all, Copernican thinking to which it was now linked, should have had greater resonance if excessive prudence had not sometimes induced him to disapprove in public what he affirmed in private.

A man of multiple interests and encyclopaedic culture, Gassendi made accurate astronomical observations; in 1640 he proved by experiment the classic principle of relativity, dropping a stone from the top of the mast of a moving ship and showing that it arrived at the bottom of the mast as if the ship was not moving. He was close to Galileo also in the doctrine of the subjectivity of sensations and the atomistic theory, which he analysed further than Galileo. Gassendi believed in the existence of a single matter, common to all bodies, divided into atoms that are separated from each other by vacuum; they are indivisible and their shape (round, elongated, pointed, etc.) causes the apparent diversity of natural bodies that are heavy not because of intrinsic virtue but because of the attraction of the Earth on atoms.

PRE-NEWTONIAN MECHANICS

5.5 Descartes

"He who has contributed the most to this work - wrote Jacques Rohault (1620-1675) in the preface to his treatise on physics - and is not mentioned by name in any passage, because it would have been necessary to repeat it too often, is the famous Mr Descartes":[126] who therefore appears to his first followers as the founder of the new physics. All the same, it is the common opinion of historians that Descartes' technical contribution to physics, and mechanics in particular, was very modest. And yet, how can we explain the hyperbolic praises heaped on him by his admirers? How was it possible that for fifty years, at the turn of two centuries, physicists were divided into two factions, Cartesian and Newtonian, one set against the other? We will postpone the answer to paragraph 5.8.

René Descartes du Perron (Cartesius, Fig. 5.2) was born in La Haye on 31 March 1596; he entered the Jesuit college of La Flèche in 1604 and remained there until 1612. In 1618, he began a military career, abandoning it after a few years to dedicate himself, driven by an almost supernatural revelation, to founding a new science: the method for conducting our reason well. To enjoy greater tranquillity, in 1628 he settled in Holland where he remained until 1649 then he moved, on the invitation of Queen Cristina of Sweden, to Stockholm where he died of pneumonia on 11 February 1650.

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[126] J. Rohault, *Traité de physique,* Paris 1671, *Préface.*

In Part VI of his *Discourse of the method,* Descartes writes: “Instead of the speculative philosophy taught in school, we may find a practical one, through which, understanding the forces and the actions of fire, water, air, the stars, the heavens, and all the other bodies that surround us as distinctly as we know the diverse arts of our craftsmen, we may use them in the same way in which they are adapted so as to render us masters and possessors of nature”.[127]

Fig. 5.2 - Descartes.
Anonymous XVII century engraving

This utilitarian concept of science is common to all scientists and scientific philosophers of the scientific renaissance, from Tartaglia to Porta, from Gilbert to Kepler, and from Bacon to Galileo. In the case of Descartes,

[127] Descartes, *Works,* op. cit., Vol. 6, pp. 61-62.

it should be underlined that his clarification of the reasons for the choice of the physical arguments to which he dedicated himself, in our opinion, explains some of his scientific positions.

If ancient philosophy had not contributed to improving life through technical progress, it was necessary to change track, find a new way of philosophizing. Gilbert, Bacon, and Galileo had also noted the fracture between philosophy and the technique, and each had believed he had overcome it with a solid bridge. Descartes saw in mathematics a model for sciences and was amazed that "on such firm and solid bases, nothing more important had been built". In his opinion, nothing more important had been constructed because mathematics had been at the service solely and partially to the mechanical arts, while he saw the possibility of applying it to the explanation and description of all physical phenomena. Treating physics mathematically and, more specifically, geometrically: that was the way forward.

Treating physics geometrically signified to direct treatment following the Euclidean model: a few axioms, self-evident, on which to base a well-ordered series of deductions that contribute to the same immediate certainties of the first axioms. But to establish those axioms it is necessary first of all to free our mind of every error of the past; in the first place, and the main one, the attribution to bodies of qualities that are, on the contrary, in us: sounds, colours, smells, etc., that are not in the bodies but in the sentient being - the Democritean motive, already dealt with by Galileo (§ 4.8), was taken to its extreme conclusions by Descartes. Banishing the occult qualities of the vulgar and magic, we must also free ourselves from the "real qualities", the "intentional forms" of the learned that, substantially, are the doctrinal translation of that first fundamental prejudice. With the "forms", "qualities" and "action", we would never be able to explain that fire burns, warms and consumes wood.

We may only correct these errors with a "clear and distinct" idea. Descartes appreciated the value of experimentation and often complained that he had neither sufficient means nor time to undertake long and expensive experimental research, but he did conduct careful and accurate experiments. But when the experiment conflicted with theoretical results, he did not hesitate to trust his own reasoning over the physical result. This diffidence to experimental results was also related to his scepticism about the fallacy of the senses,[128] much cultivated by Scholastic philosophy, as we have mentioned (§ 4.5). Therefore Descartes, not wanting to trust the senses, searched for the criterion of truth in the immediate intuitive certainty. To a

[128] Descartes, *Meditationes de prima philosophia,* VI, in *Ibid.*, *Works,* op. cit., Vol. 7, pp. 76-77.

geometrician of his genius, nothing could seem more immediately intuitive, clearer or more distinct, than extension and movement: these were, therefore, the two pillars on which to build all physics.

But is this extension, this space, of which we have immediate intuition, empty or full? The argument, centuries old, was still open. To deny the vacuum, physicists had used a variety of experiments: the clepsydras (water clock) or the barrel that do not empty as long as there is a single opening; suckers, oil lamps, constructed like modern bird feeders, suction pumps, etc. On the other side, the vacuists had established, over the centuries, a number of schools: the supporters of the existence of empty masses, promoters of the vacuum disseminated (*vacuum intermixtum*) between atom and atom; partisans of the possibility of obtaining a vacuum through violence.

Descartes declared himself totally against the vacuum in all its forms, putting forward, in addition to the Aristotelian argument of the contradictory nature of the vacuum, common physical experiments like the ones mentioned above. The absence of the vacuum forced Descartes to suppose a "subtle matter" that we cannot sense, empowered with a God-given perpetual motion.

5.6 Descartes' "Laws of Nature"

The world is, therefore, made of matter, the essence of which is its sole extension, and movement. Consequently, it is enough to establish the laws of movement to deduce a well-ordered theory of the laws of the sensible world and, therefore, the laws of movement become, without doubt, the "laws of nature".

What is movement? In the treatise *The World,* Descartes gave the following definition: "The motion is the action by which bodies pass from one place to another, subsequently occupying all the space in between".[129] This definition, purely cinematic, cannot be claimed to be satisfactory, because it does not refer to the relativity of motion. Between the drafting of the *World* and the *Principia philosophiae,* which returns to the argument, Galileo's *Dialogue* o*n the maximum systems* appeared, establishing the classic principle of relativity (§ 4.14). Perhaps influenced by Galileo's works, Descartes modified the original definition with: "Movement is the transport of one part of matter, or a body, from the vicinity of those immediately touching it, and that we consider at rest, in the vicinity of any other."[130] Here motion is conceived relativistically, but not in a Galilean way. Using modern terminology, we may say that while Galileo imagined

[129] *Ibid.,* Vol. 2., p. 40.
[130] *Ibid.,* Vol. 9, p. 76.

the possibility of infinite reference systems, Descartes saw only one reference system related to a body that at the start of the movement is immediately in contact with the moving body. In summary, the Cartesian definition in pointlessly restrictive. Fortunately, it does not correspond to the real thinking of the scientist, as can be seen, without mentioning anything else, from the fact that some pages before the explicit definition of motion, Descartes gives the example of a man on a ship to demonstrate that "the same thing, in the same time, changes and does not change the place", to conclude "that there is nothing in the worlds that is still or fixed, unless we fix it in our thoughts". Published after Galileo's sentencing, the purpose of Descartes' second definition of motion is to not arouse the anger of the Inquisition against the author, as we shall see later. If Descartes had been able, and sincerely believed to be able, to express his philosophy, he might have given a wider concept of the relativity of motion than Galileo's. In fact, while the latter, as did Newton later, believed in the absolute motion in the space, Descartes affirmed, unambiguously, its relative character: "Two men - he wrote privately to a correspondent - one moving with the boat and the other stationary on the shore [...] nothing is more positive in the motion of one than the stillness of the other".[131] Therefore, while for Scholastics movement is a becoming, for Descartes movement is a state, like rest, therefore for Descartes movement and rest are indistinguishable: "Movement and rest are only two different modes".[132]

The first Cartesian law of motion ("everything continues in the state it is, if nothing changes") and the second ("each moving body tends to continue to move in a straight line") constitute what we now know as the principle of inertia, because the uniformity of motion is implicit, according to Descartes, in the first law, according to the principle of sufficient reason.

The third law of motion states: "If a moving body meets another of greater mass [*plus forte que soy*] it will lose nothing of its movement and if meets one of lesser mass that it can move, it will loss as much as it gives".[133]

The third Cartesian law is truly a central point in kinetic mechanics: knowing how to identify it and placing it at the basis of his own mechanics demonstrates that Descartes possessed a rare mechanical intuition. We may see in the third law that Descartes intended by movement what on another occasion he called quantity of movement, that is the product of the mass multiplied by the velocity of a body, and proves the law with some experiments of collision, but the most valid justification appears to him the

[131] *Ibid.,* Vol. 5, p. 348.
[132] *Ibid.,* Vol. 9, p. 78.
[133] *Ibid.,* p. 86.

immutability of God, because "God never changes his mode of acting and preserves the world with the same action that he gave it".

Unfortunately, however, in setting out the law, Descartes commits an error that is strikingly strange for a geometrician of his quality. He did not consider that, as velocity, as we now say and he knew, is a vector, and therefore has direction and sense, the quantities of motion become vectors; consequently, their sum must be a geometrical sum rather than algebraic. This third law is, therefore, flawed; as a result, the seven rules that follow (except for the first) that make up the Cartesian theory of the collision of elastic bodies, are flawed.

The experiment should have put Descartes on the alert, and for some rules the experiment was easy. For example, for the third, in which Descartes states that when the central collision occurs between two bodies of equal mass and different speeds, the body with the lesser velocity changes its direction and the two bodies continue to move while remaining in contact, with a same velocity equal to the arithmetic median of the two original velocities; while the correct theory and the experiment show that both bodies change direction and exchange velocities. The same holds true for the fourth law in which Descartes establishes that if a still body is struck centrally by another body of lesser mass, it remains still, while the colliding body reverses its velocity while preserving the absolute value. On the contrary, the experiment demonstrates that both bodies move with speeds directed in the opposite direction. It was no difficult to provide experimental proof: the monk Thibaut, of the Order of Minors, showed it to be false by playing at billiards, or marbles, on the refectory table. Descartes, too, carried out an experiment on collision that gave the lie to the rules. Deny them and find others more coherent with physical experience? Descartes had too much faith in his own reason and his "clear and distinct" ideas. Experiment disproves the theoretical construction? So much the worse for experience that is unable, as Descartes assuredly claims, because these rules "presuppose that the bodies are perfectly hard and so separated from all the others that there is not one of them that can assist or impede their movements".[134]

But what was the "clear and distinct" idea that drove Descartes to pronounce, immoveable against the most elementary common experience, the rules of motion? We believe that it may be traced to the scientist's correspondence. On the publication in 1644 of the *Principles*, Claude Clerselier asked Descartes to explain the fourth law of motion. The scientist replied in a letter of 17 February 1645[135] that the laws depend on the following criterion, not expressed in the *Principles*: "When two bodies, that

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[134] *Ibid.,* p. 93.
[135] *Ibid.,* Vol. 4, pp. 183-88.

possess the same incompatible modes, meet, it is probable that there will be a change in their modes that renders them compatible, but this change is always the least possible; that is to say, if by changing a certain quantity of these modes, they may become compatible, no larger quantity will be changed". This is, therefore, a principle of the economy of nature: the phenomena of collision occur in such a way that the consequent changes in the motion of the participating bodies have the smallest possible size.

The formulation of the principle is quite murky, at least for us. Descartes applies it distinguishing two "modes" of motion: one is just the movement or velocity, the other is the determination of this mode in a certain sense. The *détermination* of movement is not exactly what we would call today the *sense* of the movement: it is something more; it is the sense united to the velocity in that sense. This comes very close, so much so that it is almost identical, to our concept of "vector velocity", but in general remains distinct due to a vague metaphysical nuance that recalls something like a proposition or the will of the mobile to move in a certain sense.

That said, Descartes justifies the fourth law of motion through reasoning *ab absurdum*: if a still body, struck by another of lesser mass, it begins to move, the two bodies must proceed together with equal velocity that, due to the conservation of the quantity of motion, will be less than half of the velocity possessed by the striking body. Therefore, this would lose more than half of its velocity, and "together more than half of its *determinazione* moving from the right hand to the left, as the *determinazione* is joined to its velocity"; while, on the contrary, if it bounces without moving the colliding body, only all its determination changes, that is a change smaller than half its velocity added to more than half of its determination. "And this - Descartes adds in the letter - in no way refutes the experiment" because, he states, contrary to the *Principles*, in these rules motionless bodies should be intended as a body that is part of a larger hard body. Actually, these are very obscure phrases that perhaps are aimed at justifying the same concept: pure experimentation by the sense, at the foundation of science, may be misleading as it is extraneous to the specific foreign phenomena being studied.

Descartes' seven rules basically correspond to the rules set down by Isaac Beeckman (1588-1637) in his diary, discovered only in 1905.[136] There is however a basic difference: Beeckman believes the rules applicable to inelastic bodies, therefore they are almost completely exact; Descartes, on the other hand, applies the rules to perfectly elastic bodies, therefore they are almost all false. In summary, Beeckman believes in experimental results

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136 J*ournal tenu par Isaac Beeckman de 1694 à 1634,* edited by De Waard, 4 vols, Nijhoff, La Haye 1939-53.

while Descartes believes that he can do without them. In any case, the problem remains of establishing whether Descartes was influenced by Beeckman, with whom he had daily correspondence in Breda in the last months of 1618, until the relationship between the two, both very sure of themselves, deteriorated into enmity. Beeckman was undoubtedly an ingenious man and some of his ideas, expressed only in the *Journal,* can be found in Descartes who, however, always protested that he owed no scientific debt to his old friend. But Descartes always expressed such scornful and disdainful opinions that we may have doubts over the veracity of his claims.

But let us return to Descartes' scientific construction. In both the early *Mondo* and the *Principles,* establishing the laws of motion, Descartes began his cosmological romance, based on the fundamental theory that "the heavens are fluid", and he intends "heavens" to mean the celestial spaces. He explains the formation of the Sun, the stars, the planets, and comets applying his interpretation of the most recent astronomical discoveries, such as the phases of Venus and solar spots. Thin matter, in continuous circular motion, forms vortexes of differing size and velocity by which the parts of ordinary matter are entrapped. The Sun is the centre of a huge vortex around which the vortexes of the other planets rotate, including Earth; but each planet remains immobile at the centre of its respective vortex, and all the changes observed in its position derive solely from the fact that they follow the movement of the heavenly material that contains them.[137] Earth is therefore motionless, as the Holy Bible proclaims. In reply to those who had objected that, as the vortex of the Earth rotates around the Sun, also the Earth moves around the Sun, Descartes would have answered that, according to his definition of motion, it was not possible to talk of mobility of the Earth as it does not move in respect to the thin matter enveloping it. It would seem that Descartes is cheekily suggesting that the definitions are like the rules of a game: if you set them, they must be followed.

From the heavens, Descartes returned to Earth and stated that thin matter has three actions: light, heat, gravity. He thus lays the foundations of the fluid concept of physics that dominated the XVIII century and part of the XIX century. The XVIII century may be termed the "century of fluids". It took fluids as an explanation of every phenomenon: caloric, luminous, electric, magnetic, vital, and so on. But are these useful fluids, like kind elves, helping in inexplicable situations and discretely acting on our senses, not but a return, even partial, to the occult? We would say yes.

All the same, it must be recognised that fluidism was extremely useful to physics, especially in optics and electrology. Fluidism as a scientific

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[137] Descartes, *Works,* op. cit., Vol. 9, p. 113.

concept, a provisional model, a tool for mechanistic philosophy, as we will describe in paragraph 5.8; not the special fluids introduced by Descartes, for example, his magnetic fluid made up of two types of helical particles with three inverted spirals (for which he named them *striate*) with which he explains the thirty-four questions that, in his opinion, may be posed about the magnet. This magnetic fluid, and the following admirable propositions towards the end of the *Principles*, demonstrate Descartes' excellence in constructing hypothetical-deductive systems, but add nothing at all to an understanding of magnetic phenomena.

5.7 Gravity according to Descartes

Three theories on the nature of the weight of bodies were put forward in Descartes's time: weight is an intrinsic quality of the bodies; weight is a force exercised on one body by another body, like the Earth attracts bodies to its surface; weight is a mutual attraction between two bodies, due to the mutual desire to be united as occurs between iron and a magnet. The third theory is similar to the magical concept of sympathy (§ 3.10), according to which the desire to be united arises between similar bodies: for Kepler, for example, the attraction is manifested between the Earth and the Moon but not between the Sun and the planets.

The Cartesian concept of gravity is very different. Descartes rejects all preceding theories. In his opinion, an action of thin matter "is to render bodies heavy, that has a strict relation to that which causes the roundness of drops of water. In fact, it is the same thin matter that, moving indifferently in all senses around a drop of water, pushes in equal measure all the parts of its surface towards the centre and that, owing solely to the fact that it moves around the Earth, pushes towards the Earth all other bodies termed heavy".[138] In other words, every body is situated in a vortex, surrounded in turn by other vortices that all press towards the centre: this pulse towards the centre constitutes the weight of the body, or gravity (Fig. 5.3). All the same, a body termed heavy is not pushed towards the centre of the Earth by all the thin matter surrounding it but solely by that part that rises while it falls. Descartes refers the variability of weight to the distance of the body from the centre of the Earth and finds proof in a number of facts: large birds, cranes and storks, fly high because they become lighter; kites rise from the same reason; the planets do not fall to Earth because their distance almost cancels out the weight; cannon balls, fired vertically, do not return to Earth because they rise so high that they lose their weight. The theory also led

[138] *Ibid.,* p. 210.

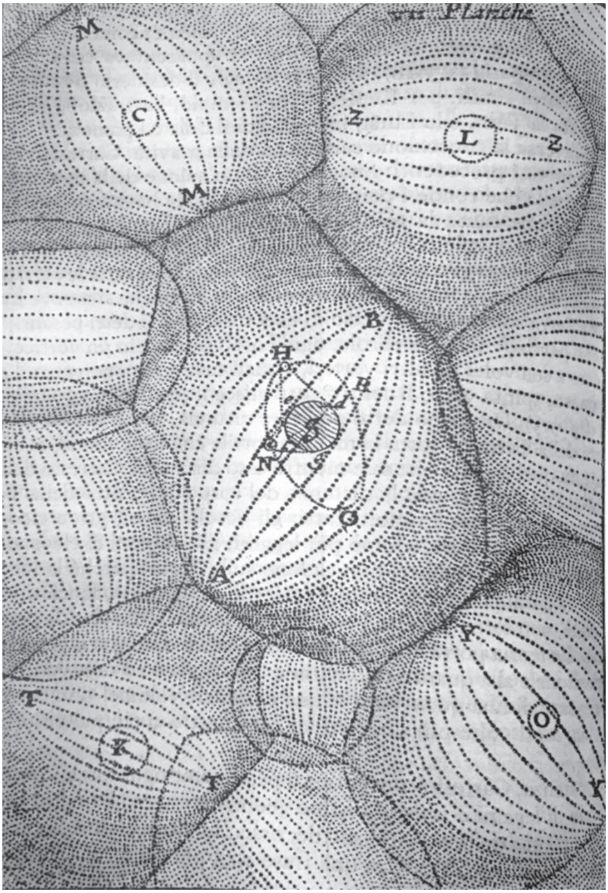

Fig. 5.3 - Vortexes of thin matter as imagined by Descartes: the Solar System is in the centre. *Source* Descartes, *"Works"*, op. cit., Vol.9.

Descartes to conclude that weight is not proportional to the quantity of the matter: "A mass of gold, twenty times heavier than a quantity of water of equal volume, does not contain twenty times more matter, but only four or five times more".[139] Basically, Descartes conceives weight, as every force, as a reaction of bonds or constraints of geometric type: it is a property of the movement of thin matter; that is, by identifying this with space, we may

[139] *Ibid.,* p. 2.

say, in a phrase that is more understandable today, weight is a property of space

The desire to free physics from the residue of magic in all current theories led Huygens to accept the Cartesian doctrine. In his opinion, claiming that gravity is due to attraction is not an explanation but a repetition *idem per idem.* We will describe hereafter Huygens' thinking, closer to physical reality than that of Descartes, following the discussion of gravity held over four sittings at the Académie des sciences in Paris between 7 August and 24 October 1669.[140]

The debate was opened by Roberval with the reading of a brief memoir in which, after having described the various theories on the nature of gravity, he claims that the Cartesian theory is the least reliable. In his opinion, the most probable theory is that of mutual attraction, that would acquire safer foundations if the following consequences were to be proved by experimentation: the weight of bodies, minimum in the vicinity of the centre of the Earth, and gradually increasing with distance to arrive at the maximum at the surface and then decrease with height - the variation in weight should be ascertained by a spring, not with scales (two weeks after the reading of this paper, Roberval read another containing a description of the scale that since then is named after him);[141] a high mountain would deviate from the vertical a plumb line placed close to it.

Three weeks later, Huygens replied with his own paper. He begins, in Cartesian mood, by observing that weight, being a tendency to move, is probably produced by a movement. We need to see the nature of the movement and which body performs it. It appeared evident to Huygens that the motion must be circular as this motion causes a centrifugal force and the body powered by that motion must be a "fluid matter". Descartes had experimentally shored up his theory by emphasizing a centripetal force greater than the centrifugal force in bodies lighter than the rotating liquids in which they are immersed. Huygens rightly observes that this effect is due to the different specific weight of the bodies and therefore Descartes, to arrive at a cause of weight, starts from the supposition that bodies possess one.

To avoid a petition of the principle, a new way had to be found. Huygens assumes a single matter some parts of which do not follow the rotatory motion of the others and follow it more slowly. In space *ABC* a liquid matter rotates around an axis projected in *D* that cannot escape the space because it is held by other bodies. If in this space the particle of matter *E* does not

[140] See L. Auger, *Un savant méconnu: Gilles Personne de Roberval (1602-1675),* Blanchard, Paris 1962, pp. 179-98.
[141] The memoir, unpublished, was later included in *Ibidem.,* pp. 198-202.

follow the rotary movement, or follows it more slowly, it will be pressed by the other parts and will move towards the centre *D.* The phenomenon may be reproduced experimentally by dropping into a container of rotating water a body slightly heavier than the water; when the body hits the bottom, and therefore its circular movement is impeded or diminished, it will move towards the centre in spirals. In these conditions, if the Earth is in *D* and all the surrounding spherical space is occupied by fluid matter moving according to all the possible maximum circumferences, the bodies in this environment that do not follow the rapid movement of the fluid matter are driven towards the centre. "This - concludes Huygens - is what the weight of bodies consists of, that may be said to be the action of the fluid matter circulating around the centre of the Earth in all directions so that it tends to move away and drives the bodies that do not follow this movement".[142] But why do terrestrial bodies not follow this movement? Because, Huygens informs us, the extremely thin and very fast fluid matter crosses all the bodies and drives them in all directions, thereby impeding motion. The theory allows Huygens to deduce that weight is proportional to the quantity of the matter, contrary to Descartes' affirmation; gravity cannot be impeded by any interposed body; the velocity of the falling bodies must follow Galileo's law.

5.8 Cartesian mechanicism

Huygens theory of gravity is without doubt too elaborate and contains too many hypotheses unsupported by experimental proofs. All the same, it is undeniably fascinating: the fascination of unitary theories. It freed physics from purely verbal explanations and attributed all phenomena to mechanical processes that could easily be understood and illustrated. If today we cannot explain the success of Cartesian physics, because the technical results seem modest, the reason is that we have assimilated Cartesian mechanicism to such a degree that we can no longer appreciate its significance and complete breakage with contemporary thinking.

It is usually claimed that Descartes' concept of physics is mechanicistic, but so were those of Galileo and Newton, because the term mechanicistic includes all the theories, sometimes contradictory, that explain any physical phenomenon through a system of movements, such as those of a machine. Two fundamental characteristics differentiate the mechanicism of Descartes from that of Galileo and Newton, or rather Newtonianism. The first, and most evident, is the concept of force. For Galileo and Newton, force is a physical reality irreducible to space and movement; for Descartes, on the

[142] *Ibid.,* p. 187.

contrary, force is, as we have seen, a property of space. Cartesian mechanicism is also opposed to atomism, according to which it is atoms that create the force fields and their hidden movements that explain every physical process. Obviously, by identifying matter and extension, Cartesian doctrine could not be atomist in the traditional sense of Democritus.

The "full" universe satisfies another requisite of our immediate intuition, that is the second distinctive characteristic of Cartesian mechanicism: the contiguity of causal action in space and time. But the efforts of the Cartesians and of an exceptional mathematician like Huygens were not enough to save, in face of Newtonian theory, even the concept of contiguity of causal action in space. The renunciation of this requisite clearly embarrassed even Newton who did not deny, but actually affirmed, that it was impossible, not only in Nature, but also by God himself, to act at a distance, that is act where one is not. However, unable to draw up a satisfactory theory, he limited himself to describing the phenomena that happen "as if" the attraction occurs at a distance; because for him, on the contrary, the Cartesian hypotheses with the intervention of invisible fluids, *pro nihilo sunt habendae* (they are absolutely not credible). But the Cartesian requirement of contiguity of causal action would reappear in physics and play a primary role in James Maxwell's equations on the electro-magnetic field, which we shall deal with later. For now, we will close, and we believe that the foregoing considerations amply justify the conclusion, by affirming that it was not mechanics, intended as the sum of technical results, but the mechanicism of Descartes that enjoyed a wide and deserved recognition in the XVII century and the following one.

5.9 Giovanni Alfonso Borelli

A co-disciple of Torricelli at the school of Castelli, Giovanni Alfonso Borelli was born on 28 January 1608 in Naples or, as others claim, in Messina: curiously, in the frontispieces to his books, he referred to himself alternatively as from Naples or from Messina.

He spent his early years in Rome and in 1635 was appointed master of mathematics in the Studium of Messina, where he remained until 1656, with some interruptions such as the long sabbatical in Tuscany in 1641, during which he met Galileo in Arcetri and made himself known, and praised, in the Galilean circle. From 1656 to 1667 he taught mathematics at the Studium of Pisa, while at the same time being one of the most authoritative members of the Academy of the Cimento (§ 5.24); he later returned to his previous post in Messina. Caught up in the political upheavals, he left for Rome and was welcomed into the court of Cristina of Sweden. He ended his days, in sad poverty, in the Roman college of the Scolopian fathers, on 31 December 1679.

Borelli was one of the most acute minds in 17th century Italian science: his fame is linked to four works on mechanics in which he defends, publicises and promotes the scientific and philosophical thinking of Galileo. In the *Theoricae mediceorum planetarum* (1666), starting from observations of the motion of Jupiter's satellites, Borelli attempts to lay the foundations of the Copernican system. Setting himself the problem of establishing why the planets move around the Sun and the satellites around Jupiter, he rejects the various theories proposed by the astronomers of the time and declares: "Firstly, each planet naturally tends to move closer to the Sun with straight motion, as the weights have a natural instinct to move towards our Earth, driven by a natural force of gravity, like iron moves directly towards a magnet [...] Secondly, let us suppose that the same planet is placed in circular motion around the Sun from West to East: as the circular motion naturally imparts to mobile a certain *impetus* by which it moves away from the centre and is repulsed [...], it follows that as the planet moves circularly, it moves away from the centre".[143] There is, therefore, a dynamic balance between the two causes of motion - towards the centre and away from the centre - so that at any moment the planet is found in a determined position. Guided by this concept, Borelli interprets the elliptical motion of the planets around the Sun. But Borelli conserves a Galilean diffidence towards any magical idea of attraction (§ 4.9): a whole chapter of a later work on mechanics is devoted to arguing that there is no attraction in nature, neither an attracting force. Borelli's fundamental idea is that attraction is an immaterial quality, and so how is it possible that a body moves without bodily contact?[144]

Borelli's theory is, consequently, not yet Newton's theory: not only the fundamental concept of attraction is missing, but also the mathematical formulation of the laws. The novelty lies in considering centrifugal force in the mechanism of planetary motion, whereby, if Borelli's theory is not yet Newtonian theory, it is certainly a good introduction, as Newton himself acknowledged in a letter to Edmund Halley dated 28 June 1680. Furthermore, Borelli predated Newton also in holding that every distinction between celestial physics and terrestrial physics was unfounded: an Aristotelian prejudice that even Descartes could never completely free himself of.

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[143] G.A. Borelli, *Theoricae mediceorum planetarum, ex causis physicis deductae,* Florentine 1666, p. 76.

[144] G.A. Borelli, *De motionibus naturalibus a gravitate pendentibus,* Lugdini Batavorum 1686, op. cit. 6, pp. 166-80. The first edition appeared in 1670.

We will deal with Borelli's contribution to the laws on colliding bodies, set out in *De vi percussionis* (1667), in paragraph 5.11. But the content of the work goes beyond its title. As already set out in the *Theoricae,* and more elegantly in the *De vi percussionis,* Borelli fights against the strange theory of the "quantity of rest" professed by Descartes, which the extremely prudent Borelli does not mention by name. He demonstrates, against "the modern philosophers", that whatever force, however small, as long as it is finite, may move a body however large.[145] He set himself the task of determining the effective motion of the fall of heavy bodies, in the assumption that they are part of the circular uniform motion of terrestrial rotation, ending with eastward deviation. This conclusion was at the heart of his argument with Stefano degli Angeli, whom we mentioned in paragraph 5.1. The controversy was settled in favour of Borelli only in 1791 when Giovan Battista Guglielmini (1763-1817) proved the deviation with the experiments on falling bodies from the Torre degli Asinelli in Bologna.

The principal aim of the *De motionibus naturalibus a gravitate pendentibus* (1670) is to study the effective motion of a body in a fluid, but it also studies many other physical factors, more or less related to the main subject. Borelli sets out a corpuscular theory of the constitution of matter that allows him to interpret the elasticity of air, the mechanical and thermal dilation of bodies, solution, viscosity and more besides. The eighth chapter is dedicated to the study of capillary phenomena, attributed to the adhesion of the liquid to the walls, with the discovery that in the capillary tubes the elevation of the liquid increases with the decrease in the diameter of the tube, a law rediscovered and better detailed in 1718 by the English doctor James Jurin (1684-1750) and named after him. It describes a tool to determine the specific weight of air that is the first example of the use of a constant volume areometer. The contributions to the specific subject of the work - the resistance of fluids to the motion of bodies they contain - are slight and perhaps the best is the observation that, in the free fall of bodies in the fluids, uniformity of motion is not reached very quickly, as Borelli demonstrates in an elegant little experiment: covering the bottom of a vase with wax and then filling the vase with liquid, he drops from various heights a lead ball with a pointed end and observes that the point penetrates the wax more deeply when the height of the drop is greater.[146]

But Borelli's greatest work - and he says that the first two were a deliberate preparation for this - is the two-volume *De motu animalium*, published posthumously in 1680-81 in Rome. The first volume describes

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[145] G.A. Borelli, *De vi percussionis,* Lugdini Batavorum 1686, p. 194. The work was first published in Bologna in 1667.
[146] Borelli, *De motionibus naturalibus,* op. cit., p. 318.

the structure, form, action and power of human and animal muscles as applied to movement. The second volume uses mechanical analogies to describe muscular contractions, heart beats, blood circulation, and digestion. The work, that enjoyed numerous re-editions, marks the beginning of the school of iatromechanics (iatrophysics). Chapter 22 on bird flight (*De volatu)* was particularly admired, so much so that was re-published separately several times. In the XX century, translated into English, it was included in "Aeronautical Classics" (n. 6, London 1911) and, in German, in "Klassiker der exacten Wissenschaften" (n. 221, Leipzig 1927).

5.10 Pierre Varignon

Borelli was without doubt the greatest mechanic of the Galilean school, even though he persisted with some of its errors, as best exemplified by the law of the composition of forces.

Galileo held that the rule of the composition of movements, and therefore the composition of forces, was valid only in the case of orthogonal motion and even in this case with recourse to a cavil consisting in claiming that only "potentially" was the diagonal of the rectangle constructed on the component velocities equal to their sum. The idea is unambiguously expressed in the second theorem of the fourth day of the *Discourses* (VIII, 280). In the case of movements, and therefore their forces not inclined at right angles, the rule of composition, according to Galileo, is no longer valid, because in that case Pythagoras's theory is no longer valid.

Following the example of the master, the Galilean school was extremely diffident towards the rule of the parallelogram; a diffidence that led to Borelli's curious attitude in *De motu animalium.* He wanted to establish the conditions of balance between two weights, *R* and *S* (Fig. 5.4) that, using two cables *AC, BC,* tied to *C*, pass over two pulleys supporting weight *T*. The two forces along the cables are respectively represented by *CM, CN*. Conducted by *M* and *N,* the perpendiculars *MO, ND,* to the vertical for *C*, Borelli demonstrates that equilibrium is obtained when

$$(R + S) : T = (NC + CM) : (DC + OC).$$

It is easy to see that Borelli's rule coincides with the law of the parallelogram. In fact, conducted by *N* the parallel to *CM*, until it meets in *F* the vertical for *C*, joining *F* with *M,* the quadrilateral *NFMC* results in a parallelogram because it has equal opposing angles. Therefore, $OC = FD$, and consequently $DC + OC = FC$. As a result, Borelli's proportionality is

$$(R + S) : T = (NC + CM) : FC$$

that is the Stevin's law or the parallelogram law of forces.

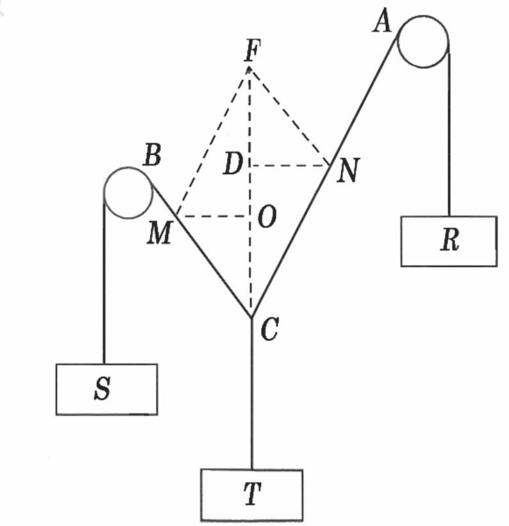

Fig. 5.4

But Borelli was not aware of the substantial similarity of his result to Stevin's, and at this point he introduces a long di But Borelli was not aware of the substantial similarity of his result to Stevin's, and at this point he introduces a long digression aimed at proving that Stevin and Pierre Hérigone (XVII century) followed a path "*non omnino tutam et legitimam*".[147] Making reference to the mathematical demonstrations of the time to prove that the diagonal of the parallelogram is the result of two concurring forces, Borelli recognizes that the demonstrations are correct, but the conclusions are wrong and have been proved wrong by experiment.

The substantial coincidence between Borelli's proposition and those of Stevin and Hérigone was noted by Pierre Varignon (1654-1722) who, in his *Projet d'une nouvelle méchanique* of 1687, added an appendix, *Esame dell'opinione del signor Borelli sulle proprietà dei pesi sospesi a corde.* Varignon paid homage to Borelli's genius, "whose principal works should be listed among the most original of the century", but his recourse to

[147] G.A. Borelli, *De motu animalium,* Romae 1680, Vol. I, p. 3.

experience proves nothing because "the resistance produced by the attrition of the pulleys on their pivots etc. makes possible many conditions of equilibrium in this type of experiments".[148]

Descartes and John Wallis had raised strong methodological criticisms of the traditional Archimedean treatment of statics that reduced the equilibrium of every machine to the conditions of equilibrium of the lever. Varignon, particularly struck by Descartes' reasoning, observed that pulleys and inclined planes are simple machines like the lever, therefore he saw no need to make them dependent on lever treatment. He believed that the traditional form of treatment was simply due to, in fact imposed by, the lack of general principles. A non-traditional treatment of statics therefore required finding a general principle to be applied to all possible machines. He found it in the following fundamental proposition: three homocentric forces are balanced, if each of these is to the other two as the sine of the angle formed by them is to the sine of the angle formed by the force considered with the third. In other words, when three homocentric forces are in equilibrium, their ratios are as the sines of the angles formed by the other two.

In the *Nouvelle mécanique,* that expands the *Projet* and was published posthumously in 1725, Varignon transformed the form of the fundamental proposition into that equivalent of the parallelogram of forces, openly deduced from Aristotelian dynamics. Varignon, regardless the weight of bodies, attrition and the resistance of the medium, theorized forces as the tension of the cables. He proposed that when a body is subjected to any system of forces, either the body will not move or will move according to the resultant of the applied forces, without making a distinction between translational and rotational movement.

Using these generalisations and abstractions, Varignon considers all simple machines, employing ingenious shortcuts that are still taught today. The lengthy treatment of the conditions of equilibrium of levers (87 pages!) concludes with the following proposition: whatever the forces applied to a lever, equilibrium is obtained when the sum of the moments of the forces with respect to the fulcrum that causes the lever to rotate in a certain direction is equal to the sum of the moments that cause it to rotate in the opposite direction.[149] The proposition is the nucleus of what would become known as "Varignon's theorem" of moments.

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[148] P. Varignon, *Projet d'une nouvelle méchanique*, Paris 1687, pp. 88, 90.

[149] P. Varignon, *Nouvelle mécanique ou statique,* Paris 1725, pp. 385-86. In the same work, Varignon gave the modern definition of the moment of a force with respect to a point (*ibid.,* p. 304). Until Varignon, the term "moment" had been used with various and vague meanings.

As a result of the generalisation of the concept of force, the rule of the parallelogram, set out in its simplest form known to everyone today and at the base of the treatment of the equilibrium of every machine, and the particular methods introduced, Varignon may be considered the founder of modern statics. His fame was hampered by the prolixity of the *Nouvelle mécanique* that wanders through two thick volumes of 865 pages to deal with the equilibrium of simple machines in their innumerable individual cases and makes for heavy reading.

5.11 Laws on colliding bodies

The question of "impact forces" occupied Galileo for more or less his entire life and seemed to him to be deeply obscure, particularly because he wanted to compare two different quantities: the force of the collision, that he considered immense, and the weight of the bodies. But, according to Mach, the apparent obscurity of Galileo "is the evident proof of his intellectual insight".[150] Baliani dealt with the same question in his treatise *De motu gravium solidorum* (1638) and coming up against this rock all Cartesian mechanics were sunk (§ 5.6).

A full theory of collision not only lacked Newtonian principles but also the concept of elasticity, that was slowly forming in the first half of the XVII century, connected to the behaviour of air. Consequently, theorists could not describe the process of collision and experimenters did not distinguish elastic bodies from non-elastic ones, so that, depending on the mental or effective experience of one or the other, different properties were attributed.

The eight porisms on collision of the Prague physicist Johannes Marcus Marci (1595-1667) were inspired by the banal experience of players of bowls or billiards, and collected in his estimable and very rare booklet *De proportione motus*. The first states: "If a ball hits another stationary ball of equal size, it moves it and stops" [151]; the other seven theorems describe with similar imprecision the phenomena of collision seen by a billiards player, as Marci must surely have been, resulting from the application of his theories to the resolving questions of billiards.

The incapacity to distinguish between elastic and non-elastic bodies is clearly shown in *De vi percussionis* (1667) by Borelli, written with the declared purpose of reviewing Galileo's speculations "on the nature and property of colliding forces, about which Galileo wrote nothing", where, as we know, Borelli was wrong. Borelli acutely observes that Gassendi,

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[150] E. Mach, *Critical and historical development of mechanical principles,* Italian translation by D. Gambioli, Dante Alighieri. Rome-Milan 1909, p. 334.
[151] J.M. Marci, *De proportione motus,* Prague 1639, p. 46.

Mersenne and he himself had first wanted to compare (as Galileo had done and Borelli did not know) the collision with the weight "of unequal quantities".[152] On the other hand, he more correctly proposes a comparison of the collisions, that is the clash of "*dura et inflexibilia*" bodies whose properties he does not however specify and in any case claims that they do not exist in nature.

This ambiguity, or "polyvalence", of attitude allowed him to express in the corollary of proposition XIX and in propositions XXXIII and XXXIV, theorems that we now acknowledge as valid for non-elastic bodies[153] and to draw attention later, in propositions LX and LXIII,[154] to the collision of elastic bodies. The corollary of proposition XIX states: if a stationary body is hit centrally by another, the ratio between the velocity of the colliding body and that of the collided body is equal to the ratio between the sum of the colliding body and the collided body and the colliding body". It is not difficult to interpret this proposition in modern terms: if v is the velocity of the body with mass A that centrally strikes the stationary body of mass B, it assumes the velocity

$$u = \frac{Av}{A + B}$$

Propositions XXXIII and XXXIV are similarly translated into modern formula and valid, on a first approximation, for the central collision of non-elastic bodies with velocities respectively of contrary or equal direction.

Proposition LX states that if a hard body hits another equally hard and fixed body, it will rebound with a velocity equal, and contrary, to the striking one. Proposition LXIII affirms that if two bodies collide with a velocity inversely proportional to their weights, they will rebound preserving the absolute value of their own velocities.

Borelli's theorems are experimental intuitions, that is idealisations of actual experience carried out on sometimes non-elastic and sometimes elastic bodies. The presumed demonstrations, that sometimes are very unclear, are inadmissible. The demonstration of the last proposition, for example, states that in a collision one motion cannot prevail over another, because each body has an impulsive force exactly equal to the resistance of the other, therefore to continue the movement it is necessary that each body comes back with the same velocity it had before the collision: consequently, as mathematically proven, it is not very convincing! But although we must

152 Borelli, *De vi percussionis*, op. cit., p. 200.
153 *Ibid.,* pp. 56-58.
154 *Ibid.,* pp 90, 92.

consider Borelli's demonstrations non-existent, his theorems of collision show that he had a good understanding of mechanics.

Starting from October 1666, the Royal Society in London began academic studies on the question of the collision of bodies, and opened competition between its members. Wallis, Wren and Huygens all took part. In January 1669, the Royal Society published in its "Philosophical Transactions" the notes of Wallis and Wren, leaving out, in dubious taste, those of Huygens.

The results of John Wallis (1616-1703), mathematician and politician, philosopher and theologian, were inspired by a curious Scholastic-Cartesian theory of mechanics that was not very different from the one Borelli had arrived at two years earlier. Wallis, too, dealt with hard bodies, that is non-elastic bodies, and set collision laws for them that are similar to those of Borelli. He also notes that the laws of the collision of two elastic bodies are different and more complicated. Only in the third part, published in 1671, of his *Mechanica sive de Motu* (1669-71), already having read the notes of Wren and Huygens, did Wallis attempt to deduce to laws of elastic collision from the laws of no-elastic collision, explaining the theory of reflection expounded by Borelli and other minor scientists.

Christopher Wren (1632-1723), professor of astronomy at Oxford University, praised by Newton as a mathematician, is today primarily known as the architect who rebuilt London after the Great Fire of 1666. His contribution to physics is limited to the condensed note on the collision of bodies where, according to the author, he expounds theories deriving from numerous experiments, but the treatment is all the same theoretical, using a particular algebraic symbolism. The theory, that lacks demonstration, is based on two principles: the first is the already mentioned proposition XLIII of Borelli's *De vi percussionis*, which is not acknowledged: the second proposes that if two bodies have an unequal motion, the central collision equals their quantity of motion.

Much more important than all that went before were the studies of Christiaan Huygens. The son of Constantijn (1596-1687), poet and politician, a great admirer and friend of Descartes, Huygens was slow to throw off the shadow of Descartes. But already by 1652 he was certain that all the Cartesian theories of collision, except the first, were false. The note sent to the Royal Society contains his fundamental discoveries on the collision of elastic bodies which, to defend his rights of priority, Huygens published, without demonstrative proof, in the "Journal des Sçavans" of 18

March 1669;[155] a complete treatise appeared in 1703 in a posthumous pamphlet entitled *De motu corporum ex percussione*.

Huygens places the following five hypotheses at the basis of the treatise: the principle of inertia; equal bodies colliding centrally with equal and opposing velocities bounce, maintaining the absolute value of their own velocities; the laws of collision are identical in two systems with uniform rectilinear motion relative to each other; a larger body colliding with a smaller one at rest moves it and loses part of its own velocity; if one of the two bodies involved in the collision maintains its own speed, so does the other.

The principle of relativity was already known and had become the work horse of the Copernicans to refute their adversaries concerning the Earth's movement. Huygens used the principle to provide an ingenious demonstration. For example, to demonstrate that when a body collides centrally with an equal and stationary body, it gives up all its speed and stops, so says Huygens. Let us suppose that a sailor holds in both hands two ropes, to which two spheres A and B are suspended, and sails so close to the coast that his companion can grab the ropes. The boatman moves his hands one against the other at an (absolute) velocity that is equal, and equal to that of the boat, that proceeds in direction AB. At the moment of collision, given the assumption, the two spheres bounce with an equal and opposite velocity to the boatman. But, to the man on the shore, who holds the two ropes at the moment of the collision, events appear different: globe B is still, globe A hits it with a velocity double that imparted by the boatman. After the collision, A stops and B rebounds at the same original velocity as A. In other words, if the boat moves at the same velocity v of the two spheres, the observer on the shore will see that before the collision the two spheres have, respectively, a velocity of $2v$ and 0, after the collision, 0 and $2v$: the theory has been proved. The same artifice is used to prove the successive two proposals.

The theory concerns elastic bodies, but Huygens had not fully understood the mechanism of collision, especially because he still did not understand the concept of elasticity and the Newtonian principles of impulse and the equivalence of action and reaction. His theorems are well known and are still more or less valid today. But one in particular should be remembered as it will become the foundation stone of mechanics: "If two bodies meet, the sum of their size multiplied by the square of their velocity is equal before and after the collision".[156] We do not know how Huygens discovered this

[155] Ch. Huygens, *Oeuvres complètes,* edited by the Société hollandaise des sciences, Nijhoff, The Haye 1888-1950, Vol. 16, pp. 179-81.
[156] *Ibid.,* p. 73.

fundamental theorem, the importance of which was highlighted by Gottfried Leibnitz, as we shall see. However, it is almost certain that his route was not that indicated in the long demonstration that follows the affirmation, conducted, like the rest of the treatise, according to ancient geometry.

An early study of elastic and non-elastic bodies can be found in the *Traité de percussion ou choc des corps* by Edme Mariotte (1620-1684), published in Paris in 1673 and republished in 1676 and 1684. After defining "spring" and "without spring" bodies, Mariotte claims that all bodies are at least partially elastic, as proved in the following lovely experiment: grease an anvil and drop an ivory billiard ball on it; a round impression will appear on the anvil whose diameter increases according to the height of the drop. Mariotte first deals with the collision of non-elastic bodies and then passes to elastic bodies, and arrives at conclusions that are not substantially different to those of Huygens. In fact, Huygens complained that he had been plagiarised: "Mariotte copied everything from me [...] I told him so and he had no answer." he wrote[157]. It is certain that Mariotte took a lot from Huygens, but while the Dutchman's treatment is theoretical, Mariotte's is experimental. To acquire a speed of collision that can be regulated, Mariotte invented a piece of equipment made up of two equal pendulums that can be dropped from regulated heights. He also described the equipment, still used to demonstrate the transmission of motion in elastic bodies, made up of a series of elastic spheres in mutual contact, hanging from connecting threads; the first is moved; it falls against the row and the last ball rises, while the others are immobile. Huygens claims he invented this equipment which was also described by Marci as an illustration of his first porism, referred to previously.

5.12 The pendulum clock

Immediately after discovering the first four satellites of Jupiter (17 January 1610), Galileo had the idea to use them to determine longitude at sea, a question, as is well-known, of great importance to sailors. Theoretically, the procedures should have been very simple: by calculating the ephemerides in a given place that determine the instant in which a certain satellite plunges into the shadow cone of Jupiter, setting the time in which the same phenomenon is observed in another place, you have the difference between the two times and the longitudinal difference between the two places. Application of the method therefore required the construction of the ephemerides and a clock.

[157] *Ibid.,* p. 209.

In 1612, then again in 1616 and 1630, Galileo tried to open talks with the Spanish government to allow him to test his discovery, but to no avail. In 1636, he renewed his proposal to the General States of Holland, who willingly accepted and immediately established a committee and decreed to send Galileo a gold necklace worth 500 florins. The commission made some criticisms of Galileo's project that he found justified but surmountable. However, the negotiation could not be carried out by post and therefore Galileo proposed that a delegation of the General States should visit him in Arcetri. Friends of Galileo went to Contantijn Huygens, Christiaan's father and then secretary to the princes of Orange, asking him to intervene. Constantijn accepted and concluded the deal. But the news reached Cardinal Francesco Barberini who immediately ordered the Inquisitor General of Florence to ban the negotiation. Galileo therefore broke off the talks and refused the gift of the gold chain that a delegation of merchants was delivering to him precisely at the time. However, the negotiation could not be carried out by post and therefore Galileo proposed that a delegation of the General States should visit him in Arcetri. Friends of Galileo went to Contantijn Huygens, Christiaan's father and then secretary to the princes of Orange, asking him to intervene. Constantijn accepted and concluded the deal. But the news reached Cardinal Francesco Barberini who immediately ordered the Inquisitor General of Florence to ban the negotiation. Galileo therefore broke off the talks and refused the gift of the gold chain that a delegation of merchants was delivering to him precisely at the time.

On 15 August 1636, during the negotiations, Galileo wrote to the General States: "I have such a time gauge and if 4 or 6 of these instruments were to be built and left to run we would find that (in confirmation of their exactness) the times measured and demonstrated, not only hour by hour but day by day and month by month, would not differ by even a second, because they work so uniformly" (XVI, 467).

It is not difficult to understand that the "measurer of time" to which Galileo alludes must have been an instrument that exploited the isochronism of pendular oscillations. In a letter dated June 1637 to Lorenzo Réal (Realio in Italian), governor of the Dutch East Indies, Galileo reports that his clock is an application of the pendulum and also describes a special counter of the number of oscillations. Viviani writes that in 1641 "he had the idea that the concept of the pendulum could be adapted to clocks with counterweights and springs" (XIX, 655) and, now completely blind, spoke about it to his son Vincenzo (died 1649). Father and son agreed to build the mechanism, which we can see in Viviani's drawing (Fig. 5.5), with the ingenious escapement called comma or "at rest". It is almost certain that Viviani actually constructed this clock: it is deduced from the inventory of the will of Vincenzo's wife and the letters of Leopoldo de' Medici, who, on 21 August 1659 sent Bouillau a drawing of the same model "with the same roughness with which a model was made that is now in my room".

In a letter dated 12 January 1657, Christiaan Huygens wrote that he had built a pendulum clock. He was given letters patent in June of the same year, and published his findings in the treatise *Horologium* in 1658. Did he, the son of Constantijn who played a large part in the negotiations between Galileo and the General States, and especially knew of Galileo's application of the pendulum to clocks, know of Galileo's project?

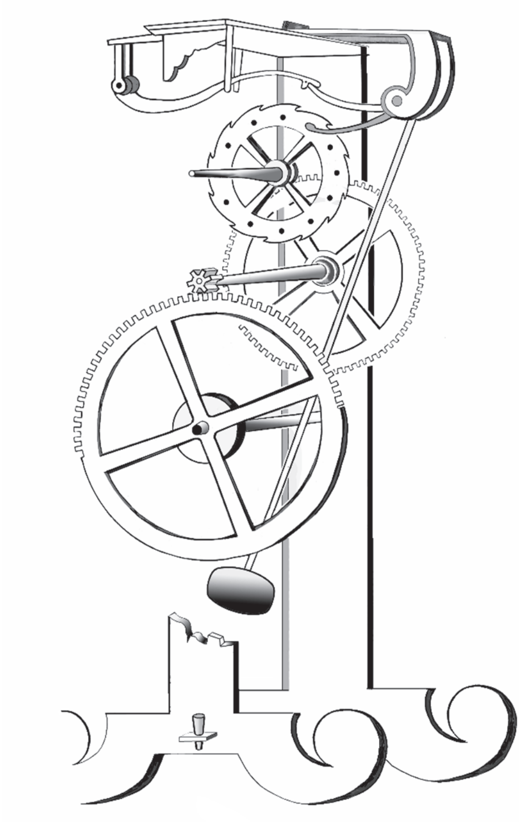

Fig. 5.5 - Galileo's pendulum clock, in a drawing by Vincenzo Viviani.

Huygens always denied it, while at the same time recognised that he had the same idea as Galileo, whose clock went as good as his, also admitting that his aim in making the clock was exactly the same as Galileo's: the determination of longitude at sea.

We can see no reason to doubt the Dutch scientist, whose scientific probity is well known. In making a clock, Huygens is inferior to Galileo when it comes to escapement as he continues with the old defect, but, on the other hand, he is superior by substituting the motorial weight with the spring and the balance.

Fig. 5.6 - Christiaan Huygens

5.13 Christiaan Huygens

Publication of the *Horologium* spread Huygens's fame in mechanics. He was already known in the small circle of Mersenne's correspondents as a prodigious scholar and, still young, was recognised as an exceptional mathematician and astronomer. Born in The Hague on 14 April 1629, Christiaan Huygens (Fig. 5.6) visited France and England; in 1629, Jean-Baptiste Colbert invited him to Paris and included him in the constitution of the "Académie de sciences" in 1666 (§ 5.26).

Huygens remained in Paris and lived a fashionable life until 1681 when, unhappy with the persecution of the Huguenots, to which he belonged, he thought it prudent to return to The Hague and cut off relations with the French academy. In 1689, he paid a visit to Newton in London and on his return published the *Traité de la lumière* (Treatise on light), considered his masterpiece in physics and with which we shall deal hereafter. He died in The Hague on 5 June 1695.

The 1658 treatise on the clock, the start of Huygens' theories of mechanics, has an especially practical basis. In the following years, Huygens studied the numerous related theoretical questions and in 1673 he published, in Paris, a fundamental work, the *Horologium oscillatorium, sive de motu pendulorum ad horologia aptata demonstationes geometricae*, in five parts: description of a clock; the fall of heavy bodies and their movement on a cycloid; evolution and dimensions of curved lines; centre of oscillation and agitation; construction of a conical pendulum clock and theorems on centrifugal force.

Huygens continues the theories of Galileo and Torricelli, that, in his own words, "he corroborates and elaborates". Galileo had founded the dynamics of a single body, Huygens initiated the dynamics of many bodies.

We will pass over the first part of the treatise as it is not strictly related to our subject. In the second part, Huygens goes over Galileo's laws on the fall of weights, detailing the demonstrations with the systematic application of the principle of the composition of motions. The study of the movement of a weight in a cycloid begins with a demonstration of some geometrical properties of the curve and readily leads to the fundamental theory: in a cycloid with a vertical axis and a low vertex, the times of the descent of a moving body starting from a still point at any position of the curve are equal to each other and also equal to the time of the descent over the entire vertical axis of the cycloid. This theory was the origin of the studies of tautochrone curves which 18th century mathematicians studied. Therefore, if a pendulum describes a cycloid, its oscillations are exactly isochronous and not approximately isochronous as in the case of a circular pendulum. But how to apply this mathematical theory to practical mechanics? The answer is given in the third part of the *Horologium oscillatorium* which founds the theory of the evolved, or developed curves, as Huygens says, with the demonstration that the evolved of a cycloid is another equal cycloid, conveniently moved. In the first part of the work, Huygens had ingeniously proposed a way to use the theorem: suspend the pendulum on silk threads that during the oscillations touch two blades shaped in a cycloid arc - pendulum P will thus describe a cycloid and its oscillations will be exactly isochronous, independent of width. The experiment aimed to demonstrate that so much mathematical precision is excessive: it is sufficient that the oscillations of a pendulum are small enough to result as practically isochronous, while the cycloid pendulum presents problems that do not reflect perfect mathematical synchronism. Blessed be the myth of mathematical precision as it resulted in one of the most ingenious treatises on mechanics ever written.

The fourth part begins with a reminder that Mersenne had suggested to the author, when he was still almost a boy, the search for the centre of oscillation, that is the point of the perpendicular to the axis of oscillation drawn from the centre of gravity, distant from the axis of oscillation of a length equal to that of the simple pendulum isochronous with a given compound pendulum.

The concept of the centre of oscillation, defined by Huygens in the preceding form, can be found already in Galileo and was repeated by Mersenne in 1646: if one has a set of simple pendulums, imagined as weights hanging from weightless cords of varying length, all attached to the same bar, the shortest would oscillate faster than the longest; if all these pendulums were to suddenly join together, creating a rigid system, they would be forced to move at the same time, so that the shorter would accelerate the motion of the longer; some pendulums would lose their velocity, others would acquire velocity, while others would neither lose nor gain velocity. He called centre of oscillation the heavy point between these last pendulums that is to be found at the perpendicular conducted from the barycentre of the body to the suspension axis.

In the wake of the preceding considerations, both Roberval and Descartes had attempted to determine the position of the centre of oscillation, driven, up to 1646, to deal with the question raised by the indefatigable Mersenne. The problem was immediately recognised as of the same nature as an older one, proposed by men of arms who wanted to know at which point they should strike the club or sword on the shoulder of the adversary to the greatest effect: that point was called the centre of percussion. Followers of Aristotle said, like Leonardo da Vinci, that the centre of percussion was near the end of the club or the sword because it was here that the motion was the quickest; later, the centre of percussion moved back towards the centre of gravity, the point, it was believed, that concentrated all matter and its effects. But the experiences of clubbing and sword blows, then anything but infrequent, disproved both opinions. On the other hand, the new mechanics taught that velocity increased the weight of the moving body, and therefore the parts of the club, from the handle to the tip, gradually increasing the velocity was like increasing its weight, so that the centre of percussion should be moved to the tip with respect to the centre of gravity.

These ideas inspired Roberval's research published in 1646. He reduced the club to a material segment rotating about its end and considered weight as the moments of the various parts of the segment. By applying the Archimedean doctrine of the equilibrium of levers, he concluded that the centre of percussion of the segment was at two thirds of its length from the centre of rotation. He extended the study to other simple figures, such as isosceles triangles, pyramids and cones. Geometrical planning revealed to Roberval an analogy between his heavy segment and a pendulum rod and led him to claim, with no demonstration, that the centre of percussion and the centre of oscillation always coincided.

For Descartes, it was perfectly clear that there must be a centre of oscillation, like every body has a centre of gravity. But also for him, the centre of gravity and the centre of oscillation were distinct points, because determining the first required only the weight of the different parts of the body, and the determination of the second required also the velocity, that increases with the distance of the point considered from the axis of oscillation. Therefore, to find the centre of oscillation it is necessary to sum the products of each particle of the body by its velocity: taking this approach, he arrived not at the general law as he believed, but one that is valid only for a flat figure oscillating around an axis on its plane. And when it was pointed out that his rule in some cases was very different to experimental results, Descartes attributed the cause to the resistance of air and the axis of suspension.[158]

With the usual acrimony against his rival, Roberval disputed the Cartesian solution in an indirect squabble using Mersenne and Charles Cavendish, an English gentleman living in Paris. Roberval showed that the error in Descartes' reasoning was to consider the agitation and not the direction of the agitation, that is, in modern terms, to consider the momentum as scalar rather that vectorial. On his part, Descartes reproached Roberval for believing that the centre of gravity contributed to the determination of the centre of oscillation, while, on the contrary, the system of weights and the system of forces of agitation are strictly linked.

The polemic between Roberval and Descartes, and the subsequent researches of Wallis, that appeared a little after the publication of the letters of the two French rivals, to which they add nothing, demonstrate that the dynamics of the time were still incapable of solving the problem of the

[158] Descartes, *Works*, op. cit., Vol. 4, pp. 383-92.

centre of oscillation. The same conclusion can be arrived at by examining the particularly simple cases studied by Mariotte in the second part of his cited treatise on collision. Mariotte, like Descartes, used non-Galilean dynamics, based on the postulate: “A body falling in free air begins its movement at a determined velocity, not infinitely small”,[159] and the reasoning of Galileo (§ 4.17) is “highly suspect” because one cannot know whether in the fall of bodies acceleration does not occur in bursts.[160]

The genius of Huygens, who perhaps was unaware of the dispute between Roberval and Descartes, lies in his abandoning of the old and tried ways of confronting the problem. A new path opened to his spirit, Torricelli’s generalised postulate: “When any number of weights begin to move due to the effect of its own gravity, the common barycentre may not rise above that possessed at the start of the movement”.[161] The principle is the same as saying that no weight can rise only by its gravity; in other words, perpetual motion is impossible.

Based on this single hypothesis, with subsequent geometrical propositions, Huygens arrived at proposition V that states: the reduced length x of a pendulum, composed of material points of mass *a, b, c*, and at distances respective to the axis *e, f, g*, is

$$x = \frac{ae^2 + bf^2 + cg^2}{(a + b + c)d}$$

d being the distance from the axis of oscillation of the barycentre of the heavy points *a, b, c*.[162] It is noted in passing that the formula may be extended to any number of material points, therefore it coincides with the modern formula, usually expressed in a different form.

Attention should be drawn to the expression $ae^2 + bf^2 + cg^2$ that almost a century later Euler, in his *Theoria motus corporum solidorum seu rigidorum* (1765), proposed calling *momento d’inerzia* through analogy with the motion of translation: as in this, acceleration is given by force divided by mass, that is by inertia, so in rotatory movement acceleration is given by the moment of the force divided by the moment of inertia of the

[159] E. Mariotte, *De la percussion ou choc des corps,* pt. 2, prop. xi, in *Ibid.*, *Works*, Neaulme La Haye 1740, Vol. I, p. 77. The first edition is dated 1717.
[160] *Ibid.,* p. 81.
[161] Ch. Huygens, *Horologium oscillatorium*, in *Ibid.*, *Complete Works*, op. cit. Vol. 18 (1934), p. 247. In this edition, the original Latin text is accompanied by a facing French translation.
[162] *Ibid.,* p. 259.

body. It would be superfluous to underline the importance of the introduction of the moment of inertia in mechanics.

In proposition XX, Huygens expounds another important theorem: the centre of oscillation and the point of suspension are reciprocal.[163] After providing a number of examples of research on the centre of oscillation of plane figures (circle, rectangle, isosceles triangle, parabola, sectors of a circle, regular polygons) and solids (pyramid, sphere, cylinder, parabolic and hyperbolic conoid, semi-cone), Huygens gave instructions on how to regulate pendulum clocks using a small supplementary weight running along the staff, an invention today used in a vast number of different applications and which Huygens arrived at in 1661.

Huygens' theory of the centre of oscillation was criticised by not a few of his contemporaries. Roberval began the gossip, followed in 1681 by abbot Catelan, a scientist of little value, who all the same managed with his "weak objections", as Lagrange called them, to attract the attention also of Huygens. Catelan substituted Huygens fundamental hypothesis with two others that substantially amount to an admission that the centre of gravity of an oscillating system goes back at a greater height than that from which it felt: if this were true, replied Huygens, the abbot would have discovered perpetual motion.

The argument was joined by Jakob I Bernoulli (1654-1705) and Guillaume-François de l'Hôpital (1661-1704), who first listened to the criticism of abbot Catelan. But with Huygens reply, Bernoulli studied the problem again tracing it back to a static problem and arrived in this way to the same formula as Huygens. As a result, the theory of the Dutch scientist gained increasing credit, both because the same result was reached by different means and because the recent discovery of infinitesimal analysis allowed to deduct the particular rules of Roberval and Descartes from Huygens' general rule. But this rule had been deduced from the principle of conservation of live forces, on which in these years there was a lively argument (§ 5.14), in which Johann Bernoulli (1667-1748), Jakob's brother, and Brook Taylor (1685-1731) independently but at the same time had the idea of researching the centre of oscillation without employing the principle of the conservation of the living force. Both arrived, in 1714, at Huygens' formula, considering the pendulum as a lever resting on the point of suspension. The new formulation of the problem allowed Johann Bernoulli to see clearly that the coincidence between the centre of oscillation and the centre of percussion, admitted without demonstration by all the predecessors, does not always occur. In fact, while a pendulum composed of different parts of varying density has a centre of oscillation that varies

[163] *Ibid.*, p. 305.

according to the medium in which it oscillates, its centre of percussion is independent of the medium.

We will close this parenthesis on the argument over the centre of oscillation by adding that the theory of reciprocity between the point of suspension and the centre of oscillation allows, as we know, to experimentally define the centre of oscillation of the composite pendulum. But it was only in 1818 that Henry Kater (1777-1835) used this theory to construct the "reversible pendulum", that is a practical instrument to determine the length of a pendulum that marks the second and the value of the acceleration of gravity in a given place.

For this latter application of the pendulum, too, we are indebted to Huygens. In 1676, Jean Richer (d. 1696) was very surprised to discover that a pendulum that struck seconds in Paris, when transported to Cayenne, lost time; it was shortened and, at the end of the study, brought back to Paris where, on the contrary, it was fast. In his *Discours sur la cause de la pesanteur*, finished in 1681 and published in 1690, Huygens interpreted the phenomenon as due to the variation in the acceleration of gravity, a variation that he attributed solely to the variation in centrifugal force arising from the rotation of the Earth. This study led him to believe that the Earth must be flattened at the poles and inflated at the equator; to give experimental proof of his observation, he imposed rapid motion of rotation on a soft clay globe impaled on a diametric axis and observed its flattening. It is well known how today the experiment is repeated for teaching purposes with elastic steel rings. The experiment had a notable influence on the genesis of the cosmogonic theories of Immanuel Kant and Pierre-Simon de Laplace.

Perhaps the plan for a conical pendulum clock, briefly described in the fifth part of the *Horologium oscillatorium*, led Huygens to study the nature of circular motion, already dealt with by Galileo, Descartes and Borelli. But whatever the reasons, already in 1659 Huygens had written a treatise on centrifugal force, *De vi centrifuga*, published posthumously in 1703. Huygens begins the treatise with the observation that a body, both free and on an inclined plane. attached to a thread exercises a force on both the thread and the hand holding it. Even a man attached to the edge of a moving wheel, who holds in his hand a short thread fixed to a lead ball feels the traction that is the same as the tendency (*conatus*) of a heavy weight in its fall. For the man attached to the wheel, the thread is always directed to the radius and if the thread breaks, the man sees the attached body moving first in the direction of the radius. The conclusion is that for the man attached to the wheel, any body connected to the axis of rotation is subject to a real centrifugal force, of the same nature as the force of gravity, with which it may be compared. The modern reader may be surprised but also admired by the attention Huygens took in detailing the motion of the observer and the motion observed.

After some geometrical considerations, Huygens summarises: "The *conatus* of a globe attached to a gyrating wheel is that which the globe would have had if it tended to move towards an accelerated uniform motion according to the radius [...]. This *conatus* is similar to that of a weight suspended from a thread. We conclude that the centrifugal forces of unequal mobiles, travelling at equal speeds in equal circles, are related as their gravities, that is their solid quantities [...]. We still need to find the size or quantity of the *conatus* for the different velocities of the wheel".[164]

We can only add that the other laws discovered by Huygens are those that today we may read, with slight variations in terminology, in any elementary treatise on physics and that can be summarised in the formula $F = mv^2/r$, where the letters have their usual significance.

The laws of centrifugal force were reported, without demonstration, in an appendix to the *Horologium oscillatorium*, as we have written. But, even without demonstrative proof, they were to have a notable influence: one only has to remember that Newton used them, and cited them in the *Principia*, for the exact measurement of centrifugal force.

5.14 The argument over life forces

In the treatise on the collision of bodies (§ 5.11), Huygens laid down a principle of conservation, also presented to Leibniz, who, after having written to Huygens, included it in a memoir, published in the "Acta eruditorum" of 1686, *Brevis demonstratio erroris memorabilis Cartesii, et aliorum*, in which he attacked the fundamental postulate of Cartesian mechanics, the conservation of the quantity of motion.

Notwithstanding the revolt against Descartes and the long polemic against the Cartesians, which we will describe later, Gottfried Wilhelm Leibniz, born in Leipzig on 3 July 1646 and died in Hanover on 14 November 1716, shares with the French philosopher, by whom he was influenced, a scientific ideal. Leibniz, too, philosopher and mathematician, historian and politician, distrusted experimentation, like Descartes: the method of research to arrive at the truth is Euclid's deductive approach. This ideal inspired his first publication in 1671 debut with the *Theoria motus abstracti*, contemporary with a *Theoria motus concreti or Hypothesis physica nova*, both impregnated with a spiritualist metaphysics in which it is difficult to divine the content and the mechanical significance. In 1675,

[164] Ch. Huygens, *De vi centrifuga,* in *Ibid.*, *Complete Works,* op. cit. Vol. 16 (1929), p. 267.

he met Huygens in Paris, who became his teacher of mathematics and mechanics, as well as his guide in scientific readings. The meeting with Huygens was decisive: Leibniz became convinced that Descartes' figurative extension alone was not sufficient to interpret nature and that it would not be possible to account for the force of bodies, if something else were not to be proposed other than extension and impenetrability. According to Leibniz, there was no other around the difficulty that frankly admitting the following metaphysical principle: the total effect is equal to the complete cause; according to him, the principle is the means to submit forces to calculation.

Leibniz also declares himself advocate of the principle of conservation of the *vis motrix*, but does not admit that the measurement is the product of mass by velocity of the body. Descartes' memorable error does not consist in having admitted an indestructible *vis motrix*, but in not knowing how to assess it. He allowed, in fact, with Huygens (§ 5.13), that a body free-falling from a certain height reached a velocity that, if it were to be directed upwards, would rise to the same height; he also recognised, with Descartes and Pascal, not quoted by Leibniz, that the force necessary to elevate a pound to the height of four ulnae is the same as raising four pounds to the height of one ulna, therefore the two bodies, respectively of one and four pounds, falling respectively from the height of four and one ulnae, at the end of the fall reach an equal force, although having a different quantity of motion, in the ratio 1/2. Therefore, the force "must be measured by the quantity of the effect it may produce, for example the height to which it may elevate a weight of given size and type, and not from the velocity it may impart".[165] And as the height to which the body may rise is proportional to the square of the initial velocity, so the live force (kinetic energy, in modern terminology) is proportional to the product of the mass of a body times the square of its velocity.

Leibniz's memoir appeared a year before Newton's *Principia*, so the *vix motrix* of Leibniz does not have the significance attributed to Newton's expression. Confusion between the two meanings led to quite a number of misunderstandings amongst contemporary scientists, in particular followers and friends of Newton who, with his grateful approval, attempted to ridicule Leibniz's theories.[166] Criticism of the memoirs of the German philosopher

[165] "Acta eruditorum", 1686, p. 181, republished in G.W. Leibniz, *Opera omnia,* edited by L. Dutens, Genevae 1768, vol, 3, p. 180.

[166] Two fragments from the Newtonian manuscripts showing that Newton agreed with, and perhaps even inspired, the sarcastic criticisms of Samuel Clarke on the concept of live forces were published by A. Koyré and I.B. Cohen in *Newton and the Leibniz-Clarke Correspondence,* in "Archives internationales d'histoire des sciences", 15, 1962, pp. 116 ff.

came immediately in the same year, 1686. An abbot D.C. of Paris and the abbot Antonio Conti countered with the argument that for more than sixty years would be the war horse of the Cartesians: a body with a velocity twice that of another rises to four times the height in twice the time and producing a quadruple effect in twice the time signifies simply having a doubling effect, not quadruple. In a long letter to abbot Conti, Leibniz points out that the fall from different heights may occur in the same times if the planes of descent have different inclinations and the actual force of the body cannot depend on its history, just as the wealth of a man does not have greater value if accumulated over a longer time.[167] Repeating the same observations to abbot D.C., he disdainfully concludes: "I trust that my objection will be studied by a Cartesian who is a geometrician and versed in such questions".[168]

But Leibniz was also a geometrician - and what a geometrician! - who, all the same, in a long reply to another critic, the Jansenist Antoine Arnauld, to demonstrate that the Cartesian law of conservation of the quantity of motion leads to the perpetual motion, states, as unfortunately Descartes had also done, that the quantity of motion is scalar, and when one confuses a vectorial size with its scalar value, one can prove anything one likes. Leibniz was also practised in these problems and, all the same, four years later, again to demonstrate that the Cartesian proposition leads to perpetual motion, he reasoned: a body of mass $4m$ falling from a height 1 and acquiring velocity v will have the quantity of motion $4mv$. If then it imparts this quantity of motion to a body of mass m, this will acquire a velocity $4v$ and may ascend to height 16: perpetual motion would therefore be possible.[169] As we know, Leibniz often abused mental experiences that cannot be physically implemented. But this time, Denis Papin, an astute Cartesian, caught him out at once, and not without irony wrote: if Leibniz "can give me one reason for which, without miracles, we may transfer all the quantity of motion of a larger body to a smaller body at rest, I promise that I or will admit the perpetual motion or I will surrender".[170] Leibniz understood the strength of the objection and tried to defend his demonstration, clutching at straws: it was not necessary that his theory should be proven in fact; besides, if all the quantity of motion cannot be transferred from the major body to the minor, he could admit that the transfer may occur from the minor to the major. If a body of one pound travelling a velocity 4 imparts all its quantity of motion

[167] Leibniz, *Opera omnia,*op. cit, Vol. 3, p. 195.
[168] G.W. Leibniz, *Die philosphischen Schrifen,* edited by C.I. Gerhardt, Weldmann, Berlin 1875-90, Vol. 3, p. 81.
[169] G.W. Leibniz, *De causa gravitatis,* in *Ibid.*, *Opera omnia,* op. cit., Vol. 3, p. 233.
[170] D. Papin, *Mechanicorum de viribus motribus sententia,* in "Acta eruditorum", 1691, p. 9.

to a body of 4 pounds, the latter will acquire velocity 1; and so, the power of the first body that could elevate one pound to 16 feet produces a power that can raise one pound to 4 feet, it is as if the remaining power is lost, or in other words destroyed, without leaving any effect or trace?[171]
The new mental experiment notwithstanding, as awkward as the previous one, Leibniz's thinking was guided by a profound intuition, even if of purely metaphysical inspiration: there exists in nature something that survives in eternity, something that is indestructible willed by the "wisdom and constancy of the Creator".

In the *Essay de dynamique*, Leibniz's clearest treatise on mechanics, written in 1691 but published only in 1860, the philosopher reiterates the necessity of conserving something absolute. In the meantime, he realised that the quantity of motion was a vector, that he called "quantity of progress" and defined as the quantity of movement with which it moves in a certain direction; he had also understood that the "quantity of progress" remains. But the quantity of progress is not an "absolute" thing, that is intrinsic to bodies, because perhaps the "progress" is null even when the force is considerable. It follows, according to Leibniz, that the force, an entity residing in the bodies without relation with the external, is the "absolute", subject to the law of conservation.

In the *Specimen dynamicum*, published in 1695 in the "Acta eruditorum", Leibniz expresses the fundamental relation between the live force and the dead force: this is a simple *conatus* of the bodies, before they have acquired a finite velocity, whereas "the live force is born in an infinity of continuous expressions of the dead force."[172] The correlation is better expressed in a letter of early 1699 to Burcher de Volder (1643-1709): "The *impetus* of the live force rests in the naked solicitation of the dead force like the infinite to the finite, that is like in our differentials the line in relation to its elements [...] in the case of a weight that receives at every moment of its fall an equal and infinitely small increase in the velocity, we may estimate together both the dead force and the live force: the velocity increases like the time, the live force according to the spaces, that is as square of the time, or according to effect: according to the analogy of our geometry or our analysis, the solicitations are dx, the velocities x and the forces xx or xdx".[173]

From Lagrange onward, we are used to expressing this correlation symbolically with the equation

[171] G.W. Leibniz, *De legibus naturae et vera aestimatione virium motricium contra cartesionos,* 1691, in *Ibid.*, *Opera omnia,* op. cit., Vol. 3, p. 253.
[172] Leibniz, *Opera omnia,* op. cit., Vol. 3, p. 318.
[173] Leibniz, *Die philosophischen Schriften*, op. cit., Vol. 2, pp. 154-56.

$$\frac{1}{2}mv^2 = \int F ds \qquad 2$$

that represents Leibniz's greatest achievement in mechanics. In his eyes, the correlation between statics and dynamics provided the measurement of the variety and fertility of his metaphysical principles: in particular the principle of continuity (*natura non facit saltus*) and the equality of complete cause and entire effect.

The argument over live forces continued for at least fifty years, and we shall return to the subject later (§ 7.3).

FLUID STATICS

5.15 Fluid pressure

In the Renaissance the old argument between vacuists and believers in fullness intensified (§ 5.3). Despite the acute criticism of Roger Bacon, the peripatetics continued to explain some physical phenomena, such as the workings of siphons, suckers, pipettes, etc. as nature's *horror vacui,* the cause of an attraction *ne detur vacuum.*

Giulio Cesare Scaligero (1484-1558) opposed this concept and Cardano, rejecting *horror vacui* as nonsense, attributed to the *raritatis violentia* the rise of water in certain experiments, supposing that it was due to the rarefying of air found above water. But this hypothesis was criticised by Porta, who, dreaming of "conducing rivers from the bottom of the valleys to the highest summits of the mountains", estimated that the cause of nature's work "was not the vacuum, not the fear of the emptiness, not the rarefying, not the thinning, but a higher cause, the conservation of its being".[174]

Vacuists, even following different ideas, included Telesio and Bruno, Benedetti and Galileo. Around 1630, one characteristic linked all the vacuists: they admitted the "absolute" weight of air, that is the weight of air outside air. It is a concept that may appear very strange to modern scientists, but for these early Physicists air in air is weightless, like water in water.

Tartaglia wrote, translating almost literally his appendix to Nemorario's booklet mentioned in paragraph 3.12:[175] let it be that no body has its weight

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[174] G.B. Porta, *I tre libri de Spiritali, cioè d'inalzar acque per forza dell'aria,* Naples 1602, p. 19.

[175] Tartaglia, *Jordani opusculum de ponderositate,* op. cit., chapt. 17 *v.* The pamphlet concludes with a description in the vulgate (chapts. 20 *r* -23 *r* n.n.) of Tartaglia's experiments on specific weights carried out between 14 April 1541 and 7 April 1551.

by itself [...], that is water in water, wine in wine, oil in oil, air in air, are without any gravity”.[176]

Cardano, Bruno and Galileo were of the same opinion. “To say - wrote, for example, Galileo in a noted *Discourse* - that water may increase the weight of the bodies placed in it, is truly false, because water in water has no gravity whatsoever, as water does not descend into water”.[177]

Now, negating that a body in its proper place has weight is the same as denying that in a fluid mass there are pressures precisely due to this weight. In other words, the concept of the weight of air did not naturally lead to the concept of atmospheric pressure; in a certain sense it excluded it. A certain idea of the internal pressure of liquids was expounded by Leonardo da Vinci and very clearly, as we have seen (§§ 3.14-15), by Benedetti and Stevin. In the *Paralipomena ad Vitellionem,* Kepler makes a reference to the equilibrium established in a siphon between a glass of water and 15 myriads of myriads of glasses of air: a ratio that is not only gratuitous, but so totally evanescent as to dismiss the idea of extending to air the concept of pressure, already accepted in liquid science. And, in fact, after the Galilean determination of the weight of air (§ 4.6), a further thirty years were needed to arrive at the concept of atmospheric pressure.

5.16 Concept and measurement of atmospheric pressure

Having learned from the fountain-makers of Florence that suction pumps were not able to raise water over 18 arms, Galileo attempted, in the *Discourses,* to explain the phenomenon by substituting the ancient *horror vacui* with the force of the vacuum, that is the resistance, measured by a column of water 18 arms high, offered by the vacuum before it could be produced: “Every time we weight the water contained in eighteen arms of a cannon, both broad and narrow, we have the value of the resistance of the vacuum”.[178]

With the theory of the force of the vacuum, already in 1630 Galileo had attempted to explain the problem of the failed functioning of a siphon that had to ascend a 70-foot hill. The problem was set him by the Genoese Giovan Battista Baliani (1582-1666), a man who “philosophises above nature and laughs at Aristotle and all the peripatetics”, according to Salviati’s presentation to Galileo in 1612, while asking him to write to him. Baliani replied to Galileo’s explanation in a letter of 24 October 1630, expressing his doubt that the behaviour of the siphon could not be

[176] Tartaglia, *Quesiti et inventioni diverse,* op. cit. c. 86 *v*.
[177] G. *Galilei, Discorso intorno alle cose che stanno in su l’acqua o che in quella si muovono,* in *Works, op. cit. Vol. 4, p. 99.*
[178] Galilei, *Discorsi e dimostrazioni matematiche*, op. cit., p. 64.

extraneous to the weight of air, knowledge of which he had gleaned from Galileo and that had led him to believe in the possibility of a vacuum. It is as if we were at the bottom of the sea and would feel pressure in every part. “The same happens - he states - in my opinion in air, that we are at the bottom of its immensity, we feel its weight and compression everywhere, because our body has been created by God of that quality that may well resist to the compression without being offended”, but were we to be in a vacuum and air weighed upon our head, we would feel a weight “very great but not infinite, and therefore determined, and that with a proportionate force may be overcome, thereby causing a vacuum”.[179]

Baliani’s letter and some passages from the aforementioned diary of Beeckman are the first mentions of atmospheric pressure, if we exclude the imaginary mental experiments of Kepler (§ 5.15).

Galileo did not believe he was able to *accept* Baliani’s ideas, but almost certainly he discussed them with Torricelli, as can be seen also in the analogy between the drawings contained in Baliani’s letter and those in the letter Torricelli sent on 11 June 1644 to Michelangelo Ricci (1619-1692) to announce the experiment of “quicksilver”: “We are submerged in an ocean of elemental air, which from experience is known to weigh and so much that this heavy weight near the terrestrial surface is almost a 400th of the weight of water”.[180] Torricelli continues with a description of a famous experiment: a glass tube about one metre long, closed at one end, is filled with mercury, and, stopping the other end, is turned over a basin containing mercury, dipping the opening; removing the finger, the mercury falls in the tube, remaining at a level “of an arm, one cubit and an extra finger”. The experiment, on the instructions of Torricelli, was carried out for the first time by Viviani and was repeated in many jars, some cylindrical, others ending in a ball, and the level reached by the mercury was always the same. To demonstrate that the space over the mercury remained empty, Torricelli made arrive in it water, which filled it completely with “a terrible impetus”. Having described the experiments, Torricelli continues: “The force supporting the quicksilver against its natural inclination to fall, was up to now believed to be inside the jar, or of vacuum, or of that rarefied substance; but I believe that it is external and that the force comes from outside. On the surface of the liquor in the basin weighs the height of 50 miles of air; therefore what magic exists if the quicksilver enters into the glass … and rises so far that it balances the gravity of air pushing it?” The letter outlines the theory of atmospheric pressure, detailed better in a later letter of 28 June 1644 in which Torricelli resolves some doubts raised by his friend Ricci,

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[179] Galilei, *Works,* op. cit., Vol. 14, p. 159.
[180] Torricelli, *Works,* op. cit., Vol. 3, p. 187.

who asked whether the column of mercury would be sustained also if the basin was closed, so that the atmospheric pressure would not weigh upon it. Torricelli replied that, even by putting a lid on the container, the column of mercury would not fall, because the air remaining in the container would have the same external "condensation", in the same way that transversally cutting with a knife a cylinder of compressed wool, the lower part of the wool remains compressed: the highly efficient simile was repeated by Pascal and Boyle, without acknowledging the source.

Ricci asked again how the weight of air that should be exercised downwards can also be manifested upwards. Torricelli jokingly replied, basically expounding that what would be rightly called Pascal's principle: "Once a philosopher who, seeing the pipe placed in a barrel by one of his servants, boasted that the wine would not escape because the nature of weights is to press downwards and not horizontally nor sideways. But the servant showed him that although the liquids naturally move downwards, in any case they push and spurt everywhere, also upwards provided that they find somewhere to go, that is places with minor resistance to their liquid forces".[181]

After these letters, which in fact contain the clear ideas on aerostatics to be found only in Boyle, Torricelli wrote nothing more of his experiments, except for the academic lesson on wind (published posthumously in 1715; Fig. 5.7), in which he explained the formation of winds by the variation of atmospheric pressure, caused by the different warming of parts of the Earth,[182] completing the explanation already provided by Benedetti, who attributed winds to the dense and the rare, provoked by the variation in warming.[183]

Ricci, a cultured man, friend and follower of Torricelli, was the Italian Mersenne; in correspondence with the leading scientists, he also represented a centre of scientific information. Ricci sent a copy of Torricelli's letter to Mersenne who hurried to Italy to witness the experiment. In Rome, in November 1644, he met Ricci, returned to France via Florence where he stayed with Torricelli, who showed him the experiment. After a long voyage through southern France, he returned to Paris and here, at the end of 1645, he demonstrated the experiment to the scholars.

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181 *Ibid.,* p. 199.
182 E. Torricelli, *Lezioni accademiche,* Florence 1715, lezione VII.
183 Benedetti, *Diversarum speculationum mathematicarum et physicarum liber*. cit., p. 192.

LEZIONI
ACCADEMICHE
D'EVANGELISTA
TORRICELLI
Mattematico, e Filosofo
DEL SERENISS. FERDINANDO II.
GRAN DUCA DI TOSCANA
Lettore delle Mattematiche nello Studio di Firenze
E ACCADEMICO DELLA CRUSCA

IL PIU BEL FIOR NE COGLIE

IN FIRENZE M. DCC. XV.
Nella Stamp. di S.A.R. Per Jacopo Guiducci, e Santi Franchi.
Con Licenza de' Superiori.

Fig. 5.7 - Frontispiece of Torricelli's *Lezioni accademiche* (Florence 1715).

The clamour among the scholars raised by Torricelli's tube is equal only to the interest in Galileo's telescope. There arose a violent dispute between peripatetics, cartesians and experimenters. The argument was especially concentrated on the vacuum, to give or not to give credence to it, on the force of the vacuum and on the atmospheric pressure, but for many of the disputants the subject of the argument was still unclear.

In this scenario Pascal, still a follower of the *horror vacui,* repeated Torricelli's experiment, that he had learned from Mersenne. To answer the arguments of a curate, who supported the impossibility of the vacuum, and attributed the Torricellian phenomenon to mercury, a bastard body that one does not know whether it must go up or down, he repeated the experiment in the courtyard of a Rouen glassworks, where he was staying. Here, with two long tubes, one filled with wine, the other with water, he obtained the results foreseen by Torricelli and already achieved by Gasparo Berti in Rome.

An historical experiment was carried out on 19 December 1648 by Florin Perier, Pascal's brother-in-law, on the request of Pascal on the Puy de Dôme, discovering that at the summit of the mountain, the level of the mercury in the tube is lower that at the base of the mountain. Pascal published the experience in a pamphlet, that appeared in the same year under the title *Récit de la grande expérience de l'equilibre de liqueurs*. The "great" experience, that in reality was a simple experimental verification of the Torricellian theory of atmospheric pressure well known to Pascal, seems almost certainly to be that suggested by Descartes in a conversation on the Torricellian experiment that the two philosophers had on 24 September 1647.

More important than the "great experiment" for the affirmation of Torricelli's theory was the experiment of "vacuum in a vacuum", conceived in 1648 by Adrien Auzout (1622-1691), or perhaps by Roberval, and made famous by Pascal in his posthumous treatise *La pesanteur de la masse de l'air,* that refers to it as his own, with a technical variation. It was this experiment, paternity of which was claimed much later also by Jacques Rohault in his *Traité de physique* (1671), that brought over some still unconvinced scientists, such as Roberval and Mersenne, to adhere to the Torricellian theory. Another Roberval experiment is of great help to stir the scientific imagination: an almost deflated goat's bladder, introduced in the barometric chamber, inflates. The experiment inspired Pascal to carry out another, no less elegant, later admired by Boyle: a closed bellows, weakly inflated with air at the base of a hill, gradually inflates as it ascends the mountain and then gradually deflates as it returns downhill.

1648 closes the polemic over Torricelli's experiment and, for all those who accepted the doctrine, there opened a period of application. The first who had the idea of continuous observation of atmospheric pressure in different places was perhaps Descartes who, in late 1647, sent Mersenne, his old fellow student, a strip of graduated paper identical to that which he had applied to the Torricellian tube, asking him to apply it to his apparatus to compare the two series of observations. A little later, Pascal and his brother-in-law Perier started a series of quantitative observations, and Pascal, recognising that the indications of Torricelli's tube were related to the meteorological conditions, attempted to set some rules for weather forecasting, while he understood that Torricelli's tube could be used to determine the difference of level between two locations.

In Italy, similar studies were immediately started by Raffaello Magiotti (1597-1656), but the papers containing the results were burned after his death from the plague. Borelli followed Magiotti's path with a systematic series of researches in 1657-58. He made the apparatus with the device transportable, invented independently also by Pascal, and later called it the *siphon barometer,* that he described in the *De motionibus naturalibus a gravitate pendentibus,* that some date to Torricelli. The apparatus was named by Boyle, perhaps already in 1663, but certainly from 1665, in his *New Experiments and Observations Touching Cold*, indifferently *baroscope* and *barometer.* Mariotte correctly considered the latter term more exact and he used it in *De la nature de l'air* (1676). The balance barometer, constituted of a barometric tube hung from the plate of a scales and from which the variations in the level of the mercury due to the variations in weight are measured, was proposed around 1670 by Samuel Morland, or Moreland, (c. 1625-1696). It was transformed into the barograph in 1791 by Arthur Macquire and brought to a high level of precision by Angelo Secchi (1818-1878) in 1867.

Also metallic barometers, much more sensitive than mercury barometers, were proposed during the XVII century: Leibniz had the first idea which he communicated in 1697 to Denis Papin and later to Johann Bernoulli, but it seems it was not realised, in the form of a holosteric barometer, before 1844 by the Frenchman Lucien Vidi, or Vidie, (1805-1866). Vidi used a metallic box closed with an undulated lid, from which air was extracted; a bar fixed at the centre of the lid acted as the indicator. The aneroid model, shaped as a tube and well-known today, was proposed in 1849 by the maker of scientific instruments Pierre Bourdon (1779-1854), but the instrument had already been in use for some years as a manometer on the German railways.[184]

5.17 Pascal's principle

The studies of Torricelli's experiment naturally led Blaise Pascal to work on fluid statics, the only branch of physics in which he was interested. He was born in Clermont on 19 June 1623. Endowed with an exceptional gift for mathematics, at only 16 he discovered the fundamental principle of the theory of conics, today known by his name. After a brief but intense period in Parisian society, aged thirty-one he abandoned scientific research and retired to the abbey of Port-Royal and dedicated himself almost exclusively to philosophical studies and religious controversies. He died on 19 August 1662 aged just thirty-nine.

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[184] W.E. Knowles Middleton, *The History of the Barometer,* The Johns Hopkins Press, Baltimore 1964, p. 403. This excellent volume is recommended to a reader desiring to learn the long history of the development of the barometer, briefly mentioned here.

Pascal's scientific career practically ended with the writing, between 1651 and 1654, of the two valuable *Traités de l'équilibre des liqueurs, et de la pesanteur de la masse de l'air,* published posthumously in 1663. These are classic treatises due to the clarity of explanation, the evocative nature of the treatment, and the experimental setting, that excuse the author's prolixity and an abundant pinch of boasting.

Pascal establishes that liquids "weigh" according to their height and he gives such a suggestive experimental demonstration that it is substantially repeated in modern times in all physics textbooks: several recipients of different shapes but with bottoms of equal area, filled with equal columns of the same liquid, have equally pressed bottoms. Pascal immediately applied the result to the hydraulic press, already imagined (§§ 3.14-15) by Benedetti and Stevin and taken up by Mersenne. "A recipient full of water - Pascal writes - is a new mechanical instrument, that is a new machine to multiply the forces as much as one wish".[185] Pascal immediately linked this principle to that of virtual workings: "It is clear that moving one hundred pounds of water one inch is the same as moving one pound of water hundred inches".[186]

Pascal establishes the equilibrium of the press, based on Torricelli's principle, without actually citing the Italian scientist: "I start from the principle by which a body may never be moved by its own weight without its centre of gravity being lowered."[187] In all Pascal's examples, the cause of the equilibrium lies in the theory that "the substance situated at the bottom of the recipients, and that places one hole in communication with another, is liquid".[188] The property of completely transmitting pressure is, therefore, characteristic of fluids: physicists call this *Pascal's principle,* even if it is basically known, as we have already observed, to earlier thinkers.

5.18 The pneumatic machine

Otto von Guericke (1602-1686), burgomaster of Magdeburg from 1646 and an able and imaginative experimenter, with a well-developed taste for the theatrical, travelled much in his youth through Germany and France, striking up friendships with scientists that informed him of contemporary research. It is therefore difficult to believe his story that he learned of the Torricellian experiment only in 1654 from a Milanese capuchin monk,

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[185] B. Pascal, *Treatise on the equilibrium of liquids,* Italian translation by F.Nicol, Boringhieri, Turin 1958, p. 39.

[186] *Ibid.,* p. 40.

[187] *Ibid.,* p. 42

[188] *Ibid.,* p. 45.

Valeriano Magno (1586-1661), that in 1647 he had astounded the court of Warsaw by demonstrating the experiment and passing it off as that of the learned and unscrupulous friar.

However, Guericke narrates that he was driven by the vivid desire to prove by experiment if it was possible to make a vacuum. To that end, at a date he does not specify, but perhaps 1650, although German historians date it to before 1641, he filled a wine barrel completely with water, applied a pump to the base and attempted to extract the liquid. When the operation started, the hoops broke; he repeated the experiment with a stronger pump, but that failed, too. He repeated it a third time with a copper barrel. He began the extraction of the piston from the body of the pump, at first easy, but increasingly more difficult. There followed something that terrified the spectators: "All of a sudden - recounts Guericke - to the great uproar and terror of everyone, the globe, like a torn sheet, fell to pieces as if it had been dropped from a very high tower".[189]

Apart from the fright provoked by this violent breakage, Guericke understood to be on the right track to build a machine that Kaspar Schott (1608-1666), publicising it first, with the author's consent, in his *Mechanica hydraulico-pneumatica* (1657) would in fact name *pneumatics*. With later perfectings, he at last managed to construct a machine with which he could empty a recipient of air. The first experiments filled him and those watching with wonder. Some of these were: the one that is still today in textbooks as the "Magdeburg hemispheres" (Fig. 5.8), carried out in 1654 at Ratisbon in front of the emperor and the princes; the experiment of the crevasses; the experiment to demonstrate that sound does not travel in a vacuum; the experiment of the baroscope, still substantially used today for the industrial determination of the density of gases (a hollow sphere is balanced by a full sphere and therefore a lesser volume; the scales overflow on the side of the larger sphere, if the air is denser, and on the opposite side when it is less dense).

We would like to recall in particular an experiment (believed at the time merely proof of the spontaneous dilatation of air) re-studied and applied in the 20th century by Charles Wilson for one of the most valuable pieces of equipment in sub-atomic research: Guericke joins by the neck two recipients fitted with taps, the lower and larger one is emptied and the upper one contains air; he then opens the taps, putting the two recipients in communication, he observes the formation of a mist, a "little sky", in the upper recipient, that first clouds over and then slowly clears.[190]

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[189] O. Guericke, *Experimenta nova (ut vocantur) Magdeburgica de vacuo spatio,* Amstelodami 1672, p. 75.
[190] *Ibid.,* p. 89.

Fig. 5.8 - The experiment of the "hemispheres of Magdeburg". Illustration from Guericke's *Experimenta.*

Perhaps through the quoted volume by Schott, Robert Boyle (1627-1691) learned about Guericke's machine and the related experiments. Boyle, Irish physicist, chemist and theologian, who trained in the study of the works of Galileo, has been one of the most acute scientists of the age. Although he protested his thinking to be independent of Guericke's, he improved the machine, making use of the experimental ability of his assistant Robert Hooke. The most important improvement made to the pneumatic machine was the use of a rack-rail/gear wheel system to make the motion of the piston less laborious.

But making the machine work, leading to a gradual rarefying of the air in the bell, was still difficult. Francis Hauksbee (1660-1713) managed to relieve the labour and consequently obtain better levels of rarefying, coupling a crank to the two pistons, whose running was off-set so that one descended while the other rose. The result was that in a descending piston the atmospheric pressure was equal to the pressure necessary to raise the other piston irrespective of the resistance of attrition, thereby eliminating the inconvenience of Boyle's machine. The ingenious English experimenter added to the bell a slip-knot to which he hung the objects to be tested. He suppressed the troublesome cementing of the edge of the bell by simply placing it on damp leather; finally, he provided the machine with a manometer (the term coined by Varignon in 1705 is not used), constituted of a tube open at the extremity placed under the bell and the other extremity immersed in a recipient containing mercury positioned outside the bell: as the rarefaction increases in the bell, the mercury in the tube rises. To sum up, Hauksbee, more than Boyle, substantially improved Guericke's device, so that his machine, a description of which opens the *Physico-Mechanical Experiments on Various Subjects*, his major work published in 1709, more or less assumes (perhaps second only to the modern vacuometer introduced by Jean-Jacques De Mairan in 1734) the aspect that can still be seen among the historical pieces in physics collections.

The most interesting of Hauksbee's experiments concern capillary phenomena and electric phenomena in a vacuum, and we will deal with them in due course (§ 7.23). Boyle's experiments are not very different to those of Guericke, but the most notable are the determination of the weight of air and the demonstration that in recipients emptied of air there is no combustion or life, not sound, nor does a siphon work.

Boyle published these and other results in 1660 with the title *New Experiments [...] Touching the Spring of the Air*. In a successive work, published in 1686 as a continuation of the previous work, Boyle described a compression pump, in every way similar, in the working principle, to modern compression piston pumps.

The possibility to remove the air from large recipients immediately gave rise to aeronautic plans. The series was begun by the Brescian priest Francesco Lana Terzi (1631-1687) who, in the sixth chapter of the *Prodromo overo saggio di alcune invenzioni nuove premesso all'Arte Maestra* (Brescia 1670), determined as 1/640 the specific weight of air compared to water, and designed a flying machine "lighter than air", made up of four hollow spheres, emptied of air, connected to a boat. Ascent (beyond the level of its equilibrium) and descent are respectively ensured by the jettisoning of the ballast and the partial immission of air in the empty spheres; the terrifying prospects of the possible use of the machine in war were foreseen. This sixth chapter of the *Prodromo*, translated into Latin and reprinted many times over the years, aroused the imagination of technicians and made its contribution to the solution of the problem of aerial navigation.

5.19 Boyle's Law

The title of Boyle's book referred to before recalls the fundamental concept that guided the scientist in the ideation and execution of his experiments: the elasticity of the air, to which we will return in paragraph

5.21. Boyle attributed the tendency of air to expand spontaneously to the phenomena of the apparent re-filling of the recipients emptied of air: it is the air outside the recipients that pushes towards the inside, not the fear of the vacuum nor the force of the vacuum inside the recipients. On the contrary, due to the greater representative power, he imagined the particles of air to be made up of minuscule helicoids that moved as soon as they were free to do so. On the contrary, due to the greater representative power, he imagined the particles of air to be made up of minuscule coil springs that moved as soon as they were free to do so.

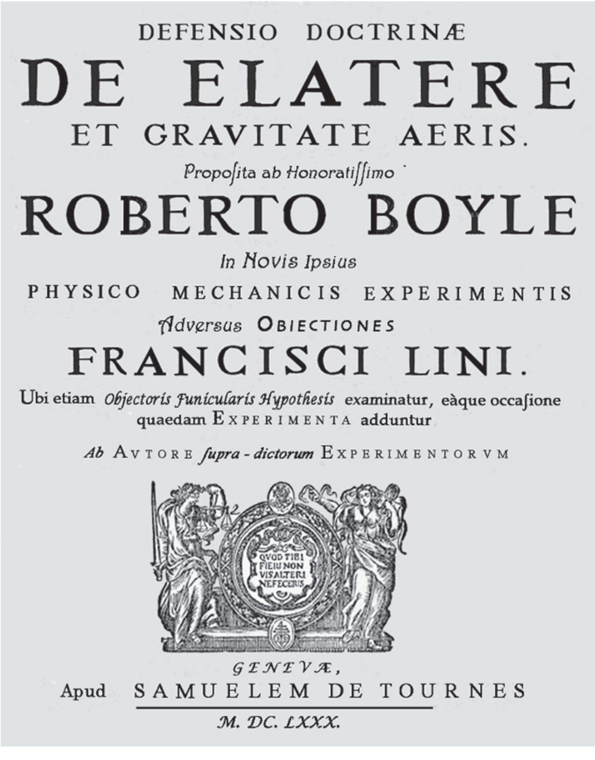
DEFENSIO DOCTRINÆ
DE ELATERE
ET GRAVITATE AERIS.
Propoſita ab Honoratiſſimo
ROBERTO BOYLE
In Novis Ipsius
PHYSICO MECHANICIS EXPERIMENTIS
Adversus OBIECTIONES
FRANCISCI LINI.
Ubi etiam *Objectoris Funicularis Hypothesis* examinatur, eàque occaſione quaedam EXPERIMENTA adduntur
Ab AVTORE *ſupra-dictorum* EXPERIMENTORVM
QVOD TIBI FIERI NON VIS ALTERI NE FECERIS
GENEVÆ,
Apud SAMUELEM DE TOURNES
M. DC. LXXX.

Fig. 5.9. Frontispiece of *De elatere et gravitate aeris* by R. Boyle (Genevae 1680).

The Belgian Jesuit priest François Linus (1595-1675) opposed Boyle's theory, that basically repeated the ideas of Peiresc and Mersenne who had attempted to attribute the barometric phenomenon and the aspiration of water in pumps to particles of air and water that attach themselves to each other. In his *De experimento argenti vivi tubo vitreo inclusi*, published in London in 1660, Father Linus observed that if a tube open at both ends is immersed in mercury, and then the upper end is closed with the finger and the tube is partially extracted from the mercury, the fingertip is drawn towards the inside of the tube. This attraction, Father Linus deduced, does not demonstrate external atmospheric pressure but an internal force developed by invisible strings (*funiculi*) of subtle matter, attached on one side to the fingertip and on the other to the column of mercury.

Ideas like these are laughable nowadays, but at the time they were to be taken seriously, especially when, as in this case, they represented a clear verbal translation of the phenomenology. Therefore, Boyle took Father Linus's criticism seriously and wrote a *Defence against Linus* in which he proposed to demonstrate that the elasticity of air was able to produce much more than the simple support of the Torricellian column: "We will now try to demonstrate, with specially designed experiments, that the elasticity of air is capable of acting much more than is necessary to attribute to it in order to explain the Torricellian phenomenon [...].

We took a long glass tube that in capable hands and with the aid of a lamp was bent at the bottom so that the upper part was almost parallel to the rest of the tube and the orifice of this shorter arm was hermetically sealed, while its length was divided into inches (each one divided into eight parts) using a strip of paper containing the divisions and all accurately glued along the arm".[191]

Experimenting, as we still do today, with this U-shaped tube, he found that when the volume of air in the shorter arm is reduced by half, the difference in the level of the mercury in the two arms was equal to the height of the barometric column, and when the volume of air was reduced to a third, the difference became double the preceding (Fig. 5.10).

In truth, it was not Boyle who understood the importance of this law, but Richard Townley, an amateur (*ingeniosus)* from Lancaster, who repeated the experiments and informed Boyle in a letter that the cause of the phenomenon was the elasticity of air. Boyle published the amateur's observations and spoke of "Townley's law".

To test Townley's law on minor atmospheric pressures, Boyle invented an apparatus that would become a classic, known as the *long bulb barometer*. Using two barometric instruments, Boyle subjected the mass of air closed at pressures varying between 1 and 1/4 inches of mercury and 117 and 9/16 inches; for each experiment, he compared the pressures observed with those that would have been obtained according to the theory of inverse proportionality and discovered a perfect correspondence between the values

[191] R. Boyle, *Defence against Linus*, London 1662, later in Ibidem, *The Works of the Honourable Robert Boyle*, London 1744, Vol. I, p. 100.

observed and the theoretical values, leading to the conclusion: “The elasticity of air behaves inversely to the volume”. Boyle’s pronouncement lacks any mention of the constancy of temperature, a circumstance that would draw the attention of Guillaume Amontons in 1702. Doubts were immediately raised on the general validity of Boyle’s law, and already in 1683 Jakob Bernoulli warned that it could be valid only under certain limits.

Boyle returned to aerostatics in 1666 with the publication of the *Hydrostatical Paradoxes* in which he confutes the ancient doctrine that a lighter liquid does not exercise any pressure on the heavier liquid. The fact is important not in itself but as documentation of the slow assimilation of the ideas.

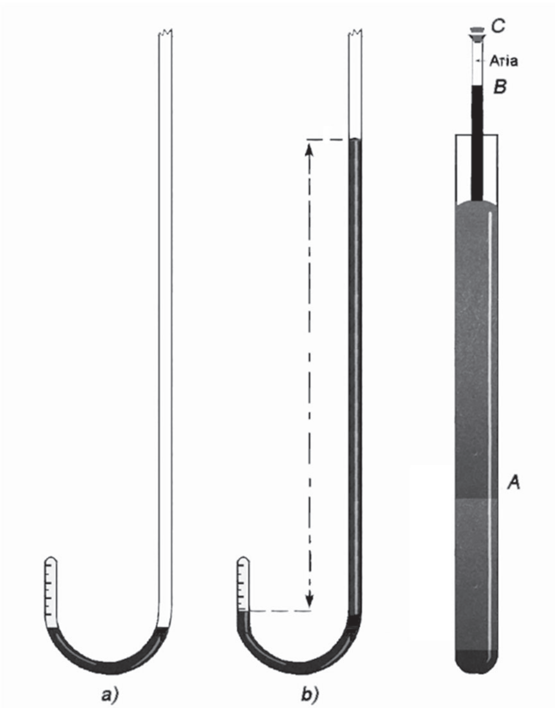

Fig. 5.10 - Boyle’s experiments. On the right, the long-tubed barometer. In the long tube *A* containing mercury, Boyle introduced the narrow tube *B*, previously heated and then closed at end *C*: cooling tube *B* more or less in *A*, the air imprisoned in *B* occupies different volumes and is subject to different pressures, deduced from the different level of mercury in the two tubes. On the left, the bent tube described in the text.

5.20 Altimetric formulae

In 1676, Mariotte, prior of Saint-Martin-sur Baune (Dijon), published his essay *De la nature de l'air* in which he describes experiments that are almost identical to Boyle's and (independently?) comes to the same conclusion: "Boyle's Law", that the French call "Mariotte's Law". But more than Boyle, Mariotte understood the importance of the law, to which he dictated a number of applications. Amongst these, the calculation of the heights using the barometer by means of the differential calculation, that Mariotte's modest mathematical preparation led to failure, is important. Hooke took up Mariotte's calculation, but, no more fortunate nor more prepared in mathematics than his French colleague, deduced that the total height of the atmosphere is infinite: the deduction seemed to him so absurd that he deemed the theory false from the beginning, that is Boyle's Law.

In 1686, the English astronomer Edmund Halley (1656-1742) approached the problem. He is more known for the discovery of the comet named after him. He arrived at a formula that, although being essentially correct, does not take into account the variation in temperature, because, as we have written, Boyle's law does not mention temperature. Basically, Halley's formula states that, while the heights grow in arithmetic progression, the atmospheric pressure decreases in geometrical progression.

It seems that Halley's work went unnoticed, because numerous mathematicians (Giacomo Filippo Maraldi, Gian Domenico Cassini, Daniel I Bernoulli and many others) throughout the 18th century and after studied the altimetric formulae from different points of view. Only Pierre Bouguer, in 1748, returned to Halley's idea. But it was Laplace who treated the problem (1821) in all its generalities, arriving at a highly complicated formula, that was simplified for practical use, and was of great help in rapid levelling, in aeronautics and even in the study of Brownian motion.

5.21 From the elasticity of air to the elasticity of solids

In the previous pages, we have often written of "elasticity". The use of the word is incorrect, even anachronistic, because the term "elasticity" appears in scientific use not before the first quarter of the 18th century, while the concept slowly matures during the 17th century, in connection with the study of pneumatic phenomena, already the pride of the School of Alexandria (§ 1.7).

In classical antiquity and throughout the Middle Ages, there were certainly bows and ballistae; at the end of the 14th century the first sprung watches began to be produced in Europe, perhaps in Florence: in book XVII of the *De subtilitate*, and then in the *De rerum varietate,* Cardano mentions them as a recent invention. During the 16th century, springs of various forms spread throughout Europe, used in a variety of apparatuses, while later elastic gum was imported from the Americas.

All the same, despite the age-old use of elastic bodies, it was not deemed necessary to study the property of some bodies to return to their original state after being reshaped, and even less the need to abstract from this behaviour the idea and term "elasticity". The neologisms *elater, elasticae virtutis robur, vis elastica,* to indicate the spontaneous dilation of air, appeared for the first time in 1651 in the *Dissertatio anatomica de circulatione sanguinis* by the French physician Jean Pecquet. The appearance of a specific word to describe an abstract idea indicates, as often happens, the maturity of the concept that was elaborated above all in Italy.

Starting from 1601, Porta, inspired by the works of Hero, and taking up the theory of the intimate constitution of air, compared it to a dry sponge that, when no longer compressed, returns to its original form and size. Porta describes a series of ingenious experiments that prove the singular behaviour of air: if it is inhaled and blown, through a hole with a straw, in an empty iron hollow sphere, other air may be extracted and introduced in the cavity, in addition to that which it naturally contains; if a well-oiled pointed stick is then introduced into a gun barrel, stopped by a finger over the lower hole, greater force is required for its penetration, a clue that the air of the barrel is condensed and restricted in itself; and if, introducing the stick to the maximum, and then freeing it, it "explodes" at a great force and is launched at a great distance; if, by opening the lower hole and introducing the stick to the bottom of the cane, and then placing a finger over the little hole, we try to remove the wand, we will need a great effort; if, finally, in the previous experiment, the wand, partially extracted from the cane, is left alone, it violently turns back, striking the cane at its end, "because in that state the air was rarefied to its maximum". These experiments lead to the conclusion, in agreement with Porta, that air "by nature compresses and dilates".[192]

If these phenomena of elasticity were the first to be printed in modern times, already in Galileo's letter of 11 January 1594 to Alvise Mocenigo on the working of "Hero's lamp" shows that he believes that the oil in the cup is pushed towards the wick by the elastic force of the air incumbent on the oil.[193] It may be, however, that Galileo was inspired by the Porta's experiments to determine the specific weight of air using the method communicated in 1614 to Baliani (§ 4.6). In the *Discourses,* he observes

[192] G.B. Porta, *Pneunaticorum libri tres,* Naples 1601, pp. 7-9.
[193] Galilei, *Works* op. cit., Vol. 10, pp. 64-65.

that the "condensed" air, if left alone, expands "by impetus" and that, furthermore, fishes have an internal small bladder full of air that allows them, by inflating and deflating, to be balanced in waters of differing salinity and density.[194]

The experiments mentioned here, and those cited in the preceding paragraphs, have demonstrated the elasticity of air. No less probative is another experiment that, at the time, was mostly overlooked, described in 1648 by Raffaello Magiotti in the pamphlet *Renitenza certissima dell'acqua alla compressione*. The experiment consisted of those toys that later, and we do not know why and by whom, became known as *little Cartesian devils*, which demonstrated the incompressibility of water and the compressibility of air.

After Pecquet, in the quoted dissertation, began speaking of the elastic force of air, experiments on this new idea multiplied. The most important were conducted by Boyle who proposed a study of *the spring of the air* (§ 5.18), while shortly after the academics of the Cimento, forever wary of introducing neologisms, studied the "force of the spring" of air. It was also Boyle who began the study of the "spring" behaviour of solid bodies, followed by Mariotte, as we mentioned in paragraph 5.11. In 1679, Hooke laid down the first two laws of the elasticity of solids: elastic force is proportional to deformation (*ut tensio sic vis*); elasticity of tension is equal to the elasticity of compression.

The validity of Hooke's first law was limited in 1749 by Euler who introduced the concept of the limit of elasticity. But the laws of elasticity of traction were established only in 1807 by Thomas Young (1773-1829), who was also the author of the concept of the module of elasticity, defined as the reciprocal of the lengthening of a bar of unitary length and unitary section, on which there acts a unitary force. Without going into the details, we would add that the laws of elastic tension were pronounced by Charles-Augustin Coulomb in 1779, while other notable particular cases of flexion were studied by Franz Joseph Gerstner (1756-1832) on beams supported at one extreme, and by Adhémar-Jean-Claude Barré de Saint-Venant on beams supported at both ends. Elastic hysteresis was discovered by Wilhelm Weber in 1835 during his research on terrestrial magnetism, and successively studied by a number of physicists.

The theory of elasticity, that is so much a part of the science of constructions, would deserve a wider historical treatment. But that wider treatment is outside our mandate, which is more modest and limited to drawing attention to the fact that the study of elasticity recognises its origins in pneumatics, almost to remind us of the unitary nature of physical research,

[194] *Ibid.,* Vol. 8, pp. 114, 123-24.

the intimate links between science and technology, the conventionality of our traditional division of physics into independent chapters.

THE ORGANISATION OF RESEARCH

5.22 Academies and scientific periodicals

During the 17th century, as the prestige of science grew in contemporary society, so did the number of followers. Italy lost its primacy to France, then England, and then Holland. The greater social opening of science, the introduction of a new vocabulary to express new ideas, the need for an easier and more popular language to break down the closed circle of learned doctors, encouraged the use of the vernacular in scientific treatises. This became ever more frequent in the course of the 17th and 18th centuries, becoming almost universal at the beginning of the 19th century. In parallel, French was becoming the international language.

The driving centres of this new mental attitude were no longer the universities. Strictly linked to public powers, universities, save for some rare Italian exceptions (Padua, Bologna) returned to their original intolerance. In 1670, the university of Paris, on the initiative of the faculty of medicine, asked parliament to prohibit Cartesian philosophy, guilty of having spread in France the theory of the circulation of blood, formulated by William Harvey almost fifty years previously. As a result of this mental closing of the universities, the great scientists of the age (John Napier, Descartes, Pierre de Fermat, Pascal, Huygens, Leibniz, to name only the leading ones) steered clear of the universities that had become centres for the conservation of the old science.

The century was characterised by new tools for research and the diffusion of the modern science and these included academies and scientific journals.

5.23 The Academy of the Lincei

The first scholars of natural sciences felt an urgent need for closer collaboration, for regular exchanges of ideas, for personal talks about experimental techniques and the presentation of the experiments. So there began the gradual decline of the myth of the "secret", that is private science, to be replaced by the conviction that the exchange of information helps personal research. Under the impulse of this need there arose the scientific academies, similar to the literary academies.

It is said that already in 1560, Porta had founded in Naples the first academy of physics, the "Academia secretorum naturae", but probably this was not a real academy with statutes and organs, but regular meetings at Porta's house of lovers of peculiar subjects, dear to him, including science, magic and astrology.

Altogether different was the Academy of the Lincei, officially constituted on 17 August 1603 by Cesi together with the Dutchman Johann Eck, Italianised as Ecchio (1577-1620?) with Francesco Stelluti (1577-1651), Fabriano and Antonio De Filiis (1577-1608) of Terni. The aim of the academy was the study and diffusion of scientific knowledge of the physical world, in the widest sense of the term. The academy was to be made up of houses, called "lyceums" distributed across the four corners of the world; its sign was the lynx, whose sight was believed to be so penetrating that it could see inside objects, surmounted by the motto *sagacius ista* ("more acute than this"); members of regular religious orders were not admitted. But the academy was immediately challenged by Cesi's father, a coarse man, who accused Eck of heresy and accused him of wanting to abduct his son and take him to Holland. Consequently, Eck, Stelluti and De Filiis had to abandon Rome and the academy was dissolved.

In 1609, Cesi re-founded the academy, inviting new Italian and foreign members, the leading one of which was Galileo who joined on 25 April 1611. Although is fundamental purpose was the study of natural sciences, the new academy did not ignore "human letters" and many of its members were above all humanists.

Between 1609 and 1630, the year of Cesi's death, the academy flourished and maintained a constant firm attitude of open defense of Galileo's doctrines, so much so that in 1616 it expelled the mathematician Luca Valerio (1553-1618) who had criticised the Copernican position of the Pisan scientist. In the same period, it published some important scientific works, including those we have already mentioned (§§ 4.7-8): the *Istoria e dimostrazione matematiche intorno alle macchie solari e loro accidenti* (1613) and *Il saggiatore* (1623) by Galileo.

After the death of Cesi, attempts to keep the academy going were in vain. After the ephemeral attempts to revive it in Rimini in 1745 and then in Rome in 1795, in 1802 it assumed the name Academy of the Nuovi Lincei, and stuttered along until 1840 when it was dissolved by Pope Gregory XVI. Re-established in 1847 by Pius IX under the title Pontifical Academy of the Nuovi Lincei, was transformed in 1870 into the Royal Academy of the Lincei and, through the particular work of Quintino Sella (1827-1884), president from 1874 to his death, considerably raised its scientific level. Merged in 1939 with the Academy of Italy, it was re-established in 1944 as the National Academy of the Lincei, while at the same time the Academy of Italy was disbanded.

5.24 The Academy of the Cimento

Ferdinando II, grand duke of Tuscany, and his brother Leopoldo de' Medici, together with Viviani and Carlo Renaldini (1615-1698), took the initiative to establish a scientific academy, officially founded with the name "Accademia del Cimento", that is of the "assay" of natural questions, like goldsmiths assay gold and other precious metals. Its president was Leopoldo de' Medici and the first meeting was held on 19 June 1657.

The Academy of the Cimento, like the Academy of the Lincei, aspired to a form of scientific apostleship and its aim was to broaden, through collegiate experimental research, the understanding of physics, following the method started by Galileo, by whose works the Academy was explicitly inspired. Its emblem was a furnace with three crucibles, surmounted by a cartel with the Dantean motto "*provando e riprovando*" (trying and trying again), intended in the sense of "proving and reiterating the proof" (Fig. 5.11)).

Fig. 5.11 - The coat-of-arms of the Academy of the Cimento.
Source: *Essays in natural experiences,* Florence 1667.

Vincenzo Viviani, the brothers Candido and Paolo del Buono, Alessandro Marsili, Antonio Uliva, Carlo Renaldini, Giovanni Alfonso Borelli, Lorenzo Magalotti were enrolled as "active members". It seems that Franceso Redi and Carlo Dati were also members, and there were also numerous Italian and foreign corresponding members.

The activity of the academy - intense in 1657, almost non-existent in 1658 and 1659, rising in 1660 and falling in the following year, and so on in alternating fortunes - was irregular, due to the political responsibilities of its president, the arguments between the academicians, and the lack of rules of association. All depended on the whims of president Leopoldo, in whose apartments the meetings were held on days and at hours that he decided. It was also up the president's pleasure to assign roles to the various academicians. An interesting practice was introduced: the execution of experiments during meetings. This custom was maintained in successive academic institutions to later pass, in the following century, to teaching.

The best part of the scientific activity of the academy was described by Magalotti, the "valued secretary" from 1660, in his famous work *Saggi di naturali esperienze fatte nell'Accademia del Cimento* (Florence 1667), translated into English in 1684 ("*Essays on natural experiments carried out by the Academy of the Cimento*"), into Latin in 1731 and reprinted in 1957 in a photo-lithograph by the Domus Galilaeana of Pisa. A wider description of the work of the academy was given more than a century later by Giovanni Targioni Tozzetti in the four volumes of the *Atti e memorie inedite dell'Accademia del Cimento e notizie aneddote dei progressi delle scienze in Toscana* (Florence 1780). But the diaries of the academy, other writings on experimental physics and astronomy, the correspondence, collected and conserved in the National Library in Florence, are still largely unpublished.

The *Saggi*, a treatise on experimental physics in the modern sense, admired also because of its linguistic precision, begins, after a preface, the scientific treatise with a description of thermometers and the related construction, then passes to the description of hygrometers, barometers and equipment for the application of the pendulums to the measurement of time. There follow fourteen series of systematic experiments: on atmospheric pressure; on solidification; on the thermal variation of volume; the porosity of metals; the compressibility of water; the presumed positive lightness; magnets; electrical phenomena; colours; sound; projectiles.

One of the greatest merits of the academy was the construction of thermometers, derived from Galileo's primitive air thermoscope, transformed into a liquid thermometer (first water, "acquarzente", that is wine spirit) by Torricelli or, as others claim, by Grand Duke Ferdinando II. After the academicians, the construction was so much improved and adapted to various uses that in the 17th century the "Florentine thermometers" became famous, introduced in England by Boyle and spread through France by the astronomer Ismael Bouilliau (1605-1694) who had been gifted an example by a Polish diplomat.

During their studies of heat, and trying to demonstrate that all bodies dilate on heating, the academicians proposed an experiment that is still repeated in schools under the name of "Gravesande's ring"; instead of a cold sphere that just passes through a ring and when warm does not, the academicians used a cylinder. The academy also demonstrated that the thermal dilation of liquids is greater than that of solids and had a clear concept of thermal capacity, even though the related experiments are not published in the *Saggi.*

We will pass over the interesting experiments relating to the resistance of air, the compressibility of liquids and the phenomena of the vacuum in a barometric chamber. We will add only that, having improved the construction of barometers and thermometers, the academicians began systematic meteorological observations, using also a condensation hygrometer invented by Grand Duke Ferdinando II, and sometimes a rain-gauge already proposed by Castelli. The observations were carried out at five determined times of day, first in various locations in Tuscany and subsequently in Milan, Bologna and Parma, noting also wind direction and sky conditions. The study of the data collected by the academy lead to the conclusion that the meteorological conditions in Tuscany in the second half of the 17th century were not dissimilar to current ones.

On 5 March 1667, the academy held its last meeting and in the same year it was dissolved. The reasons for its disbanding are unknown but maybe there were three causes: anonymity of the discoveries, so that every experiment, each observation, each construction or improvement of equipment was never recognised with the name of the author but attributed to the academy; the rivalries and jealousies arising among the academicians, especially the leading ones, Viviani and Borelli; last, the animosity and suspicion of the Roman Curia, that incited rancour between the academicians and derided their work. According to some authors, prince Leopold was promised a cardinalship, that he obtained in the same year 1667, on condition that the academy was dissolved.

Whatever the causes, the end of the Academy of the Cimento was a sad occasion for Italian science. Shortly after (1690), again in Italy, the Arcadia was established, and for almost a century Italian physics had little to say to European science to which it had made a great contribution.

5.25 The Royal Society of London

Returning to England from Italy in 1644, Boyle began to frequent meetings of students of the "new philosophy", or the "experimental philosophy". These "*virtuosi*", as he called them, made up the "invisible college" of which he speaks in letters dating from 1646 and 1647. The custom of philosophical meetings had begun in Cambridge, under the influence of readings of the works of Francis Bacon, followed by Gilbert, Galileo, and Harvey. The meetings, as advisable in the political regime of Oliver Cromwell, were rather more discrete than clandestine, with a careful choice of mostly naturalistic arguments (falling bodies, weight of air, empty and full, eye-pieces, and so forth) that distanced them from any suspicion by politicians and priests. Shortly after 1640, the meetings moved from Cambridge to London and Oxford. The "virtuosi" of Oxford were in close contact with their colleagues in London, where all the activities were eventually concentrated. On 28 November 1660 in London, 12 "virtuosi" decided, perhaps in imitation of the Academy of the Cimento, to set up a society that, officially recognised by King Charles II, in 1662 assumed the name *The Royal Society*, completed in the following year as the *The Royal Society of London for Improving Natural Knowledge*, and adopted the motto "*Nullius in verba*" (On the word of no-one), a phrase from Horace, that acquired a polemical significance in the arguments with Scholastic philosophy.

In the early years, the meetings of the society were prevalently dedicated to the execution of experiments in front of its members and subsequent discussion. Consequently, the society nominated two "curators of experiments". The first to fill the unpaid post was Hooke, at the time assistant to Boyle and later secretary of the society. The museum set up by the Royal Society was also famous and in 1781 it passed to the British Museum, a cultural institution, planned by Hooke in 1676 and founded in 1754 thanks to a legacy of books and money left by Hans Sloane (1660-1753). Another statutory job of the Royal Society was the publication of scientific works: the society's greatest merit in this respect was the publication of Newton's *Principia.*

The purpose of the Royal Society was the search for truth, recognition of mistakes and the path towards the unknown: a programme, wrote Thomas Sprat (1635-1713), bishop of Rochester and first annalist of the society, that does not end "in a season, but is carried out over time: an assiduous, lasting, popular and uninterrupted work".[195]

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[195] Th. Sprat, *The History of the Royal Society,* London 1734, p. 62. The first edition appeared in 1667 and was immediately translated into French in 1669.

How far the Royal Society kept faith with its commitment to the durability of its work can be seen in its enviable continuity: from its foundation to today it has been intimately linked to Great Britain's scientific history.

5.26 The Académie des Sciences of Paris

Parisian scientists were accustomed to meet on fixed days, almost always at Mersenne's apartments, to exchange information and discuss scientific questions of common interest: this coterie was known as the Parisian Academy. On the death of Mersenne, the meetings continued to be held at the home of the referendary Henri-Louis Habert de Montmort (1634-1679) and then in the apartments of Melchisèdec Thévenot (1620-1692). Around 1666, Jean-Baptiste Colbert (1619-1683), a minister of Louis XIV, on the immediate example of the constitution of the Royal Society, understood the advantage, and prestige, to the state if the private society of scientists that had formed around Mersenne were to be officially recognised by the state, as had happened with the French Academy, officially established in 1635. So, in 1666, the "Académie des sciences" was born, with a starting membership of 21, including Huygens, Roberval, Auzout, Pecquet, Jean Picard, Richer, and Mariotte. With the end of the generation of the great French scientists - Descartes, Fermat, Pascal - the activities of the new scientific academy in its early years were modest in quantity and originality, increasing, by reflection, the prestige of the Royal Academy, who counted among its members such exceptional men as Boyle, Newton, Hooke, and Wren. The modest contributions of the French academy of science in the 17th century sometimes bore no fruit also because for more than forty years the academy produced no periodical publication. Only in 1732-33 did Bernard Le Bovier de Fontenelle publish the volumes of the "Histoire de l'Académie royale des sciences" and the "Mémoires" relating to 1666-1717. Two volumes of memoirs had appeared in 1692 and a history of the academy in 1697, prepared in Latin by its secretary Jean-Baptists Duhamel. Regular publications began in 1720 and continued until 1792 with a regular delay of two years: a fact to be borne in mind in the dating of the memoirs published in this period by the French academy.

5.27 The academies of Berlin and Saint Petersburg

In England and France, the scientific academy was conceived with broader aims that in Italy, and more intimately connected with the life of the country. Thirty years were required for a tradition to take root, and the grand academy became a symbol of the country's cultural standing, as had been the universities in the XIII and XIV centuries. The greatest scientists entered the competitions opened by the academies and victory conferred international prestige comparable to the modern Nobel prize.

The example of the French and English academies, drivers of scientific research and diffusion, rapidly extended across Europe in the XVIII century, from Holland to Germany, Scandinavia, Austria, Russia, and Italy. We cannot here report this historical process, that the reader will find in dedicated works but will limit ourselves to mention only the establishment of two grand national academies: Berlin and Saint Petersburg.

Founded in 1700 from an idea of Leibniz, who was its first president, the Berlin academy met for the first time in 1711 under the name of the "*Societas regia scientiarum*", pursuing a difficult and scarce scientific activity until 1740 when, on the ascension of Frederick II, the academy enjoyed a period of great splendour, thanks to the co-opting of leading European scientists: Pierre-Louis Moreau de Maupertuis (from 1741 to 1753) who was president and drew up the reform plans, Euler (from 1741 to 1766), Lagrange (from 1766 to 1787), Johann Lambert (from 1765 to 1777). The official language of the academy was French, thereby establishing an island of high French culture in the heart of Prussia.

In 1724, in Saint Petersburg, Peter the Great set up an academy of sciences, opened the following year by his wife Catherine. Fifteen scientists were admitted, with a stipend and a teaching post, the leading being Nikolaus and Daniel Bernoulli, Georg Bilfinger, Kaspar Friedrich Wolff, Jakob Hermann, and, above all, Euler from 1727 to 1741 and then from 1766 to 1783. The presence of Euler made the Russian academy a centre of high culture that gradually rid itself of the foreign influence to become, and remains to this day (after moving to Moscow), the major national body for scientific research and diffusion. The greatest merit for this transformation in the XVIII century lies with Michail V. Lomonosov (1711-1765), a poet and scientist of genius and from 1741 assistant professor and then full professor at the academy. The museum of science that has occupied the seat of the old academy since 1947 is named after him.

5.28 Scientific journals

During the XVII century, the need for scientific exchange increased the number and importance of letters between scientists, giving rise to the abundant correspondence contained in the works of 17th-century scientists. The letters had the same function as today's scientific papers or, more exactly, the *preprints*. The system became more efficient when some people voluntarily, and without pay, took it upon themselves to collect and distribute scientific news, basically carrying out the uninterested business of modern journals: in Paris, as we have mentioned (§ 5.4), the indefatigable Father Mersenne, replaced on his death by Pierre de Carcavi (d. 1684); in Rome Ricci, elected cardinal in 1681; in London, Henry Oldenburg (1615-1677), were particularly diligent.

But the system had many defects, firstly that of provoking arguments over priority, that perhaps in no other era were so numerous and bitter. Consequently, in imitation of the journals, organs of general information that spread across Europe at the beginning of the century (it seems that the first appeared in Antwerp in 1605), scientific periodicals began to appear in the second half of the century. First in order of priority was the "Journal des Sçavans", published in Paris on 5 January 1665 and forced to suspend publication three months later after the intervention, so it is said, of the apostolic nuncio who protested about an article on the Inquisition. In January 1666, the publication returned and continues to this day, although with periodical interruptions. Besides mathematics and natural sciences, the "Journal des Sçavans" also wrote about literature, history and theology, published original memoirs, wide reviews of works published in France and abroad, news of inventions and discoveries. Its success can be seen not only in its longevity but also in the later editions.

A few months after the "Journal des Sçavans", the first number of the monthly publication "Philosophical Transactions" appeared in London on 5 March 1665. From 1741 onwards, it became the official organ on the Royal Society and was edited by the secretary of the London academy, Oldenburg, and after his death, Hooke. Besides the scope of the periodical, the aims were also identified with those of the academy, so that even though the journal was not its official organ, the two were intimately linked. Thomas Birch's history of the Royal Society is a sort of posthumous supplement to the periodical.[196]

The "Philosophical Transactions" were of a more scientific nature than the "Journal des Sçavans" but the two periodicals maintained a close alliance for over a century, each translating the important pieces and news of the other. The two journals were written in their respective native languages, but the "Philosophical Transactions" also frequently published contributions in Latin and, later, also in French

The use of national languages was an obstacle to the popularity of the journal outside the country of origin, much more serious than today as an

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[196] Th. Birch, *The History of the Royal Society of London,* 4 vols, London 1756-57. It is the most authoritative history of the first hundred years of the Royal Society and has been reprinted in facsimile (Johnson Reprint, New York 1967).

understanding of non-national living languages was less widespread. Every educated man of the time, on the other hand, read Latin almost as easily as his own mother tongue. For this reason, the “Acta eruditorum”, published in Leipzig between 1682 and 1745 in Latin, enjoyed immediate success, also helped by the collaboration of Leibniz and the halo of intellectual liberalism that opposed the conservatism of the “Journal des Sçavans” controlled by the Jesuits.

Following the example of the French journal, in Rome Francesco Nazzari (c. 1634 - 1714), on the advice of Ricci, published, from 1668-1681, the “*Giornale de' letterati*”, a useful collection of works published in Italy and abroad. Nazzari, in collaboration with French scientists, and particularly with Auzout, edited the summaries of the French works. The “*Giornale de' letterati*” was imitated, also in the title, by others that appeared in Parma, Modena, Venice and elsewhere, all with an ephemeral life, of modest scientific interest and a limited distribution that did not extend beyond national borders.

To conclude, throughout the second half of the XVII century, three great periodicals, in France, England and Germany, acted as diffusers, that is the democratisation of experimental science. Although they did not replace, nor intended to, scientific correspondence, that continues to be intense throughout the century, they prepared the literature for the popularisation in the century that followed.

The organisation of science that we have described so far - academies, museums, periodicals - had an enormous task: overcome the hostility of humanists, theologians and politicians to experimental science. This was not as easy as may be believed today. Diffidence towards experimental sciences was still strong, fostered by private interests, such as the protection of traditional university seats. Experimental science was accused of introducing unnecessary changes, of corrupting young people, encouraging them to analyse and discuss everything, of inciting disobedience to government, of damaging the universities, of being a danger to the Christian religion and the Churches, and so on and so forth.

Now we may see these fears were not unfounded: experimental science did indeed have a liberating effect on man, even if that effect was slow and took centuries.

But at the time, it was necessary to convince the fearful that the claimed dangers were mere shadows. That the mission was not easy can be seen, for example, in the third part of the quoted *History* by Sprat that dedicates more than one hundred pages to reassuring the reader of the harmlessness of the study of experimental sciences while praising the advantages. The treatment is very prudent. For example, to demonstrate that the study of experimental sciences does not represent a danger for the healthy eduction of young people, the future bishop of Manchester assures that it is not undertaken by youths but by men who have completed their studies: “The art of experiments - states Sprat - is not charged to boys or obliged to be carried out by beginners in the school, but proposed in an assembly of mature men”.[197]

It was easier for Sprat to glorify the advantages of the study of experimental sciences because the thirst for research and scientific organisation in the century was driven by a deep conviction that became almost a faith: the faith in progress. The concept of progress, that had emerged in the previous century, is characteristic of the new science and one of its founding elements. Overall, medieval science was immobile: it believed that everything had already been said, all ordered and catalogued, without hope of change: *nihil novi sub sole* was its motto. In the 17th century, on the other hand, scientists firmly believed in the immense, inexhaustible horde of secrets held by nature; it was their job to unveil them, one by one, in an endless task. And science, to which Descartes had dedicated the task of improving the material life of mankind by possessing the wealth of nature, would increase the comfort of our existence, invent new machines to relieve the burden of our work. At the end of the century, science rose to being a myth; the “science” par excellence was no longer mathematics, but physics, the science of most immediate usefulness. The poor efficiency of some instruments, the inevitable failures, the shadows of theory did not shackle the hunger for research: progress would make the instruments more efficient, restored after the failures and illuminated the theory. History, often proving the expected optimism, consolidated that faith.

OPTICS

5.29 Kepler

Johannes Kepler (Fig. 5.12), or Keppler according to a less common usage that he himself adopted- was one of the dominant figures of the scientific resurgence of the XVII century: mathematician, physicist, brilliant astronomer, and gifted with an enormous imagination, to which he often gave free rein. He was born in Weilder Stadt (Württemburg) on 27 December 1571 to a modest family and entered the seminary of Tubinga in 1589, moving then to the university of the same city where the astronomer Michael Maestlin (1550-1631) introduced him to the Copernican system. After teaching mathematics at the gymnasium in Graz (Styria) and practising

[197] Sprat, *The History of the Royal Society,* op. cit., p. 323.

astrology and astronomy, in 1599 he moved to Prague together with the Danish astronomer Tycho Brahe (1546-1601), and on the latter's death he became director of the astronomical observatory. His *Astronomia nova* appeared in 1609 and is considered Kepler's most important work in which, from the numerous observations of Tycho, he discovered that the heliocentric vector radius of the planet Mars describes equal areas in equal times: today it is known as "Kepler's Second Law", that chronologically precedes the First.

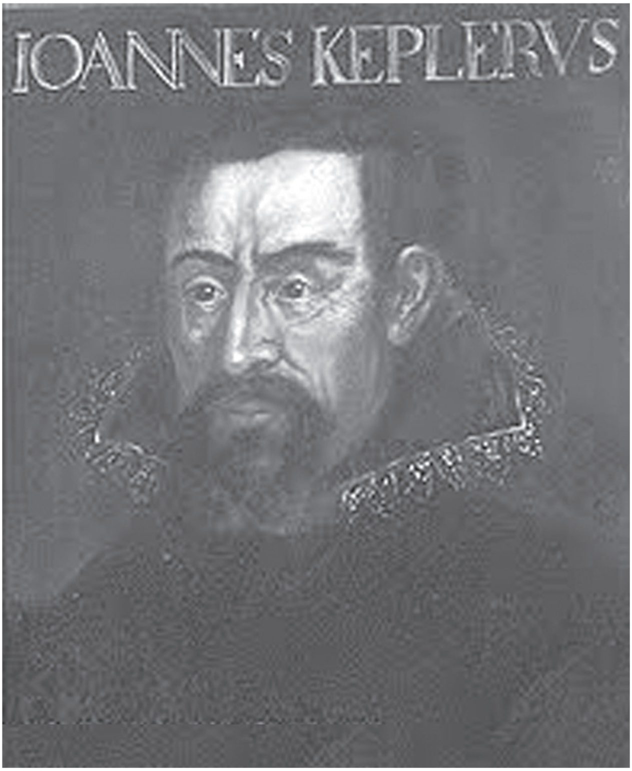

Fig. 5.12 - Kepler. Anonymous portrait.

The trajectory of the planet first appeared to him to be a flattened oval, later he discovered that it was an ellipse in which the Sun occupies one of the focusses: Kepler's "First Law". In the *Harmonices mundi*, published in 1619, Kepler extended the first two laws to all the planets and set out the "Third law": the ratio between the cube of the orbital radius of a planet and the square of its period of revolution around the Sun is the same for all planets.

In the meantime, Kepler had settled in Linz, continuing an unhappy life of poverty that persisted throughout his lifetime, and suffering persecutions to which he was subjected as a Protestant by Roman Catholics. He died in Regensburg on 15 October 1630.

In Kepler's day, physics was practically divided into two sectors: mechanics and optics, that found common ground in astronomy. And to answer the needs of astronomy, Kepler wrote a fundamental work on optics,[198] which he modestly considered, as the title shows, a simple complement to the optics of Vitellione, that is, as we have said (§ 2.8), of Alhazen. Kepler repeatedly states that he was inspired by Book XVII of Porta's *Magia* and the *De recfractione* to which he gives greater merit than do modern critics, and actually proclaims him the "excellent high priest of nature"; it is curious also to observe how many of Kepler's ideas can be found in Maurolico, whose writings on optics had not yet been published.

For our history, the first six chapters of Kepler's work are of greatest interest as the last five deal with astronomical questions. Kepler, taking up the ideas of Alhazen, removed from optics simulacra and the like, and considered that cones of rays are emanated in all directions from every point of light. With these rays, he explained a problem that had been a mystery for all preceding opticians: why in a mirror one sees images where they certainly are not? Because, responds Kepler, and with him today's physicists, the eye cannot know the path of the rays striking it, therefore it refers the point of light to their prolonging. A similar explanation is given to the localisation of images seen through refraction, and therefore simply explains the experiment of the broken stick, that had been unexplained for thousands of years: *macula foeda in pulcherrima scientia,* as Kepler has it.

The fifth chapter is dedicated to refraction. With an ingenious experimental tool, Kepler attempts to define the law of refraction, but in the end he settled to rely on the old rule attributed to Ptolemy (§ 1.12): for angles less than 30°, the angle of refraction is proportionate to the angle of incidence; the ratio of proportionality depends of the refracting medium and for the glass is 2/3, while the critical angle between the glass and air, adds a successive proposition, is 42°. The rule was most useful to him in the study of refraction through a sphere, to which he introduces a new and very important experimental technique. Kepler recognised that it is not the same thing to view images with the eye or receiving them on a screen, and he realised that the latter experimental approach is very simplified and more objective. To

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[198] J. Kepler, *Ad Vitellionem paralipomena, quibus astronomiae pars optica traditur,* Francofurti 1604.

sum up, he replaces medieval optical physiology with modern geometric optics. And by combining this technique with Barbaro's idea to diaphragm the sphere (§ 3.17), Kepler discovered a fundamental fact: also in refraction through a diaphragmed sphere, an object point corresponds to an image point, and a band of parallel rays "converges" (and the use of the word in this specific sense can be attributed to him) in a point that he called, for the first time, *focus.*

The sixth chapter is the most famous of the work: it deals with the mechanism of vision. More audacious than Alhazen, Maurolico and Porta, Kepler had light striking the retina, an idea that Benedetti had arrived at before him[199], and of which he was unaware. He recognised that the image had to be formed upside down, but he did not believe that this fact implied seeing objects in reverse: it was enough for the eye to locate the luminous point above when the stimulus is below and locate to the right the point that gives the stimulus from the left; and *vice versa.*

Kepler's work did not arouse interest in the cultural circles of the time; certainly in 1610, Galileo did not know of him, therefore we may exclude that the *Paralipomena* in some way influenced the construction, or even less the use, of the telescope. On the contrary, Kepler's reserve in the first announcement of Galileo's astronomical discoveries leads to us to believe that even he had little trust in the spyglass. But Kepler's reserve, while justified, was soon overcome by the exaltation of Galileo's astronomical discoveries and the drafting, in August and September 1610, of the *Dioptrice (*Dioptrics) published in 1611, whose purpose was to propose the theory of the telescope, thereby supporting the indications of the senses by mathematical demonstrations.

The *Dioptrice* is based on the geometric optics expounded in the *Paralipomena* but broadens and details them, and above all applies them to the study of lenses, the function of the crystalline lens in the eye, the correction of myopia and presbyopia. Kepler then passes to the study of the combination of several lenses, clearly expounding the idea that the image of one may function as the object of the other; he applied the results to a description of a convex telescope, today known as astronomical or Keplerian. Proposition LXXXVI offers a very simple reasoning: the real and reversed image given by the lens functions as the object of the ocular and this, if used as a magnifying glass of the real image, gives an even larger virtual image. Kepler thus demonstrates that he well understood the theory of convex lenses. The same cannot be said for concave lenses (studied in Propositions XC to C), as can be seen in Proposition XCVI in which, to demonstrate that concave lenses give reduced images, he states that rays

[199] Benedetti, *Diversarum speculationum,* op. cit., pp. 296-97.

refracted by the lens converge towards the eyes, as if the concave lenses had a real focus like convex lenses. Although he succeeded in describing the tool now known as a zoom lens - insufficiently constructed by Ignazio Porro (1801-1875) in 1851 and successfully realised only in the first quarter of the 20th century - in proposition CVII Kepler provides a theory of the Galilean telescope that is very inadequate. Basically, he hopes to get away with saying that the concave lenses form confused images due to the excessive divergence of the rays, while convex lens images are unclear due to excessive convergence; therefore, by coupling a convex lens with a concave one, the two opposing excesses are compensated (*se mutuo tollunt),* therefore resulting in clear vision:[200] sight will be clear, but the theory is confused. It seems that the Keplerian telescope was effectively constructed in 1630 by Christoph Scheiner. But the Neapolitan Francesco Fontana (c. 1580-1656) claims priority in the construction that he would have executed "by practice" in 1608, but he wrote about it only in his *Novae coelestium terrestriumque rerum observationes*, published in Naples in 1645 - too late for the claim to be taken seriously due to the lack of other proofs.

5.30 The law of refraction

With Kepler's two works, science had acquired the elementary geometric optics of our time, with the exception of one fundamental law: that of refraction. Galileo's astronomical discoveries had rendered optics a subject of great contemporary interest, while Kepler's works removed many parts, especially the theory of sight, from the domain of philosophy.

In this cultural environment, Descartes, excited by the invention of the telescope, with the declared, denounced and failed purpose of improving its construction, perhaps in the hope of emulating and exceeding Galileo in astronomical discoveries, began studying optics. He well understood that the basic problem of the renewed science was the theory of light. In fact, the first "discourse" of the *Dioptrique* has the promising title "De la lumière". Unfortunately, it is only the title. The scientific fantasy of the philosopher was not up to the task of dealing with such a complex problem. Although he promised an explanation of "all" the known properties of light and a deduction of "all" the others, he declared that it was not necessary for him to explain the true nature of light, but that his ultimate aim was to explain vision and telescopes using two or three analogies. The first analogy was twenty centuries old: just as a blind man, tapping with his cane, is aware of objects, so light "is a certain motion or a certain action" that passes from the

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[200] J. Kepler, *Dioptrice seu demonstratio eorum quae visui et visibilibus propter conspicilla non ita pridem inventa accidunt,* Augustae Vindilicorum 1611, p. 56.

luminous body to the eyes through air and the other transparent bodies. The second analogy, contradicting the first, endows light with a material nature: just as two currents of must exit undisturbed from two holes in the bottom of the barrel full of grapes, so the currents of subtle matter proceeding from the Sun towards our eyes do not disturb each other, and are not disturbed, by ordinary matter. The third analogy belongs to Alhazen: a ray of light may be compared to a material projectile. Descartes continues with these three analogies, using them alternately, and speaks of the rectilinear propagation of light, transparency, reflection and diffusion. The ideas cannot be said to be "clear and distinct", because it is not clear at the end what Descartes intends by light: objective or subjective? Movement or matter? This first "discourse" of the *Dioptrique* is so unclear and vague that a mathematician, and also a Cartesian, like Huygens had to confess that he did not understand what Descartes wanted to say about the nature of light. The scientists concluded, too quickly in our opinion, that the *Dioptrique,* presented by Descartes as an example of the application of his "method" proves instead its failure.

The second "discourse", or we have come to say, the second chapter of the *Dioptrique,* deals with the reflection and refraction of projectiles, following the example of Alhazen. The results are arbitrarily extended to analogous phenomena of luminosity. Cartesian reasoning can be summarised in the following manner.[201]

[201] R. Descartes, *Discours de la methode pour bien conduire sa raison, et chercher la verité dans les sciences. Plus la dioptrique les météores et la géometrie qui sont des essais de cette methode*, Leyden 1637; later in *Ibid.*, *Works*, op. cit., Vol. 6, pp. 96-105.

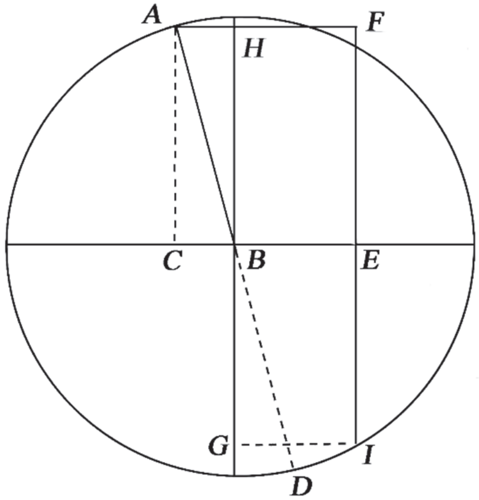

Fig. 5.13

A marble is launched from *A* towards screen *CBE* (Fig. 5.13), it breaks the screen and passes through, losing some of its velocity, for example, half. Now, the determination of the motion of the marble (by *détermination* Descartes means, more or less, the speed vector) may be imagined as composed of two determinations: from top to bottom and from left to right. The screen cannot change the latter to which it makes no opposition, but varies the former. Setting the circumference at centre *B* and radius *BA,* as the marble, by hypothesis, has lost half of its velocity, it must move through space *BI* under the screen in double the time taken to cover the equal space *AB* above the screen; but, as the component in direction *BE* has not changed, when the marble arrives at *I,* the space *BE* travelled must be twice *BC*. Therefore, making *BE* twice *BC* and conducted by *E* the parallel to *BH*, point *I* of the meeting of the parallel with the circumference is that towards which the marble moves once it has gone through screen *CBE.* If, on the other hand, the marble, arriving at *B*, does not encounter the screen, but is struck by a racquet that increases its vertical velocity, by analogous reasoning it may be deduced that point *I* must be lower than *D.* Conversely, it may be concluded that, depending on whether *I* is lower or higher than *D*, the velocity of the marble in the second part is respectively greater or lower than the first. If it is not possible to follow the geometric construction, because $BE = 2\ BC$ is greater than the radius, the projectile does not penetrate the screen, but is reflected (the case is not explicitly described by Descartes, but is obvious).

"Finally, as the action of light follows the same laws as the movement of the marble", it may be said that when its rays pass obliquely from one transparent body to another, they are refracted in such a way as to form with the refracting surface a greater angle on the side in which the body moves more easily. But the bending of the refracted ray must be measured by the ratio between segments *AH* and *GI,* irrespective of the inclination of the rays: it is the law of sines.

This Cartesian treatment was accused of plagiarism and contradiction. Willebrord Snell van Roijen (1581-1626) had already theoretically found and experimentally demonstrated the law of refraction, so Leibniz and Huygens, together with a number of others, accused Descartes of having published Snell's law, which he knew of, as his own. There is no documentation; it is speculation and not worth dealing with, especially when we think of the fundamental concept, the real discovery that allowed Descartes to arrive at his law: the decomposition of the speed of light. But Alhazen had already made that discovery. Descartes took up the discovery and gave it mathematical form: this is such a convincing internal history that perhaps it is useless to go back over other inductions.

The contradiction, to which we alluded before, results from the fact that while Descartes supposed the speed of light to be infinite, which he then decomposes and changes a component, which obviously is significant if a finite speed is presumed. This irrefutable contradiction was always deemed a scandal by historians. In our opinion, this may be explained, without too much cavilling, by recalling the utilitarian concept of science, attributable to Descartes, that necessarily implies a certain dose of empiricism. On a theoretical level, Descartes had to admit an infinite speed of light, because a finite speed, joined with the speed of the Earth, would have produced the phenomenon that would later be called aberration, discovered by James Bradley only in 1725. But, on the other hand, meditating on Alhazen's writings, already in 1627 Descartes had discovered the simple law of refraction, searched for without success for two thousand years. Having found it, he subjected it to numerous experimental tests: the tenth "discourse" of the *Dioptrique*, for example, describes an apparatus to determine the index of refraction of glass, devised much earlier than the work was drafted; the eighth "discourse" of the *Météores* gives a value of the index of refraction of water compared to air: 250/187 = 1,331, that is still today a good average value. This law of refraction allowed him to lay down what he believed to be the best profiles of lenses; this law allowed him to explain the rainbow. It was, in short, a rule that was, in his hands, very fertile, Descartes had found it starting from a mechanical analogy, and had hoped to be able, over time, to frame it in a theory of light. His hopes frustrated, should he renounce the law? Should he renounce that which, according to him, should have been the fundamental scope of the science, the search for economic usefulness? With the failure of the attempt to construct a theory of light, Descartes falls back on analogies that, in his words, "help to understand in the easiest way" the nature of light; using models that do not aim to be theories, but provisional working hypotheses, representative and not explanatory. Descartes explicitly says that his models are all false or uncertain, but all the same he believes to have arrived at true and useful consequences, "imitating in this way - he adds - the astronomers who, although their suppositions are all false or uncertain, since they refer to their observations, deduce from them many very certain results".[202] In sum, in our opinion, Descartes too - Descartes the rationalist -, on this occasion joins the ranks of "opportunist" physicists, a group that includes in this century Pascal, Huygens and Newton, to mention only the most famous.

Before dealing with the brilliant application of the law of refraction to explaining the rainbow, we must examine a great change contained in the first "discourse" of the *Dioptrique.* The light of the physicists of the first half of the 17th century was a colourless light, in the real sense of the word. Even Kepler believed that colour was distinct from light, a "quality" that needed further study by philosophers. And the philosophers had said and continued to say things about colours that seem incomprehensible to us: that colour is a quality residing in the surfaces of bodies; that it previously exists and is visible potentially, and is made actually visible through the power of external light; that it is the diversity of the limitation of the diaphanous and the opaque. The Aristotelian theory is more solid, professed up to Newton: colours, he said, are a mixture, in variable proportions, of light and dark; red is produced by an alternation of true light and darkness; violet by a mixture of dense shade and weak light; the other colours are a mixture of red and violet.

Descartes threw out the theories of Aristotle and all the other related *philosophemes* and affirmed that we distinguish different colours due to the action of the light striking our eyes. More explicitly, in the eighth discourse of the *Météores,* Descartes declares: "The nature of colours consists in nothing else but the fact that the parts of subtle matter, that transmits the action of light, tend to rotate much more strongly than moving in a straight line, so that those moving more strongly give rise to the colour red and those that are a little weaker create the colour yellow [...] And all this reasoning perfectly agrees with the experience that I did not believe it possible, after

[202] Descartes, *Works,* op. cit., Vol. 6, p. 83.

having understood both, to doubt that the phenomenon is not that which I explained".[203] We will leave aside a technical analysis of this text to return to the fundamental, and truly innovative, concept: colour is a physical-physiological phenomenon caused by the various sensations aroused in us by the changing rotation of light particles.

One of Descartes' most felicitous moments as an experimenter is his experiments on the formation of a rainbow, described in the eighth "discourse" of the *Météores.* All modern treatises contain the Cartesian explanation of the rainbow, that Newton completed and that 19th-century science, through the works of Thomas Young, George Biddel Airy and Josef Pernter, did not change but refined to take into account the phenomena of diffraction and interference, unknown in Descartes time. But modern treatises have a relatively easy task, without recourse to the experimental part, because they can rely on the concept of minimum deviation, that came to light in optics only with Newton and, even more so, in 1725, with Euler. But Descartes, who did not know the phenomenon of minimum deviation, first had to experiment, as had De Dominis, with a spherical glass phial filled with water and lit by the Sun's rays. He noted that when the visual that went to a certain point of the phial made an angle of 42° to the direction of the incident rays, this point of the phial took on a bright red colour; if that angle was a little smaller, it successively assumed other colours. Then, with a small screen that covered the various parts of the phial, he managed to determine the band of incident rays that we say are in a position of minimum deviation and to trace their path inside in the phial. It was at this point that, to clarify his ideas on the nature of colours, he compared the colourings of the phial with those obtained using a glass prism, which provided him with the means to construct, on an experimental basis, the theory of colours that we have mentioned. The precious law of refraction then allowed him to explain, with a long numerical calculation, the reasons for that well determined angle of 42°, formed by the incident rays and emerging from the phial. The explanation of the rainbow was therefore found (Fig. 5.14), with a series of well thought out experiments, accurately executed and subjected to calculation: a true master class in modern physical investigation.

[203] *Ibid.,* pp. 333-34.

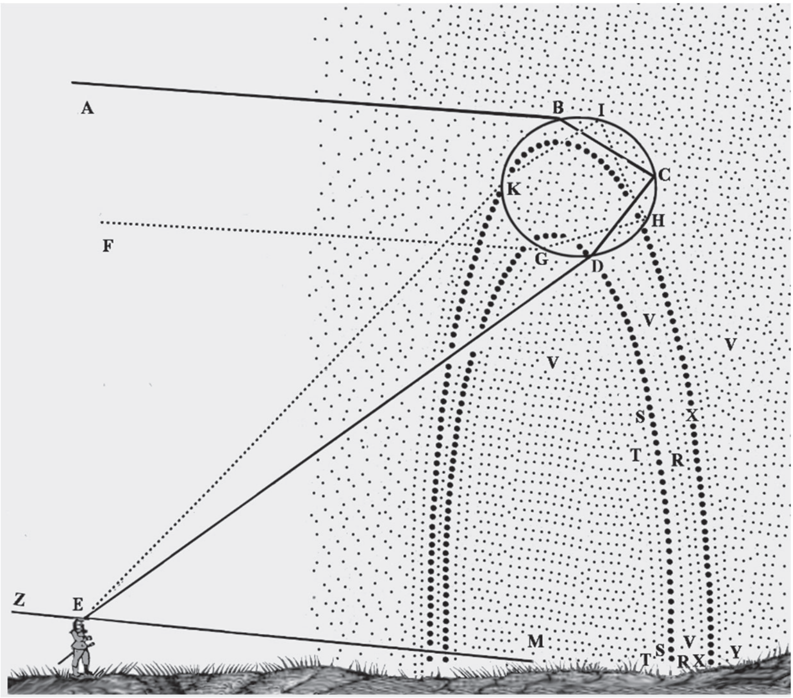

Fig. 5.14 - Cartesian explanation of the rainbow: solar ray *AB* is refracted, is dispersed, and is reflected in the drop of water and strikes the observer in colour according to *DE*.

5.31 Theoretical research into lenses

Descartes' law of refraction, criticised even before it was published, slowly spread through the 17th-century scientific world. In 1647, Cavalieri does not use it in his *Exercitationes geometricae* even though he was aware of it. And Cavalieri was repeatedly occupied with lenses and he must have been famous in the Galilean scientific environment as an expert if Torricelli, at the beginning of his career as a maker of telescopes, turned to him for advice on constructing lenses.

In 1632, in a short study of refraction, Cavalieri had observed that "the lenses, that are only a little full, will almost together be parabolic and hyperbolic",[204] disappointing those who, from Porta to Descartes and others, had hoped that lenses different to spherical ones could prove to be more efficient. Moreover, he experimentally proved the formulae of the focal distance of lenses given by Kepler in the *Dioptrics*. He had calculated the focal distance f (measured from the forward face of the lens) as a function of the radius of curvature r, for flat-convex lenses ($f = 3r$, if the first face is the convex one; $f = 2r$, if the first face is the flat one) and bi-convex ($f = r$) with equal convexities of the radius;[205] for the "meniscus" (he introduced the word), he settles on concluding that the further the focus moves away from the lens the thinner the lens.[206]

Cavalieri returned to the question, dedicating almost an entire exercise, the sixth, to it. He proposed unifying Kepler's rules in a single general rule, assuming Kepler's as the law of refraction. Translated in modern symbols, the general law is as follows.[207] If r_1 and r_2 are the radii of the curvature of the surfaces and f the focal distance, measured from the leading face of the lens, we have

$$(r_1 + r_2) : r_1 = 2r_2 : f$$

which coincides with the modern

$$\frac{1}{f} = (n-1)(\frac{1}{r_1} + \frac{1}{r_2})$$

for n =1.5, the value attributed by Kepler and Cavalieri to the refractory index of glass with respect to air.

The demonstration separates into various cases according the nature of the lens (flat-convex, flat-concave, bi-convex, bi-concave, convergent meniscus, divergent meniscus); each case is sub-divided into two sub-forms, depending on whether the light strikes one face or the other. In the case of flat surface, that is an infinite radius, the demonstration is conducted *per quandam analogiam,* where the analogy consists in operating indifferently on the infinite as on the finite.

Although we may not pursue Cavalieri in his lengthy demonstration, it may be opportune to describe his method at least in on case, simplifying the

[204] B. Cavalieri, *Lo specchio ustorio* (The burning glass), Bologna 1650, p. 0. The first edition of the work dates, as we have said, to 1632.
[205] Kepler, *Dioptrics,* op. cit., pp. 10-16.
[206] *Ibid.,* pp. 71-72.
[207] B. Cavalieri, *Exercitationes geometricae,* Bononiae 1647, p. 462.

argument by using modern symbols. After rediscovering Kepler's rules on flat-convex lenses, Cavalieri turned to the question of bi-convex lenses. If *G* and *L* (Fig. 5.15) are the centres of curvature of two spherical bowls *CAD* and *CFD*, both with a width of less than 30°, with respective radii of r_1 and r_2, ME being the incidental radius parallel to the axis that, if the surface *CFD* were flat, would be refracted according to *EI* and, in the case of a flat-convex lens, it would be $IA = 3\ r_1$. Meeting the spherical surface, it refracts according to *EP: P* is the focus and $PA = f$ the focal distance. Conducting *LE,* by the law of refraction, the result is

$$O\hat{E}I = E\acute{L}I + E\hat{I}L = 2 \cdot I\hat{E}P \qquad [1]$$

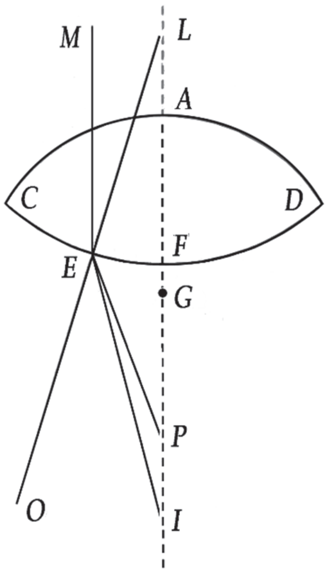

Fig. 5.15

Applying to the triangles *IEP, IEL* the theory of sines and replacing the sines by the angles, the resulting proportions are

$$IP : EP = I\hat{E}P : E\hat{I}P \qquad IE : EL = I\acute{L}E : E\hat{I}L \qquad [2]$$

and composing the latter

$$(IE + EL) : EL = (I\acute{L}E + E\hat{I}L) : E\hat{I}L$$

and for [1]

$$(IE + EL) : EL = 2 \cdot I\hat{E}P : E\hat{I}L$$

from which, for [2] we get

$$(IE + EL) : EL = 2 \cdot IP : EP \qquad [3]$$

Now, for the small thickness of the lenses, we may write:

$$IE = IA = 3r_1 , \quad PE = PF = PA = f$$

and $IP = IA - PA = 3r_1 - f$. Substituting the measurements in [3], we obtain

$$(3r_1 + r_2) : r_2 = 2 \cdot (3r_1 - f) : f$$

equal to

$$(r_1 + r_2) : r_1 = 2 \cdot r_2 : f$$

as Cavalieri aimed to demonstrate.

The mathematician makes the light refract only once, on the second surface of the lens; he also introduced too many approximations to the calculation. All the same, the historical experiment demonstrated that things, on a first approximation, happen just so.

In 1693, the astronomer Halley repeated the calculation, taking into account the thickness of the lens and the refraction on both faces, but still assuming very small angles, so as to substitute their sines. He obtained very complex formulae that, for thin lenses, by ignoring the thickness, reduced to the Cavalieri's formula for the focal distance and to

$$\frac{1}{p} + \frac{1}{q} = \frac{1}{f}$$

with p and q as the respective distances of the lens from the object and the image, to arrive at the “formula of lenses”.[208]

5.32 Construction of optical instruments

Not only Descartes, as we have mentioned, but almost all the century’s physicists (Auzout, Huygens, Hooke, Viviani, Borelli) built their own telescopes. Some of them even sold examples with the aim of augmenting their meager income. This close collaboration between theoretical and practical optics, characteristic of the time, was a driving force for the progress of both.

In the twenty years after the publication of the *Sidereus nuncius*, the best telescopes remained Galileo’s, that the Girolamo Sirturo (from Milan, died around 1631), author of a *Telescopium, sive ars perficiendi novum illud Galilaei visorium instrumentum ad sydera* (Francofurti 1618), could not improve on, nor equal, despite the many technical artifices contained in his little work. But around 1630, a rumour spread in the Galilean circle, and Galileo himself was alerted to it by Micanzio, Castelli, Cavalieri, and others, that the Galilean supremacy had fallen, or was declining, through the work of a Neapolitan, Francesco Fontana (c. 1580 -1656), who was not “a man of letters, but through continuous work and construction of telescopes, is said to have fallen on such a singularity that for the things in heaven is a miracle”.[209]

The really excellent Fontana’s telescopes stimulated the emulation, not to say jealousy, of Torricelli, who was guided and advised by Galileo in the manufacture of lenses. In October 1642, he had “much surpassed mediocrity”, but after just over a year he claimed to have constructed two telescopes “better than the Grand Duke’s perfect one built by Fontana”, receiving as reward from the Grand Duke a chain of 300 *scudi* and a medal with the motto *virtutis praemia*.[210]

Torricelli attributed the merit of his telescopes to a “secret” contained in some papers left as he was dying to the Grand Duke and passed on to Viviani. Some historians judged Torricelli’s “secret” to be fantasy. In our opinion, the scientist’s letter to Magiotti and the letters left by Viviani lead

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[208] E. Halley, *An instance of the Excellence of the Modern Algebra in the Resolution of the Problem of the Foci of Optick Glasses Universally,* in “Philosophical Transactions”, 17, 1693, pp. 960-69.
[209] Micanzio’s letter to Galileo of 1 July 1638, in Galilei, *Works,* op. cit., Vol. 17, pp. 363-64.
[210] Torricelli’s letter of 6 February 1644 to Magiotti, in Torricelli, *Works,* op. cit., Vol. 3, pp. 165-66.

to the Torricellian "secret" that basically consists of, in addition to smidgen of vanity, a very accurate work on surfaces ("the figure is very important") in selecting a good quality of glass (up to the second half of the XIX century, the quality of glass was the greatest obstacle to the construction of good optical instruments) and not attaching the glass "with pitch, or other materials by means of fire". But this recommendation to not alter the shape of the lens, that was known, according to Torricelli, only to him and to God,[211] was made public twenty-five years earlier by Girolamo Sirturo.[212]

Nonetheless, Torricelli constructed excellent telescopes, although it may be an exaggeration to claim they were the best of the time, as he said, because in his time very good lens makers were active: besides Fontana, there was also Eustachio Divini (1610-1685) of Rome, and Ippolito Mariani, known as "il Tordo" (thrush), and Antonio Novelli. And from Paris, Mersenne warned Torricelli that telescopes were being built in France better than his and that Gassendi owned one. Each constructor, obviously, boasted the excellence of his product; comparisons were difficult due to the limited number of examples produced in different places and their use by people of varying expertise. The arguments were numerous.

Important progress in the construction of telescopes was made by Christiaan Huygens, with the help of his brother, Constantijn. In the hope of removing iridescence from lenses, he replaced the simple eye-piece with two convex lenses, joined by the flat parts. Some years later, Newton, despairing of being able to correct the chromaticism of spyglasses, introduced the telescope, with which we will deal later. With his spyglass, which enlarged one hundred times against the thirty of Galileo's piece, Huygens discovered Saturn's first ring, that he announced in 1659. That the coupling of the two lenses in the construction of the eye-piece had corrected the chromaticism of the telescope was an illusion on the part of Huygens; all the same, the artifice certainly inspired the solution provided by Peter Dollond in the following century.

Even the scientists who built telescopes for commercial purposes never thought of establishing industrial-type laboratories for the construction of optical instruments. The first examples of organisations of this type are found in Rome, with Divini and Giuseppe Campani (1635-1715) who, in truth, were not just good technicians, but also experimenters and observers of celestial phenomena. Other less known constructors worked in Livorno, Florence and Genoa.

The use of the simple microscope, that is an enlarging lens, must be as old as glasses for long-sightedness. But before Galileo, the diffidence of

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[211] Torricelli's letter to Magiotti of 4 December 1643, ibid., p. 152.
[212] G. Sirturo, *Telescopium,* Francofurti 1618, p. 48.

philosophers towards every new instrument interfering between nature and the observer was so great that the use of magnifying lenses for scientific purposes was almost non-existent (§ 4.5). Although the questions of priority are still on-going, certain testimonies attest that already in 1614 Galileo was using a compound microscope, that he called *occhialino* ("little lenses") constructed in imitation of the spy-glass (convex objective and concave eye-piece). The microscopic observations made by Cesi using an eye-glass given to him by Galileo are also famous. But a number of technical difficulties impeded the construction of the compound microscope: the bad quality of glass that resulted in confused images; the chromatic aberration that was much more troublesome that in the spy-glass; the need to illuminate the object; the necessity of a system to support and move the objective. Therefore, in the first half of the XVII century very few examples of compound microscopes were built and none has come down to us. Even theoretical descriptions of the instrument are few and often confused with the description of the spy-glass; the most noted theoretical description of the time comes from Descartes, using parabolic lenses.

Only in the second half of the century was the compound microscope truly constructed and used: it seems that around 1660 it could be easily found for sale. The lenses were set in moveable tubes; the enlargements were modest, between 30 and 40. In Italy, compound microscopes were built by Divini and Campani; in Holland the brothers Christiaan and Constantijn Huygens placed a field lens between the objective and the eye-piece; in England Hooke, who made many observations to which we will return later, published in his *Micrographia* (London 1665) the first treatise on microscopy that is important also in the history of the microscope. The works of Marcello Malpighi (1628-1694), the founder of microscopic anatomy, also date to this period.

At the end of the century there were several types of compound microscope that were covered in parchment or leather decorated with gold. But due to the previously mentioned defects in construction and difficulties of use, their scientific usefulness remained limited throughout the XVIII century. Even in the time of Hooke, simple microscopes were preferred in scientific observations. Antoni van Leeuwenhoeck (1632-1723), the greatest microscopist of the century, a modest employee in Delft (Holland), built hundreds of simple microscopes (of glass, quartz, one of diamonds) not for commercial purposes. They were made up of a simple bi-convex lens 5 millimetres in diameter with a focal distance of 6-7 millimetres: some achieved magnifications up to 270 diameters. With this instrument, opportunely mounted on slides with a related support, Leewenhoeck made innumerable observations and discoveries in the field of biology (red blood cells, infusoria, spermatozoa. etc.).

5.33 Fermat's principle

In describing the history of this principle, the importance of which wave mechanics renewed, we will give some details that seem to demonstrate clearly the slowness in assimilating certain ideas that today seem obvious, and, at the same time, illustrate the tortuous and cloudy ways in which sometimes physics has arrived to establish its principles.

Even before the publication of Descartes' *Dioptrique*, Mersenne had sent Pierre de Fermat (1601-1665) the first "discourses", asking for his opinion. And already in September 1637, Fermat replied basically formulating two criticisms of the Cartesian process: in the first he accused Descartes of having arbitrarily extended the motion of projectiles to the propagation of light as the speed of projectiles is finite and variable, while light propagates instantaneously (some years later the same criticism was advanced by Cavalieri and perhaps shared by Torricelli); with the second criticism, Fermat refuted the principle of the decomposition of motions, that he demonstrated not to have understood and against which he always nurtured great diffidence: twenty years later, in 1657, he would write that care needed to be taken on the use of compound movements that are like medicines that become poisonous if not used properly. But the diffidence towards compound movements was quite widespread, as we have described in § 5.10, among scientists of the century and would continue until the first half of the XVIII century.

In reply to Descartes, again through Mersenne, Fermat insisted in his criticisms and, above all, to misunderstand the utility and legitimacy of the principle of decomposition and to misread the Cartesian concept of *détermination,* for him equivalent only to direction, while Descartes used in the sense of vector velocity. In December 1637, the argument between the two scientists was practically over: two letters from Fermat, one from Descartes, and both stood by their opinion.

But Fermat continued his reflections on the subject which he set out in a "discourse", unfortunately lost, written for his friend Marin Cureau De La Chambre: reflections that must have confirmed his belief in the falsity of the Cartesian law of refraction or at least the inconsistency of the demonstration. We have proof in a vivacious letter of 1658 to Claude Clerselier, a Cartesian, in which he repeats his old observations and adds another: no-one authorises us to think that the tangential component of the speed of light in the second medium has remained unchanged, precisely because the second medium has changed.

But already in returning to the polemic with the Cartesians, the course of Fermat's thinking had changed, thanks to a reading in 1657 of an optics book by De La Chambre in which the law of reflection was deduced using Hero's method (§ 1.13), that is using the metaphysical principle that nature always acts through the shortest routes: a generic principle so uncertain as to allow it to be stretched in all ways to be adapted to any event. Fermat immediately began by modelling it to the necessity of placating his friend's scientific conscience, disturbed by the fact that, in some well-known cases of reflection on concave mirrors, nature acts along the longer routes. Fermat claims that in these cases, the shorter routes must be taken as the simplest ones, therefore, as a straight line is simpler than a curve, the ray of light falling on the concave mirror should be referred to the plane tangent to the mirror at the point of incidence, with the result that, referring to this plane, the path of the ray of light is still the shortest: it would be an exaggeration to say that this is a clear explanation!

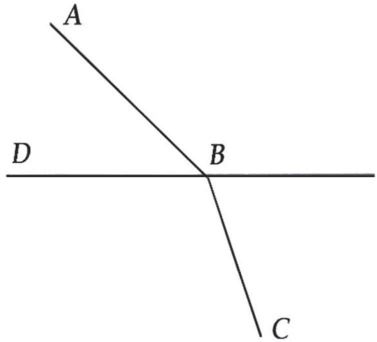

Fig. 5.16

If the principle explains all the cases of reflection so well, why not try to apply it also to refraction? Certainly, if the ray of light, to move from point *A* (Fig. 5.16) to point *C* of another medium, is refracted in *B*, the path *ABC* is longer than *AC*. But the principle of the economy of nature must be interpreted in the sense that the shortest routes are the easiest ones, the routes of least resistance. And if we suppose that the second medium offers different resistance than the first to the propagation of light, it may well be that the path *ABC* overall offers less resistance than path *AC*. The idea - the first nucleus of which will be the definitive formulation of the principle - is undoubtedly ingenious, but immediately appears in contradiction to the scientist's convictions: in fact, the concept of resistance at once gives rise to the idea of the propagation in time of light that Fermat, on the contrary, had believed up until then to be instantaneous. The mathematician did not miss the implication; in order to overcome it, he preserved instantaneous propagation and justified resistance through the antipathy between light and matter, postulated also by De la Chambre. We believe that not even Fermat was satisfied by this shortcut. We may be nearer the truth by supposing that in the mind of a mathematician of his calibre there arose a question of pure geometry, that he tried to adapt in some way to physical reality. The problem, described in the same letter to De La Chambre, is the following: placing point *A* (Fig. 5.16) in one of the semi-planes determined by the line *BD* (and by *A*) and point *C* on the opposite semi-plane, and calling *m* a coefficient different to 1, determine on *BD* a point *B* so that the sum *AB* + *m BC* is the minimum of the sums formed in the same way.

At the time, the problem was not easily solvable, but Fermat promised to provide an answer whenever his friend wanted: Fermat is "Gascon" (a braggart) said Descartes. Four years were needed before the solution was given and, perhaps, as has been repeatedly observed in the psychological analyses of scientific inventions, it struck Fermat unexpectedly through a new adaptation of the principle of the economy of nature: the shortest paths should be taken not as the easiest or simplest paths, or those of least resistance or least antipathy, but the paths that *breviori tempore percurri possint.*

This metaphysical principle of finalism implied abandoning belief in the instantaneous propagation of light and admitting a finite velocity. Repudiation of such a widespread belief, that Fermat himself had implicitly accused Descartes of having abandoned, deserved some words of justification and comment by the mathematician from Toulouse. They came in a fleeting note to a letter to De La Chambre, the true meaning of which is hard to decipher. "If you - the mathematician writes to his friend - persist in not agreeing on movement successive to light and in sustaining that it occurs in an instant, you may compare the facility or flight and resistance smaller or bigger in different mediums, and that in different proportions depending on the greater difference of the mediums; they may be considered in a certain ratio and consequently enter very well in the calculation like the time of the motion; my demonstration will be equally valid in any case"[213]: facility, flight, resistance in the propagation of light are all concepts that necessarily recall the idea of propagation in time; we therefore believe that in Fermat's thinking they do not correspond to concrete physical ideas but are simply

[213] P. Fermat, *Works,* edited by P. Tannery and Ch. Henry, Gauthier-Villars, Paris 1891-1912, Vol. 2, p. 462.

verbal translations of the *m* ratio in the problem of pure geometry that he himself set.

Having understood the principle, a numerical example allowed Fermat to claim how it could explain refraction, demonstrating that light may travel a path broken into two different mediums in the shorter time than a rectilinear route with the same extremities. But, in Fermat's opinion, passing from the particular to the general presented two difficulties: one technical and one psychological. The first difficulty referred to the search for a minimum expression that was represented by four square roots (the mathematics of the time did not comprehend the general methods to look for minimums); the second was the fear that the route to be followed might lead to some strange law of refraction, different to that of Descartes, while he was informed on several fronts, especially by a precise experimenter like Pierre Petit (1598-1667), that Descartes' law was in perfect agreement with the experiments. Encouraged by his friends, he finally overcame his hesitation and, returning to the subject in the last week of 1661, he had the inspiration of an approach that halved his work and had the merit of "being the most extraordinary, most unexpected, and happiest imaginable".[214]

The problem is solved in the addition to the letter to De La Chambre under the title *Analysis ad refractiones.*[215] The physical question is substantially to determine the law of refraction admitting the following hypotheses: light has a constant speed in a determined medium and decreases with the increase of the density of the medium; it takes the minimum time to travel from one point to another.

And that is the answer. *AP* (Fig. 5.17) is the surface of the separation of the two means, *CD* an incident ray and *DI* the corresponding refracted. The time of the passage *CDI* must be less than the time of any other passage *COI* between the same extremities. Let's describe a circumference with centre *D* and radius $DC = DI = r$, draw the perpendiculars *CF, IH* to *AP;* suppose that $DF = v_1$ is the speed of light in the first medium. Assuming $DO = h$, $CO = a$, $OI = b$, $DH = x$, the times to travel along *CDI* and *COI* are respectively:

$$\frac{CD}{v_1} + \frac{DI}{v_2} \quad , \qquad \frac{CO}{v_1} + \frac{OI}{v_2}$$

[214] *Ibid.,* Vol. I, pp. 170-71.
[215] *Ibid.,* Vol. 2, p. 462.

Now, Fermat's own method to find the maximums and minimums is as follows: given that the increase of function is *F(x±e) - F(x),* corresponding to a maximum and a minimum, is infinitely small compared to *e,* the values of *x* corresponding to the maximum or minimum of *F(x)* are the roots of the equation *F(x±e) - F(x) = 0.* By then calculating *CO* from the triangle *COD* and *OI* from triangle *OID,* Fermat's method means we may write, neglecting the constant factor $\frac{1}{v1v2}$,

$$v_2\sqrt{r^2 + h^2 - 2hv_1} + v_1\sqrt{r^2 + h^2 + 2hx} = r(v_2 + v_2)$$

Freeing this equation from the square roots and dividing it by *h* and then suppressing the terms containing *h,* one gets

$$x = v_2$$

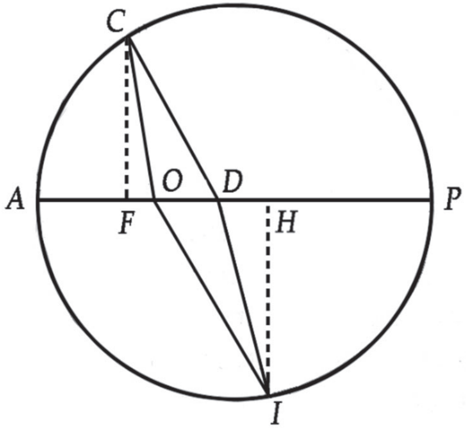

Fig. 5.17

that is, since *DF* is the velocity v_1 in the first medium, *DH* measures the speed of light in the second medium. In other words, for any angle of incidence, we have

$$\frac{sin(i)}{sin(r)} = \frac{v_1}{v_2} = constant$$

And this result amazed Fermat: his principle led to Cartesian law, with the only variation of the inversion of the ratio of velocities. This happy convergence of results, obtained through opposing hypotheses - velocity that increases with density for Descartes and that decreases with the increase of density for Fermat - further confirmed the mathematician from Toulouse's conviction that the Cartesian demonstration was "terribly wrong and full of paralogisms". But he was forgetting that his own demonstration was not based solely on this hypothesis but also on the principle of minimum time, so much so that he managed, using an elementary but laborious procedure, to invert the theorem, demonstrating in a second fragment entitled *Synthesis ad refractiones* that, when refraction follows Cartesian law, light follows a path in the minimum time to arrive from point *A* in a medium to point *B* in another medium.

The Cartesians immediately rose up against Fermat's principle. A letter by Clerselier, often animated to the point of being offensive, contains the main criticisms: the principle that nature acts along the shortest or simplest routes is not a principle of physics as it would mean that nature acts sentiently. In fact, once the ray of light reaches the line of separation of the two mediums, it must recognise that to bend in a certain way takes less time, and therefore time would cause motion. The variations on this theme are almost innumerable and Clerselier makes a lot of them to conclude that Fermat has the merit of demonstrating that refraction occurs "as if" light follows the shortest paths.

At the outset, physicists were also diffident to Fermat's principle. Petit, a renowned physicist of the time, wrote to Huygens: "I am not at all happy with this analysis, and neither with the construction of a triangle of times and of segments compared each other".[216] The most authoritative scientist of the time with regard to optics, initially made clear his unfavourable opinion of Fermat's demonstration. In a letter dated 8 March 1662 to his brother Lodovico, to whom he had sent a copy of Fermat's letter to De La Chambre of 1 January 1662, Huygens declares he was completely unconvinced by Fermat's doctrine, "who assumes many things related to the nature of light and diaphanous bodies, about which he has no certain proof; and after all this, he still uses the miserable axiom that nature always operates through the shortest routes, with which I have never seen any truth proved".[217] And so he resolved, not without a certain humour, the long argument between Descartes and Fermat: "To bring an agreement between him and Mr Descartes, I would say that neither one nor the other has proved

[216] Huygens, *Complete works.,* op. cit., Vol. 4, p. 75.
[217] *Ibid.,* p. 71.

the fundamental theory of refraction, and only experiment can make it certain".

However, this initial negative judgement was progressively relaxed. After three months, on 22 June 1662, having read the *Analysis ad refractiones,* Huygens judged it "very good and subtle", but the principles that he assumes for refraction, that do not include geometry, but physics, are not certain, *sed plane precaria*".[218] Later, he did Fermat's calculation again and found it exact. It was precisely the confidence acquired with Fermat's principle that convinced him that, as expressed in his theory, the index of refraction is equal to the quotient of the speed of light in the first and in second medium.

Last, in the preparatory works for his *Traité de la lumière* (1691), dating to 1676 or 1677, Huygens concentrates on the "minimal property", the *phaenomenon Fermatii,* as he terms it, and demonstrates it for refraction starting from the hypothesis of the law of sines and the refraction index equals v_1/v_2.[219] In sum, with the first moment of diffidence behind him, Huygens's attitude towards Fermat's theorem may be summarised as: without analysing its metaphysical significance, if it has one, Fermat's principle gives a good description of optical phenomena so that it may be confidently employed as a useful tool for scientific research.

5.34 Diffraction and the nature of colour

The Cartesian law on refraction was only just confirmed theoretically by Fermat when another phenomenon of the inflection of light was discovered. The announcement came in a book by Father Francesco Maria Grimaldi, born Bologna 2 April 1618, died in the same city 28 December 1663 (Fig. 5.18). He entered the Jesuit order as a very young man, and first taught philosophy and then mathematics in the College of the Order in Bologna. His name also arises in the history of astronomy for having initiated, in 1640, the naming to lunar configurations after terrestrial regions and illustrious astronomers. His map of the Moon and naming, reworked by Riccioli, was entered into his *Almagestum novum.* He was a singularly expert man, with an infinite patience for experimentation, who wanted to trust only things and not the authority of the masters, as he himself assured in the preface to his fundamental 535-pages, double-column work *Physico-mathesis de lumine, coloribus et iride,* published posthumously in 1665 by fellow Jesuit Riccioli who added a short and moving eulogy.

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[218] *Ibid.,* p. 159.
[219] *Ibid.,* Vol., p. 834; Vol. 19, p. 417.

Fig. 5. 18. Francesco Maria Grimaldi (Anonymous portrait)

The volume opens with the announcement of the discovery of the new type of inflection of light, that Grimaldi calls “diffraction”, inventing a term that would remain in science.

The discovery was certainly accidental and due to Grimaldi experimenting with very thin bands of light, obtained by opening a narrow hole in the window exposed to the Sun. The scientist placed an obstacle in the path of the ray and observed the shadow on a white screen; he noted that on the screen the shadow was dilated compared to the geometric shadow that should have resulted, and also that it was spoiled by three luminous strips, coloured blue towards the inside and red towards the outside. If then the ray of light is made to fall on an opaque screen, parallel to the first, with a second hole, and the emerging ray is projected on another screen, a central luminous spot can be seen to be forming, very much larger than that foreseen by geometrical optics, with the borders coloured red and blue. There is no margin for doubt: light bends behind the obstacles.

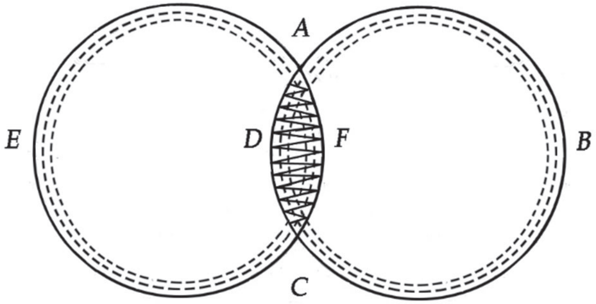

Fig. 5.19

There was another notable experiment that today would easily be misunderstood, as some historians have misunderstood it. Two small holes are made in the window of a *camera oscura* at a sufficient distance between them that the cones of light, of which they are the vertices, are partially superimposed on a white screen placed at a great distance from the window (Fig. 5.19). Either one or other of the two holes is closed and a luminous circle appears on the screen, corresponding to the open hole surrounded by a dark circle. Then both holes are opened: "When the common segment *ADCF* is small [...] both arcs *ADC* and *AFC* will redden. But when the screen is further away from the holes, and therefore with a greater common part of the segment, both circles *ADC* and *AFC* will be significantly darker"; which immediately gives rise to the proposition: "Sometimes light, by its communication, makes darker the surface of a body that was previously illuminated on another part".[220] The statement was later translated into an apparently paradoxical phrase, attributed to Grimaldi himself: light added to light may result in darkness. It is not easy to understand what Grimaldi observed in the experiment of "the two holes", repeated and applied better to the purpose by Thomas Young in 1801. Certainly, he could not have observed a phenomenon of interference that arises when the source is punctiform or linear by some seconds of amplitude, while the Sun has an amplitude of 32‘, without adding that Grimaldi claims to have observed the darkening also with two cones of light obtained anyway and then partially superimposed by a mirror. But even for Grimaldi, the phenomenon must

[220] F.M Grimaldi, *Physico-mathesis de lumine, coloribus et iride aliisque adnexis libri duo,* Bononiae 1665, p. 187.

have been problematic as he felt the need to conclude the treatise by saying “The experiment is in the hands of the skill of the experimenter and the keenness of the observer’s eye”.[221]

The iridescence at the fringes of diffraction naturally pushed the scientist towards a study of the nature of colours, the arguments about which had long been lively between philosophers and scientists, so much so that we may suppose that Grimaldi’s experimental study of colours led him to the accidental discovery of diffraction.

Following the Cartesian theory (§ 5.30), in 1648 Johannes Marcus Marci had experimented with glass prisms placed in front of a hole in the *camera oscura* and had noted that the coloured rays had different refrangibility and that a new refraction imposed on them at the exit from the prism does not alter the colouration. Therefore, he concluded, as would Newton later with an identical experiment, that colours are inherent to light.[222] But Marci’s work must have been little known if the well-informed Grimaldi did not know it even if he experimented, like Marci, with a prism. Grimaldi sent sunlight on a prism through a small hole in the *camera oscura* and observed that in the emerging light red was at one end of the band and violet at the other end, separated by a yellow, or even green, zone, depending on the obliquity of the screen to the rays, its distance from the prism and the prism’s inclination towards the incident rays. It is a shame that Grimaldi did not persevere along these lines and, above all, did not observe the spectrum at a greater distance from the prism, because he couldn’t think of diaphragming the ray of light entering the *camera oscura*.

However, the experiments with the prism allowed him to conclude that light takes its colours by refraction, but that it could also be coloured, without reflection or refraction, by diffraction. If, therefore, there are many ways in which light is coloured, but none of them is necessary, Grimaldi concludes that colour must be inherent in light, an intrinsic modification: “Solely by a certain intrinsic modification does light sometimes mutate to an apparent colour, as we say, without having to assume another entity at the same time”.[223]

But what is this “certain intrinsic modification”?

Grimaldi’s book is an interesting and curious work. Interesting for its content that deals with a variety of physical questions (which will see again in § 5.37); curious because the first book supports the material nature of light and the second confutes the arguments put forward in the first and

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[221] *Ibid.,* p. 190.

[222] J.M. Marci, *Thaumantias. Liber de Arcu coelesti colorum apparentium natura ortu et causis,* Praguae 1648, p. 95.

[223] Grimaldi, *Physico-mathesis de lumine,* op. cit., p. 239.

proposes its accidentality or, to put it more expressively, as he himself writes, its wave character. There is another even more curious fact: the two theses are declared in the long title of the work. In the end, Grimaldi concludes, as modern physicists conclude, that there is no reason to prefer one theory to another and that both may be true. The wave hypothesis allows him to explain which modification produces colours: “Modification of light - he writes - through the force of which it is coloured both permanently and (as is claimed) apparently, or is made visible through the denomination of colour, may not improbably be claimed to be due to its minute bent undulation, such as a tremor of diffusion, with a certain extremely minute undulation through which it stimulates the organ of sight through its own and determined activity”.[224]

Grimaldi provides a number of arguments in support of his theory of undulation, in particular the analogy with sound, the intensity of which varies, as Galileo had taught, with the various aerial undulations. However, in one of the last propositions, Grimaldi adds that his preferred theory is that “colours are not distinct from light”.[225]

But oppositions to the theory must have been loud and strong, at least in Grimaldi’s circle, as he remembers that in various discussions with learned men, he was never able to change their conviction that colours were permanently fixed in the visible object, in conformity to the immediate testimony of the senses. The obstinacy of the learned doctors drove the weak and sick Grimaldi wild: “Again! Every time I hear this illustrious philosophy, I am stricken by the bile!”[226]

Experiments similar to Grimaldi’s on diffraction were carried in 1672 by Hooke, who claimed they were unrelated. However, Hooke’s great defect in claiming for his own the discoveries of others is famous. In any case, Hooke’s experiments add nothing to Grimaldi’s.

On the other hand, the experiments described by Hooke in his *Micrographia,* published in 1665, the same year in which Grimaldi’s *De lumine* appeared, are much more important. *Micrographia* is an interesting book, in particular for the history of the microscope used by Hooke with an uncommon ability and described in the long foreword: a eulogy to experimental science and the instruments that sharpen the senses. In addition to the microscope, the preface describes other notable instruments, such as the refractometer with which Hooke proved the law of sines and discovered that the greater density of a body does not always correspond to a greater

[224] *Ibid.,* p. 342.
[225] *Ibid.,* p. 523.
[226] *Ibid.,* p. 411.

refrangibility: for example, wine spirit is more refrangible than water despite being lighter.[227]

The observations using a microscope of most interest to us are those related to thin foils. The observations follow in from those made by Boyle (1663) who noted the coloured fringes in thin glass film, soap bubbles and shallow layers of other transparent liquids.[228] Hooke began the observations using thin strips of chalk, perhaps of the lenticular variety, on which, with the aid of a microscope, he noted little white spots surrounded by systems of round or circular rings. The colours succeeded each other, from the centre of the spots towards the outside, in the order blue, purple, scarlet, yellow, and green, and repeating themselves in the same order in successive systems a great number of times. Similar colourations are obtained by superimposing two crystal strips, each the size of a shilling; in this case, however, the colour in a certain point changes with the pressure exercised by one strip against the other, passing successively, for example, from red to yellow, then blue, green, and purple. After careful examination of the phenomenon, Hooke was convinced that this depended on the strip being immersed in a medium of different refrangibility. And, in fact, he observed the phenomenon not only in the thin layer of air between the crystal strips but also in thin layers of blown glass, soap bubbles, shallow layers of other liquids, the surfaces of red-hot steel, etc. The phenomena occurred in such a varied and astounding way that the experimenter was driven to examine the causes. He began with the study of the conditions in which the thin strips, whose thickness he never managed to measure, present the phenomenon of colouration, drawing conclusions that are mostly still considered valid today. Accurate examination allowed him to confute Descartes' theory of colours, according to which the rotation of the luminous corpuscles, the cause of the sensation of colour, began at the moment of refraction and ceased due to successive refraction in the opposite direction. In the thin strips, the two refractions did indeed occur but the colour persisted.

Hooke proposed a theory of vibration to counter the Cartesian theory. In his opinion, light is provoked by a movement of the medium; the movement, or jolting, is transmitted through pulsations perpendicular to the direction of

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[227] R. Hooke, *Micrographia or Some Physiological Descriptions of Minute Bodies by Magnifying Glasses, With Observations and Inquiries thereupon, London* 1667, The Preface, n.n. This edition is the same as that of 1665 apart from the change to the frontispiece.

[228] R. Boyle, *Experimenta et considerationes de coloribus,* in Op. Cit., *Opera omnia,* Venetiis 1696, Vol. I, p. 965. In this same case, Boyle recalls seeing a "variety of small irises" by looking at the Sun near the horizon through a feather of the appropriate size and shape.

propagation. The velocity of propagation, says Hooke as had Grimaldi, is huge but not infinite, equal in all the directions of a homogeneous medium, as occurs in waves on the water's surface caused by a dropped stone. When a ripple meets a medium different to the one in which it was propagated, it changes velocity. It follows, according to the scientist's confused reasoning, that the pulsation from the perpendicular to the direction of the propagation becomes oblique and it is that obliquity that causes the sensation of colour, more exactly: "Blue is an impression on the retina of an oblique and confused pulsation of light, whose weakest part comes first and is followed by the stronger. Red is an impression on the retina of an oblique and confused pulsation of light whose stronger part precedes and is followed by the weaker".[229] The other colours arise from the combination of these two fundamentals. In the thin strips, Hooke concluded, the retina is struck by the ray reflected by the first face and the ray reflected by the second, weaker than the former not only because of the two refractions but also because of the time needed for the light to pass through the layer twice: the various modes of combining the two pulsations, that the retina cannot distinguish due to the speed with which they follow each other, give the sensation of the different colours.

In conclusion, the few pages Hooke dedicates to the colouring of thin strips, while being admirable for the richness of experimental observations, leave gaps with regard to theory, despite some genial intuitions that were ahead of their time. They end an historical period of almost thirty years, from Descartes to Hooke, during which the more progressive scientific thinking demolished the thousand-year-old belief in colours being fixed in bodies or the result of the various mixing of light and shade, replacing it with the concept that colours are modifications (substantial or kinetic) of "pure light", that is white light. We have to wait for Newton to overturn the proposition almost completely: each homogeneous light possesses its own colour, that it is the same as saying colour is not a modification of light, but is itself light.

5.35 Double refraction and speed of light

Before coming to the next chapter and the treatment of the two major opposing theories of light that have marked, with alternating fortune, the centuries, we must mention two discoveries that preceded, and influenced, them.

[229] Hooke, *Micrographia,* op. cit., p. 64.

In 1669, the Dane Erasmus Bartholin (1625-1698) published a work entitled *Experimenta crystali islandici dis-diaclastici, quibus mira et insolita refractio detegitur* in which he describes crystals of Iceland spar and the "admirable and unusual" experiments of double refraction observed in them. News of the discovery spread fast, especially thanks to the lengthy summary published in "Philosophical Transactions".[230] Bartholin first describes the deposits and the ways of mining the crystal, its crystallographic form, its physical and chemical properties, followed by the phenomenon observed: objects, viewed through one of these prisms, appear double in certain positions; the distance between the two images, weakly coloured, increases with the thickness of the prism. But in certain positions of the crystal, the image is only one, like other transparent bodies: more precisely, a single image is obtained if the visual that passes from the eye to the object through the prism lies in the plane bisecting one of the two obtuse dihedrons of the prism or is parallel to them. If the prism is rotated, one of the two images stays still and the other moves around it. Bartholin correctly interpreted the phenomenon as due to a double refraction of a ray of light passing through the Iceland spar. Because of this double refraction, he named the crystal *dis-diaclastico* (doubly refractive), a neologism that died at birth. Of the two rays refracted from the single incident, one obeys the Cartesian low of refraction, with an index of refraction measured by Bartholin as 5/3, while the other, that he called *mobile* and now referred to as *extraordinary,* does not. Bartholin attributes the phenomenon to the disposition of the pores in the crystal. We will see Huygens' interpretation that returns, with greater accuracy, to the study of the crystal and of the double refraction phenomenon.

Notwithstanding doubts raised by Galileo (§ 4.12), Kepler, Descartes and numerous other scientists continued to believe the speed of light to be infinite; the attempts of the academicians of the Cimento to determine the speed of light using Galileo's approach, naturally followed by a failure revealed in the *Saggi,* strengthened the belief in instantaneous propagation, that was not totally a prejudice: the reasoning adopted by Descartes (§ 5.30) carried great weight and it required non-conformist spirits of the stature of a Grimaldi or a Hooke, to postulate finite speed.

In 1672, the astronomer Gian Domenico Cassini (1625-1712), one of the many Italian scientists summoned to Paris by Louis XIV, began a systematic study of the Jovian system. He was assisted by Olaf Römer, a young Danish scientist (born Aarnus, 25 September 1644, died Copenhagen 19 September 1710) who had studied under Bartholin and was persuaded to move to Paris by Picard who recognised his particular skills.

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[230] "Philosophical Transactions", 1670-71, pp. 2039-48.

During the observations of Jupiter's satellites, Cassini noted that in the interval between an opposition and the successive conjunction with Jupiter, the appearances of the first satellite in Jupiter's cone of shadow were progressively delayed; *vice versa*, in the interval between a conjunction and the successive opposition, the immersions of the satellite in the cone of shadow of the planet were progressively earlier. Cassini deemed that the difference between the period of the satellite observed when the Earth was almost in opposition, that is closest to the planet, and the period observed when the Earth is almost in conjunction with Jupiter, that is further away from the planet, was equal in absolute terms to around 14 minutes. Initially, Cassini hypothesized that the inequality might be the result of the fact that light requires time to travel from the satellite to us. But shortly after he changed his opinion due to both the reaction of the Cartesian circle and because the phenomenon was not observed, or rather seemed not to be observed, in the other three satellites.

The interpretation, abandoned by Cassini, was taken up by his young assistant who, on 22 September 1675, as a plaque at the Astronomical Observatory of Paris commemorates, wrote the first note on the measurement of the speed of light deduced from the observation of the periods of occultation of Jupiter's first satellite in the shadow of the planet. In September 1676, Römer predicted the delay that would be observed in the eclipse of the Jupiter's first satellite in the following November; the prediction was proven, and in the same month he set forth his theory[231] to the Paris *Académie des sciences*, affirming that light takes 22 minutes to travel through the Earth's orbit (today, the more exact value is taken to be 16 minutes and 36 seconds).

But, at the time, the Academy and the University of Paris were dominated by Cartesian thinking, so that Römer's theory met with strong resistance: Cassini publicly dissociated himself from Römer's ideas. But there were numerous supporters in France, and especially outside France: one of the most enthusiastic, and more significant, was the astronomer Halley. The opponents were not driven solely by sectarian motives. The objections of Descartes and Cassini carried scientific weight and were added to in 1705 with the criticism of Giacomo Filippo Maraldi (1665-1729), Cassini's nephew: if the irregularities in the periods observed in Jupiter's first satellite depend on the variation in the distance between Earth and Jupiter, they should also depend on the position of Jupiter in its own orbit because that position sets its distance from the Earth. The objections were

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[231] D*émonstration touchant le mouvement de la lumière trouvé par M. Roemer,* in "Mémoires e l'Académie royale des sciences", 10, 1730, pp. 575-77. The complete memoir was published in the "Journal des Sçavans", 1676, pp. 267-70.

removed one at a time: the recognition of the errors of the astronomers of the time in calculating the periods of the revolution of the other three Jovian satellites reduced Cassini's argument; Maraldi's criticism was absorbed with the acknowledgement of its fundamental truth and the position of Jupiter was considered in the calculation of the speed of light.

Römer's theory was definitively confirmed and Descartes' objection removed by the astronomer James Bradley (1693-1762) who, in 1725, while attempting to measure the parallax of certain stars, realised that in their culminating position they always deviated southwards. The observations, that continued up to 1728, showed that in one year these stars appeared to describe an ellipse. Bradley interpreted the phenomenon, termed "aberration" by Eustachio Manfredi (1676-1730) in 1729, as due to the composition of the speed of light coming from the star with the speed of the Earth in its orbit.

Although terrestrial measures of the speed of light had to wait another century, after Bradley the gradual propagation of light was commonly agreed on as a given fact.

MAGNETISM AND ELECTRICITY

5.36 In the footsteps of Gilbert

Gilbert's work (§§ 3.20-21) was new in its method and revolutionary in the ideas. It found its first admirers and detractors in Italy: admirers like Galileo who keep their heads; critics like Nicola Cabeo (1585-1650), a Jesuit from Ferrara, who came down on the experimental aspect, which was exactly Gilbert's territory.

In 1629, Cabeo published in Ferrara a *Philosophia magnetica* that still deserves to be studied not only because of some new revelations that we will deal with later, and for the illustration of the experimental phenomena already discovered by Gilbert, but also for an understanding the arguments, not always scientific, that counselled him to oppose the stupendous fundamental Gilbertian idea: the Earth was a giant magnet. On the contrary, the purpose of the book was to negate the qualitative identity between Earth and his magnetic model, "*terrella*". And if Gilbert, to support his theory, taught that iron placed on the terrestrial magnetic meridian is magnetised, Cabeo tried to disprove the argument, claiming a new phenomenon that he had discovered: even iron held vertically and door hinges are magnetised, with the south pole above and the north pole below.

And while Gilbert did not accept the ancient theory handed down from Lucretius on the attraction of amber, because it seemed to him that if the discharges of amber repel air, they must also repel the other light bodies present, Cabeo replied that the repulsion exists and can be ascertained in an experiment that is the first proof of electrical repulsion. Cabeo rubbed a piece of amber and held it close to some sawdust. The sawdust was drawn to the amber and stood up like hairs on the surface, moved weakly as if blown by a slight breeze, hesitated and finally made a very agile leap. [232] Comparing the phenomenon to the rebound of an elastic body, he attributed the cause to the force of air, thereby returning to Lucretius' theory.

Cabeo admired the experimental part of Gilbert's work but tried to criticise his theories, perhaps more for a sectarian spirit than scientific conviction; and we may say that he attempted to do so because, instead of replacing them with more probable theories, he just reworked old beliefs that had been surpassed by the new scientific mentality.

Castelli seems to have possessed this new thinking as, in a "discourse", or memoir as we would say, on the lodestone, he aimed to provide a theory of the constitution of magnets, magnetisation and magnetic attraction. Unfortunately, the "discourse" remained unpublished until 1883 when it was included in volume XVI of Boncompagni's "*Bulletin of the bibliography and history of mathematical and physical sciences*". Castelli starts from some well-known and lesser-known experimental results; the latter include that which we call the magnetic spectrum, realised more or less as we use today, placing a sheet of paper over a magnet and scattering on it not iron, but magnetic filings.

Castelli supposes that there exist "magnetic bodies of the first order" in which there are spread particles of lodestone, that is tiny magnetic needles, that may be ordered due to the external magnetic action and, once ordered, all, or nearly all, remain in the new position. He also supposes that there exist "magnetic bodies of the second order" in which there are disordinate magnetic particles that have the propensity to return to their original positions. The reader may compare these theories with those found in a treatise on the constitution of magnets, drawn up in 1891 by James Alfred Ewing (1855-1935) and see that, conceptually, the difference is only in the words and understand that, as with Castelli in his simple theories, he explains the make-up of magnets, temporary and permanent magnetisation and magnetic attraction.

Grimaldi, too, in *De lumine,* dedicates more than thirty pages to magnetism, describing past and present experiments (including that of a magnetised iron wire that losses its magnetism when repeatedly bent and straightened) and therefore attempting an explanation with the hypothesis,

[232] N. Cabeo, *Philosophia magnetica, in qua magnetis natura penitus explicatur,* Ferrariae 1629, p. 194.

inspired by Descartes, of a single substantial magnetic fluid that passes from one pole to the other of the magnet: the opposing directions of the flow would result in apparently opposing effects. Each magnetic object not magnetised, like the iron, contains disordinate fluid: the magnet orders it and therefore induces magnetism in the object. Although Grimaldi's theory derives from Descartes, it does possess the singularity: it introduces the concept of a single fluid and does not theorise on the form of its constituent particles.

5.37 Guericke's experiments on electricity

With the invention of the "versorio", Gilbert had introduced a very sensitive instrument to reveal electrical phenomena. By introducing the rotating sulphur globe, Otto von Guericke allowed to amplify them to the extent of revealing new phenomena. It is also interesting to note that the electrical machine was suggested to Guericke by Gilbert's *terrella*: just as this imitates the magnetic properties of the Earth, according to Guericke there must also be another "terrella" that imitates its electrical properties.

The electric "terrella" was prepared by Guericke using a sphere of melted sulphur, the size of a child's head, diametrically penetrated by an iron bar around which it was rotated with the dry palm of the hand (Fig. 5.20). This is therefore the first electrostatic machine using continuous rubbing. The electrified sulphur sphere attracted threads of gold, silver, paper, etc. that were then repulsed, just as Cabeo had observed. Guericke argued that the repulsion could not be caused by a simple mechanical game but must reside in an intrinsic property of the sulphur globe because the corpuscles attracted and then repulsed are not again attracted by the globe if they do not touch a third body. The experiment becomes particularly interesting, and amusing, with a light feather that, repulsed by the electrified globe with which it came into contact, remains hanging in the "sphere of action" of the globe and may be taken around the room by moving the underlying globe. And during the movement around the room it may be seen that the feather attracts everything close to it or, if it has no force enough, sticks to it, especially if the other object has projections. If you bring your finger closer, the phenomenon is really amusing: the feather moves towards the finger and then returns to the globe, and from the globe mounts the finger, and keeps repeating the action. The sphere then transmits its power of attraction also in a wire longer than an ulna (0.67m) so disproving the theory of attraction through the intermediary action of air. Last, if the sulphur sphere is excited in the dark, there is a characteristic cracking and at the same time it shines, like sugar when it is crushed.

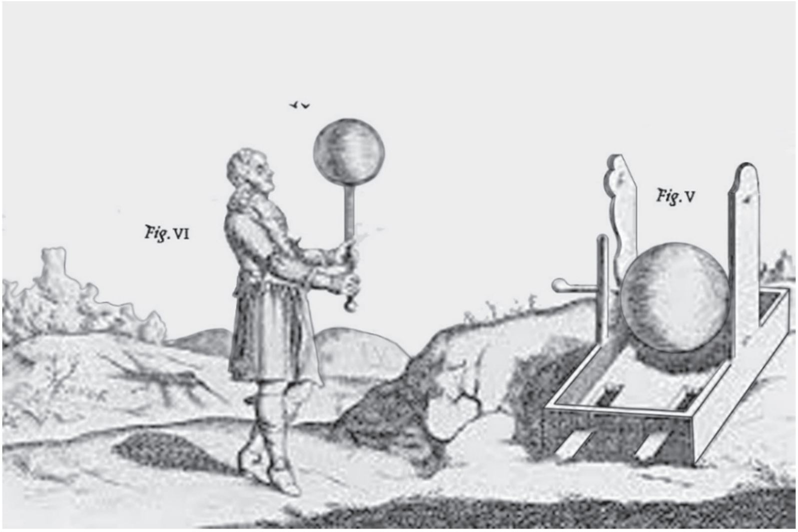

Fig. 5.20 - The first electrostatic machine: on the right the rotating sulphur globe; on the left an electrically charged feather made to move by the sulphur globe that is also charged.

Guericke's experiments, that should be mentioned not just because of their importance but also because of the scarce interest they aroused among his contemporaries, perhaps due to their metaphysical framework, were repeated by Boyle with the same results. Boyle added an experiment that the Cimento academicians had carried out on the lodestone and had projected for electrically charged objects without success. He proved, using a pneumatic machine, that magnetic and electrical attractions occur also in a vacuum. The old theory of the intervention of air was completely disproved. But what could physicists propose as its most probable replacement? Nothing but a return to Gilbert's theory of a fluid that issues from the electrical object and links the light corpuscles. This was Boyle's theory; this was Newton's theory to explain the electrical dance that he arrived at by rubbing a cloth over a glass disc, supported by a bronze ring and placed about 8 millimetres from the table beneath on which were spread pieces of paper.

6. Isaac Newton

6.1 The man

On 25 December 1642 (old style, corresponding to 5 January 1643 in the Gregorian calendar), Isaac Newton (Fig. 6.1) was born into a family of small landholders in Woolsthorpe, in the county of Lincolnshire. He completed his early schooling without showing any particular intellectual gifts, but his passion for drawing and all types of manual exercise led him to construct a variety of excellent instruments (kites, windmills, clocks, etc.). In 1661, he matriculated at Cambridge University where he supported himself with part-time work and little domestic jobs for his teachers. He graduated in 1665: the university was not aware of his genius; only his tutor Isaac Barrow (1630-1677) saw his exceptional aptitude and encouraged him in the study of mathematics and optics. But in 1665 the bubonic plague, that had broken out in 1664, spread uncontrollably in London and threatened to spread also to Cambridge. The university was closed and Newton returned to his home town, where he remained until the spring of 1667.

This period of intellectual solitude was the most creative in the scientist's life, as Newton himself remembered in his old age. In the rural peace of Woolsthorpe, Newton had his most profound intuitions on mathematics, mechanics, and optics, that he developed later in his career.

In 1667, with the end of the plague, he returned to Cambridge and renewed relations with Barrow who, in 1669, having read a famous paper by his pupil on the basics of infinitesimal calculation, decided to retire to theological studies and leave to Newton the "Lucasian" chair (named after Henry Lucas who had founded it, leaving a legacy, in 1664) that has always been a position occupied by eminent scientists. In 1672, at the request of the Royal Society, he sent an example of the new reflecting telescope, with which we will deal later, and, again at the invitation of the Society, he illustrated its genesis and advantages in a paper on the nature of light and colours published in "Philosophical Transactions". The paper was criticised by Hooke, Lucas, Linus, and Ignace Pardies and an argument arose (encouraged by Henry Oldenburg, the then secretary of the Royal Society, who disliked Hooke) that embittered Newton, grumpy temperament and intolerant to criticism.

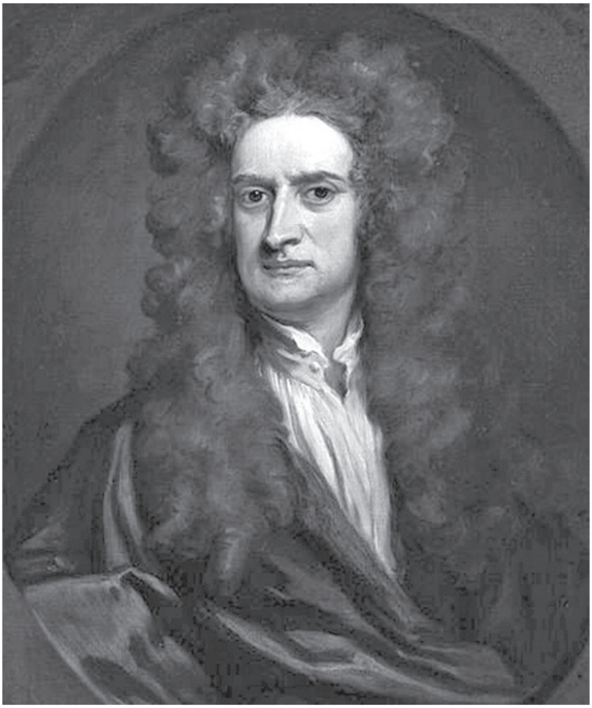

Fig. 6.1. Isaac Newton. Portrait by Godfrey Kneller (1702).
Source: National Museum of science and technology, Milan.

Between 1684 and 1686 he made an enormous intellectual effort: under the insistence of Halley, he systematised his theories on mechanics in the paper *De motu* presented to the Royal Society in 1685, and then in the timeless work *Philosophiae naturalis principia mathematica,* deposited at the Academy on 28 April 1686 and published in 1687, and overcoming the considerable financial difficulties with the help of Halley who stood guarantor for the publishing costs. Newton himself curated two successive editions: in 1713 with the help of Roger Cotes (1692-1716) and in 1726 with the collaboration of Henry Pemberton (1694-1771). Each edition was amended and improved compared with the preceding one.

1687 saw Newton take his first political stance. He had never taken a clear position on the two parties involved in the 1688-89 revolution that created liberal England, but it seems he was closer to the *Whigs*, liberals, rather than the *Tories*, conservatives. In any case, in 1687 Cambridge University included him in the delegation that was to defend the autonomy of the university in its refusal to confer an academic title to a Benedictine monk, as imposed by King James II. While the delegation, fearing the insulting treatment of the Chancellor, was about to surrender to the court's demands, Newton stood up firmly against such a move, saving the prerogatives and prestige of the university.

Curiously, after the publication of the *Principia,* Newton lost interest in scientific studies and started to look for a state employment, coming up against serious obstacles. But in the meantime, the long intellectual effort to write the *Principia,* lack of rest, insufficient and irregular meals, the delusion over the obstacles to his search for employment, brought him to the brink of madness. He recovered almost completely in 1694.

In 1696 Charles Montagu (1661-1715), who became Lord Halifax in 1700, treasury minister and a friend of Newton, nominated him inspector at the Royal Mint in London. Newton accepted and moved to the capital. In 1699 he was appointed director of the Mint; a position that was, and continues to be, at the centre of anachronistic, and also unfounded malignity, naturally recorded by Voltaire: the appointment was not due to his exceptional scientific merits but to the charms, appreciated by Montagu, of the Newton's beautiful niece and governess of his London house, because Newton remained a bachelor all his life, indeed, according to Voltaire's gossip he never knew a woman (in the biblical sense).

The appointment as director of the Mint practically ended Newton's creative research. The scientific studies became marginal in his activities; in the expectation that his works would be printed or reprinted, he turned to monetary questions, ancient chronology, sacred history, chemistry and alchemy, the latter having been a passion since his youth. But above all, he rendered great service to his country as an honest and very able administrator. In 1703, he was elected President of the Royal Society, of which he had been a member since 1672, and held that office until his death in the early hours of 20 March 1727.

Our task here is to deal only with Newton's physics. However, his interests were wide-ranging: passing, as we have just mentioned, from mathematics to theology, from physics to economics, from chronology to alchemy. His work on alchemy is less known. It is contained in voluminous manuscripts that began to be examined in the XX century. However, it is certain that he believed in the possibility of the transmutation of the elements, and in particular the making of gold, but he also believed that the discovery would cause an "immense danger for the world": it would be puerile to speak of prophecy as Newton was thinking of nothing like the atomic bomb.

Newton was a contradictory figure: scientist and mystic; generous and mean; modest and arrogant. John Maynard Keynes (1883-1946) called him the first modern scientist and the last magician: a very colourful epitaph but one that is very appropriate, maybe suggested by the fact that he came into possession of a large part of the English scientist's manuscripts on alchemy.[233]

MECHANICS

6.2 "Regulae philosophandi"

The first scientific interests of the young Newton were concentrated on optics, especially the experimental part for which, as we have noted, he had a particular disposition for invention and technical ability. From optics, Newton gradually moved on to the study of mechanics and with the passing of the years his taste for experimentation declined with a parallel increase in his passion for theoretical questions. But as his greatest work on mechanics appeared in 1687 and that on optics in 1704, it seems proper that we should also follow that chronological order.

Galileo and Huygens had applied the mechanics of bodies to terrestrial surface. Newton's work was different in that it generalised the principle of inertia and the concept of force; for the fundamental effect attributed to mass in mechanical processes, and the extension, already begun by Borelli (§ 5.9), of the laws of mechanics to the entire universe.

This latter extension, which gave back to the world the unity and continuity shattered by Aristotelian mechanics (§ 1-3) is justified by Newton with some *regulae philosophandi* on which all his researches on

[233] The history of Newton's manuscripts is very complicated. Newton left them to his nephew; through hereditary rights, they passed to the Portsmouth family who, in 1872, decided to bequeath the scientific part to Cambridge University, while the family kept all that which referred to theology, chronology, history and alchemy. The Commission charged with the selection of the manuscripts, composed of eminent scientists with no knowledge of the history, established which works should be granted to Cambridge University and described them in 1888 in a paper entitled *A Catalogue of the Portsmouth Collection.* The other manuscripts, returned to the Portsmouth family, was sold at auction in 1936 and Lord Keynes acquired about half; on his death, they were left to King's College, Cambridge. The manuscripts sold at the auction are described in Southeby and Co.'s *Catalogue of the Newton Papers, Sold by Order of the Viscount Lymington to Whom They have Descended from Catherine Conduitt Viscountess Lymington, Great-niece of Sir Isaac Newton,* London 1936.

mechanics are based, even though they are introduced in the third part of the *Principia.*

The first rule is to accept no causes of the phenomena other than those that can explain them. The second rule states that reference should always be made to the same cause of similar effects: for example, the light of a kitchen fire and the light of the Sun must operate in the same manner. The third rule advises applying to all bodies those qualities that we experience, without increase or decrease, on any body on which we may experiment: this is the Newtonian precept of induction that leads, for example, to conclude the impenetrability and extension of all bodies, even if it is possible to carry out experiments only on some. Finally, the fourth and last rule (added only in the third edition of the *Principia*) prescribes the validity of every proposition obtained by inductive experience until other phenomena are discovered that limit or contradict it.

A fifth rule, essentially philosophical, that remains a manuscript, expands the fourth. It poses the empiricism of his philosopher friend John Locke (1632-1704) against Cartesian innatism. It states: "Everything that does not derive from real things or through external sense or through the sensation of thought is to be considered hypothetical. Certainly, I feel that I think, which could not happen if at the same time I did not feel that I am. But I do not feel that any idea is innate. And I believe phenomena to be not only that which we know through the five external senses but also that which through thought we understand in our mind, such as: I am, I believe, I want, I do not want, I am thirsty, I am hungry, I am happy, I suffer, etc. And that which cannot be deduced from events, nor proved by an argument based on induction, I hold to be a hypothesis".[234] And this is the only confession of a philosophical faith that Newton would have wanted to make in the *Principia* and which, for reasons that escape us, he never did.

The third rule allowed Newton to formulate the law of universal gravity: if all bodies gravitate towards the Earth and the sea gravitates towards the Moon and the planets gravitate towards the Sun, we can conclude that all bodies gravitate towards each other. In expounding the law, it was not Newton's intention to assign the cause of gravitation: "I have not been able to deduce from the phenomena the reason for the property of gravity and I do not invent hypothesis [*hypotheses non fingo*]. In fact, all that cannot be deduced from the phenomena must be termed hypotheses; and the hypotheses, both metaphysical and physical, both of the occult and the mechanical qualities find no place in experimental philosophy. In this philosophy, propositions are deduced from the phenomena and are

[234] Published, also in facsimile, by A. Koyré, *Les regulae philosophandi,* in "Archives internationales d'histoire des sciences", 13, 1960, p. 14.

generalized by induction. Consequently, we understand impenetrability, mobility, impetus of bodies and the laws of gravity. And it is enough that gravity effectively exists, acts according to the laws we have expounded and causes all the movements of the heavenly bodies and of our seas".[235]

These are resolute and concise affirmations that, introduced only in the second edition of 1713, appear to be polemical against the Cartesians (§ 5.5). Newton argues against the "physics of hypotheses" of Descartes using the "physics of principles". The aversion of the scientist to Cartesian physics, in particular for its hypotheses, increased with time. Indeed, in the first edition of the *Principia,* the first two *regulae philosophandi* were included in a group of nine propositions entitled precisely *Hypotheses.* But a conviction was gradually growing in Newton that the Cartesian hypotheses were untrue, with no experimental justification, that must be replaced with more certain "principles". But the principles are essentially arbitrary generalisations of experimental facts, and who had ever known how to separate them completely from the hypotheses? No wonder then, that, despite his profession of faith, we will see Newton. too, rely on abstract bodies in his construction. Overall, however, his work is perhaps the accomplished model ever in physics of the harmonic fusion of experimental facts and theoretical considerations. And the famous epitaph penned by Alexander Pope (1688-1744) expresses the admiring stupor of the reader, especially a reader competent in history, before one of the greatest creations of human intelligence: "Nature and nature's laws lay hid on night; and God said 'Let Newton be' and all was light."

Before giving an idea of the general architecture of the work and its characteristics, we would like to observe once again that, while Newton's optical experiments were, as we shall see (§§ 6.14-23), ingenious and extensive, the, very limited, mechanical experiments were aimed at verifying known facts. In mechanics, the genius of Newton shone in the co-ordination of the work of others and the generalisation of laws already known in particular cases.

6.3 Mass

The first pages of the *Principia* contain the fundamental concepts, the axiomatic of classical mechanics. The part is made up of eight definitions,

[235] I. Newton, *Philosophiae naturalis principia mathematica,* Londini 1726, *Scolio generale*, p. 530. We shall always refer hereafter to this edition. All the same, to facilitate comparison with the many other editions and translations, we will quote books and propositions rather than pages. The first translation into Italian was edited by A. Pala (I. Newton, *I principi di filosofia naturale,* Utet, Turin 1965).

three laws of motion, six corollaries and a scholium. The concept of mass, introduced in the first definition, is not Newton's original idea. Contrary to that commonly believed, it came to maturity over several generations. Traces can be found in the *Mechanical problems,* a spurious work attributed to Aristotle, that poses the question: "Why can an unladen balance be moved with less force from its equilibrium than a laden one? And, equally, why does a wheel turning around an axis require greater force to be set in motion the heavier it is?". The author solves the problem by claiming that every heavy body offers resistance not only when it is pushed upwards, but also when it is moved in any direction. Therefore, he knows that something, related to the weight of the bodies, influences their motion in any direction.

The question needed attentive study in successive centuries, by experiment, perhaps in the Museum of Alexandria. This is proven by the fact that in his *Mechanics,* Hero restates it in the following, more refined, terms: "Why does the same weight added to one of the two dishes of a balance in equilibrium cause it to move differently depending on whether it is more or less laden? Why, for example, if in the two dishes there are three mines and an extra half mine is placed in one of them, does the balance incline at a certain speed while, on the contrary, if there are ten mines in the two dishes, the adding of a half-mine causes the balance to incline at a slower speed?". The reply is: "In the first case, the set of the two weights is set in motion by a (relatively) greater force equal to its sixth part, while in the second event, the ten mines are moved by a force equal to the twentieth part of them".[236] This balance, so ably operated, is not conceptually different from the "machine" designed (1784) by George Atwood (1746-1807), and since then much used in schools until the early decades of the 20th century, to demonstrate the influence of mass in dynamic phenomena.

With the discovery of magnetic polarities and the reciprocal actions between magnets with a prolonged experimentation on spherical magnets (§ 2.12), the Middle Ages suggested to the reformers of astronomy to assimilate gravity to a magnetic action. Copernicus already believed weight to be a "natural tendency" of all parts of the world "to join together in a spherical shape"; Gilbert, as we have seen, considered the Earth a huge magnet, rotating around itself due to a magnetic and primary power, and extended the hypothesis to the Sun and the Moon. Kepler believed that the magnetic nature of the Earth was certainly demonstrated and the extension of the Gilbertian theory to all the planets to be legitimate.

Inspired by this concept in the *Epitome astromomiae copernicanae* (Lentiis ad Danubium 1618), redacted like a question-and-answer catechism, Kepler attributes to heavenly bodies a *prensatio* or *vis prensandi (prenso* =

236 *Heronis Alexandrini opera,* op. cit., Vol. I, p. 180.

to attract), explicitly compared to magnetic attraction. It follows that the *prensatio* is a property of the matter but is not the matter itself. Kepler notes: "The real nature of matter, that makes up the majority of the Earth, is inertia, that reacts against the movement and more strongly when the quantity of the matter amassed in a limited space is greater".[237] Further on, in a very important passage that has so far escaped the notice of historians, there is a clear distinction between quantity of matter (termed "weight") and *prensatio.* In answer to the question: "You therefore claim that the planets have weight?", the answer is: "Weight must be considered as natural and material resistance, that is inertia, to abandon its place once occupied, that drags the planet from, one might say, the hands of the Sun in rotation, so as not to follow the attractive force".[238] In the passage of Kepler, the mass of a body is substantially defined as its inertia or the measure of its inertia. This definition, which met with some favour, was criticised, and is still criticised, as being unclear, because we have no clearer concept of inertia than the concept of mass or quantity of matter.

The first plain distinction between weight and mass (a distinction we would call physical as against the preceding distinctions of astronomical origin) is to be found in Giovan Battista Baliani's *De motu gravium solidorum,* published in 1638. Baliani recounts that, having experimentally proved the falsity of the common belief in the proportionality between the velocity of falling and weight, he was led to think "that while weight acts like an agent, matter acts like a patient, and therefore bodies move according to the proportion of their weights to their matter so that when they fall without vertical impediment, they must all move at the same velocity because the heavier ones have more matter or quantity of matter".[239] In the fourth book, published only in the second edition of the work in 1646, Baliani reiterated the concept: "The nature of heavy bodies is such that their weight is connected to the matter and always follows that condition: as heavy as the weight is, that is its power of action, so is the matter and therefore the resistance, for which it follows that the final effects are equal".[240] In summary, with Baliani not only the concept of the distinction between weight and mass is clear, but also the idea of their proportionality. However, it must be added that if Baliani distinguishes between weight and quantity of matter, even he limited his considerations to the particular case

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[237] G. Kepler, *Opera omnia,* edited by C. Frish, Heyder & Zimmer, Frankfurt-Erlangen, 1858-71, vol, 6, pp, 174-75.
[238]*Ibid.*, p. 374.
[239] G.B. Baliani, *De motu naturali gravium solidorum et liquidorum,* Genuae, 1646, p. 7. As noted, the first edition appeared in 1638.
[240] *Ibid.,* p. 98.

of the fall of heavy objects, without arriving at a generalised concept. In fact, his physics, like the physics of his teacher Galileo, is still a physics on the surface of the Earth.

As we have already mentioned (§ 5.13), Huygens too, was drawn to a distinction between weight and mass in the course of his studies of centrifugal forces. The references to particular examples that conclude with the concept of mass could be continued with Borelli, in the study of planetary motion, with Hooke in the essay on the motion of the Earth, or with Mariotte in the study of the collision of elastic bodies (§ 5.21). But what we have so far reported and documented is sufficient to conclude that pre-Newtonian physics has understood, albeit sometimes in a confused way and limited to specific cases, that bodies inherently contained "something" (known alternatively as inertia, or quantity of matter, or gravity, or body) proportional to weight and determining motion.

It is to Newton's great merit that he organised the abundant material left by his predecessors, reducing all the particular cases to one general concept: mass. The *Principia* in fact open with a definition of mass that reads: "The quantity of matter is its measurement obtained from both its density and its volume". Newton adds that he will call quantity of matter also with the words "body" and "mass". Furthermore, through precise experiments on pendulums, he realised that the quantity of matter is proportional to the weight of the body. Newton prepared simple pendulums of equal length with an oscillating mass made up of empty spheres with equal external diameters, equal weight and made of different materials (gold, silver, lead, glass, sand, household salt, wood, water, wheat) and proved the equality of the periods. To tell the truth, these experiments prove the constancy of the acceleration of gravity, not the proportionality of weight to mass.

Newton's definition of mass, accepted by most physicists for more than a century, led, we might say, to a river of ink. The first thing one notes is the vicious circle of the definition: the quantity of matter is defined by its density, while the density cannot be defined other than the quantity of matter per unit of volume. Actually, Newton did not fall into this trap as he later speaks of density without defining it, as if for him density is a primitive concept, more intuitive that the concept of mass. It may be that in Newton's time, the physical acceptance led to that conclusion, but it is certain that even in his time the definition could not have seemed untouchable, if Henry Pemberton, his collaborator on the third edition of the *Principia,* deemed it appropriate to replace it with the Keplerian definition: "We will write hereafter - states Pemberton in a public *Essay* - without hesitation on the quantity of matter in bodies as a measure of the *Power of inactivity* [*sic*] or *Vis inertiae"*.[241]

Newton's definition of the quantity of matter certainly has its faults, but this defectiveness did not prevent from it being at the foundation of the greatest, most organic, most coherent treatise on mechanics ever written. Therefore, it is not true that an undefined entity contains something undetermined and vague, a concept that reverberates on the subsequent ideas defined by the first concept. Frequently, it is not possible to define a body, not because it is unclear, but on the contrary because it is too well known, so much so that we cannot find simpler concepts on which it may depend.

The history of the criticism of the Newtonian concept of mass leads us immediately to the XIX century: it might be better to delay a description.

6.4 Force

The second definition on the *Principia* concerns the quantity of motion, and the third that which we call inertia and Newton terms the *vis insita* or *vis inertiae* of matter (an expression, therefore, that has a different meaning to our inertial force). The fourth deals with the applied force (*vis impressa*), defined as a determining factor of acceleration.

Kepler had already expounded a concept of force as the cause of motion, but he measured force by velocity. With Galileo, force was equal to weight, but, going way beyond Kepler, he measured the force from the applied acceleration. It may be that Newton did not have this clear Galilean intuition, because in the IV definition he states: "The force impressed is the action exerted on the body to change its state of rest or uniform rectilinear motion". And the definition is illustrated in the following way: "This force consists of a single action and does not remain in the body after the action. In fact, the body perseveres in each of its new states only through the force of inertia. The applied force has, in addition, different origins, such as collision, pressure, centripetal force". The successive definitions, from V to VIII refer to centripetal force, that Newton separates into absolute force, accelerating force, driving force.

Newton gives examples of centripetal force: gravity; magnetic force; that force, whatever its nature, that maintains planets in their circular orbits; the force exercised by the hand to keep the stone inside a sling. From these examples, he deduces the possibility not only of artificial satellites around the Earth, if the projectile is launched at a sufficient velocity, but also even bodies launched from Earth that go to the heavens and continue their motion

[241] H. Pemberton, *Essay on the philosophy of Sir Isaac Newton*, (Italian translation, Venice 1733) p. 11. The English original was published in 1728.

infinitely: “If the velocity of a lead ball, fired by a cannon mounted at the top of a mountain, increases, the distance it may reach may be increased as wished [...] or it may describe an orbit around the Earth, or finally arrive to the heavens and continue its rectilinear journey to the infinite [...]. It is the job of mathematicians to calculate the force that precisely holds a body in a given orbit at a certain velocity” (definition V). Only in 1956 was this mechanical forecast experimentally proved: previously, university students were set the problem as a calculation of science fiction.

The VIII definition states that the driving quantity of the centripetal force is measured by the velocity produced in a given time, that is, in our terminology, by acceleration. And therefore, this driving quantity is what we call applied force and in the case of falling bodies we identify with weight. Arguing against the Cartesian idea of vortexes, Newton clarifies the concept of force: “I will hereafter deal with attraction and accelerating and driving forces in the same way. I use the terms attraction, impulse, any propensity towards a centre indifferently, as I consider these forces mathematically and not physically. Therefore, the reader should be aware that I have not used these terms to describe a type or method of action or a physical cause or reason, or that I have tried to attribute truly and physically forces to the centres (that are mathematical points); even if I have said that the centres attract and the forces are applied to the centres” (Definition VIII).

These are verbal precautions. The fact remains that Newton multiplies the concepts and definitions; he defined an absolute centripetal force (Definition VI), an abstract body to which he makes no further mention in the *Principia.* For him, force and mass are two independent concepts, and would remain so until 1845 when Jean-Marie-Constant Duhamel (1797-1872) in his *Cours de mécanique* introduced what would become the traditional approach that defines mass as the ratio between the force applied to the body and the acceleration imparted: Mach’s severe criticism of this approach amounted to less than nothing.

6.5 Time and space

A celebrated scholium follows the eight definitions that prompted profound reflections first by philosophers, from George Berkeley (1685-1753) on, and then physicists, in the 20th century. It is the scholium that postulates absolute time and absolute space, metaphysical bodies on which, following Newton, all physics was based up until the XIX century. The most famous passages of the scholium are: “I. Absolute, true and mathematical time, of itself, and from its own nature flows equably, without regard to anything external, and by another name is called duration. Absolute time is to be contrasted with relative, apparent, and common time, which is some sensible and external (whether equal or unequal) measure of duration by the means of motion, which is commonly used instead of true time, such as an hour, a day, a month, a year; II. Absolute space, in its own nature, without regard to anything external, remains always similar and immovable. Relative space is some movable dimension or measure of the absolute spaces, which our senses determine by its relationship to bodies, and which common people confuse with the immovable space [...]. It is possible that uniform motion does not exist, by which time can be measured precisely. All movement may be delayed or accelerated, but the flow of absolute time may not be altered [...]. Times and spaces are the places of all things and of themselves. All things are placed in time as to order of succession; and in space as to order of situation. And this is what determines their essence, and that the primary places of things should be movable is absurd. These are therefore the absolute places; and translations out of those places, are the only absolute motions [...] The effects which distinguish absolute from relative motion are the forces applied in the bodies to generate motion. True motion cannot be created neither changed, unless by forces applied to the body; while relative motion may be generated and changed without mutating the applied forces".

For Newton, therefore, force is a primitive entity, irreducible to space and time. Uniform rectilinear motions are relative, while he is convinced that absolute motion exists, as he tried to demonstrate with the example of a bucket of water rotated around its vertical axis. During the motion, the water climbs the walls of the vase, while, with respect to the vase it is in relative rest. "That ascension indicates [...] the true and absolute circular motion of water, completely opposed to relative motion" Newton affirms. Today, the argument seems somewhat weak, but it took Mach to give a clear indication of the weakness: centrifugal forces are generated by a rotation relative to fixed stars; without that relativity, the forces would not exist.

Criticisms of the Newtonian idea followed quickly. Berkeley observed that if only one body existed in all the space, it makes no sense to claim that it moves or rotates from right to left, up or down. Leibniz's philosophical counter arguments are deeper. Motion must be measured considering the totality of the constituent bodies of the universe. For Leibniz, space is the *order of its co-existing elements* and time is *the order of the things that follow one another.* Newton maliciously adds: "I suppose that he meant to say that space is the order of co-existence in space and time the order of successive existence in time, that is space is space in space and time is time in time."[242]

We do not have space to pursue this fascinating argument. We shall close by observing that Newton wanted to found his philosophy on observation and experimentation on which he based his theories of absolute time and space, abstract ideas, completely removed from our perception.

6.6 The Laws of motion

The definitions are followed by three laws of motion: inertia, proportionality between force and acceleration, action and reaction. The three laws, today used a reference in any treatise of physics, are well known. Less known, and not conforming to use of those times, is the homage Newton pays to his predecessors in the scholium that closes this part of the treatise: "The principles that I have expounded so far are accepted by mathematicians and confirmed by numerous experiments. Using the first two laws and the first two corollaries (on the composition of forces), Galileo found that the fall of heavy weights occurs according to the square of time and that the motion of projectiles is parabolic, that is confirmed by experience, taking into account that their motion is rather retarded through the resistance of air [...]. From these two laws, and the third, Christopher Wren, John Wallis and Christaan Huygens, undoubtedly the principal geometricians of recent times, discovered, each on his own account, the laws of collision and the reflection of hard bodies, that are almost exactly the same, and almost at the same time were communicated to the Royal So ciety". On his part, Newton repeated the experiments on collision, already carried out by Wren and Mariotte (§ 5.11), using two pendulums of different mass and concludes that the quantity of movement is maintained in the collision of soft and hard, elastic and anelastic bodies.

Subsequently, to justify the principle of action and reaction, Newton thinks so in the case of attractions: if two mutually attracted bodies are separated by an obstacle and if the respective forces were not equal, the obstacle, pressed more on one side than the other, would move in the direction of the greater force and in the vacuum it would acquire a uniformly accelerated motion that would lead to the infinite, which is contrary to the first law; therefore the two bodies press equally on the obstacle. The conclusion is proven by Newton with experiments on a magnet and a floating iron.

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[242] From a manuscript of the Portsmouth Collection, referenced in Koyré and Cohen, *Newton and the Leibniz-Clarke Correspondence,* op. cit., p. 73.

Newton recognises, as we have seen, that the three laws of motion are not his. Also for the third law, that historians commonly attribute entirely to him, notwithstanding his explicit admission, the scientist acknowledges his predecessors, in which he might have included Descartes (in the second rule of *Le monde*) and we may add Leonardo da Vinci (§ 3.6). All the same, these acknowledgements take nothing away from the great achievements of Newton. He set down precise and general formulations of the laws, that have survived down the centuries, and formed the axiomatic foundations of the new mechanics. And the fact that Cotes, a man of acute ingenuity, did not accept the third law and wanted to remove it from the second edition demonstrates how far the science of the time was from so much generalization.

6.7 Centripetal motion

Definitions and laws are a sort of introduction to the treatise that begins with the first book and is arranged geometrically, which renders it difficult for modern readers used to analytic explanations. Very probably, Newton, precisely to be read and understood by as many people as possible, used the geometric form instead of differential calculus that he certainly knew and understood. But there are also people who claim to have heard him say that he wanted to make the treatment difficult to avoid the bother of having to explain it to less intelligent people. Even if the attributed phrase is true, we believe it was a joke, something that the scientist, albeit rarely, was not averse too.

The first book concerns the motion of bodies subject to central forces. In a very simple demonstration, Newton begins with the following theorem and its opposite: the motion of a material point subject to a central force is flat and acts in such a way that the areas described by the vector ray are proportional to the time required for their description. Successively, he establishes that the forces are moved forward when the areas described by the vector radius increase and, on the contrary, are moved backwards when they decrease.

The sixth corollary of proposition IV demonstrates that forces are inversely proportional to the squares of the distance when the squares of the periods are proportional to the cubes of the distance. Proposition VI sets forth a general theory on the motion of a body along a curved line around a centre. The theory is applied in the third section of the book that deals with the motion on conical sections. In a difficult reasoning, he demonstrates, in successive theorems, that when a body moves over a conical section it is subject to a centripetal force directed towards a focus on the section; it follows that in these cases the centripetal force is inversely proportional to the square of the vector radius. These are new theorems of mechanics, sufficient to interpret Kepler's empirical laws and to extend the new dynamics to planetary motions.

Proposition LIX demonstrates that when a system of several bodies *A, B, C, D, ...* is such that *A* attracts all the other bodies with forces inversely proportional to the distances and similarly does *B, A* and *B* are mutually attracted with forces proportional to their masses. In the scholium to the proposition, Newton again warns that the words "attraction" and "impulse" are used in a mathematical sense to indicate the trend that leads bodies to be attracted to each other, without saying anything on the nature of these trends.

The 12th section that immediately follows the previous proposition is dedicated to the mutual attraction between spherical bodies. The key of the treatise is the solution of a problem that had long troubled Newton and had delayed, as we shall see (§ 6.11), a dynamic interpretation of the world system: how does a material sphere behave in attracting an external material point? Newton replies in proposition LXXI: "A corpuscle placed outside the surface of the sphere will be attracted to the centre of the sphere through a force inversely proportional to the square of its distance from the centre". This is the same as saying that when the point is outside the sphere, the attraction occurs as if the entire mass of the sphere were concentrated in the centre: a proposition that Halley intuitively admitted before Newton's demonstration, but which Newton refused to admit. Proposition LXXII concludes by saying that when the point is inside the sphere, it is attracted by a force proportional to its distance from the centre. It follows that the two spheres made of homogeneous shells are attracted by forces inversely proportional to the square of the distance from the centres.

The 13th section concerns attractive forces of non-spherical bodies and introduces the 14th, and final, section dedicated to the motion of very small bodies subject to attractive forces by large bodies: these are theorems, according to the author, that apply to the reflection and refraction of light. But the application, as Newton was quick to acknowledge, is not the result of some hypothesis on the nature of light, be it corpuscular or not, but only the consequence of a proven analogy between the trajectory of these very small bodies and the trajectory of rays of light. In fact, the occultation of the Jupiter's satellites (§ 5.35) demonstrate that light propagates at a finite speed: Cartesian law is identical to that demonstrated by the motion of these corpuscles, Grimaldi's experiments show that the rays of light are abstract and curved along the edges of the bodies (§ 5.34); finally, as the theories of mechanics demonstrate for material corpuscle, also "the refraction of light occurs not at the point of incidence, but gradually through a continuous curving of the rays, partly occurring in the air before reaching the glass, and partly, if I am not wrong, in the glass after they has passed through" (scholium to proposition XCVI). In summary, Newton proposes that the granular composition of light is not a hypothesis but the results of factual experiments; to say the least, the deduction is a bit stretched. However, the analogy between the motion of these very small bodies and the propagation of light led him to close the book with some useful suggestions on the construction of lenses.

6.8 The motion of fluids

The first book of the *Principia* is entirely based on the supposition that bodies move in a non-resistant medium under only the action of the applied forces. To complete the science of motion it was necessary, as Newton did in the second book, to study the modifications to the laws discovered when bodies move in a fluid, as occurs for bodies on the surface of the Earth.

Wallis had supposed the resistance that a body meets moving in a fluid to be proportional to its velocity; but Huygens had observed that as velocity increases so the mass of fluid moved increases, therefore the resistance must be proportional to the square of the velocity. Newton comments on both cases but observes that a body moving in a fluid, in addition to moving the fluid, must also overcome its viscosity, so he sets resistance proportional to the sum of two terms: one proportional to the velocity and the other to the square of the velocity.

The results are applied to the motion of projectiles in air, the motion of bodies subject to centripetal forces in resistant mediums and pendular motion. They are proved by experiments on pendulums and experiments of bodies falling into air and water. Newton therefore starts the study of the influence of the shape of the body of the resistance it meets in motion. He expounds the theorem that resistance is proportional, *coeteris paribus,* to the maximum section of the body perpendicular to the direction of movement: these results naturally led him to study aerodynamic shapes, as we now call them, that is the form of solids that, all other circumstances being equal, find the least resistance to fluid motion.

In proposition XXVI he reprises the examination of the outflow of liquids from recipients. From Torricelli onward, many experimenters had dealt with the question but without adding anything new. Even Newton's treatment in the first edition is weak; but in the second edition, Newton correctly deduces the velocity of the outflow; he observes the "contracted vein" and measures it approximately, but does not provide a satisfactory explanation, invoking the convergent direction of the fluid threads. A few years later, in 1718, independently of Newton, Giovanni Poleni (1683-1761) studies the same phenomenon, determining the influence of the opening of the outflow and observing that the contracted vein disappears when a short cylindrical tube is attached to the orifice of the outflow.

6.9 Acoustics

The eighth section of the second book is important. It is entitled *Propagation of motion through fluids* and constitutes the theory of the propagation of motion in weighable mediums and sets out the fundamental theorems of wave motion. Newton starts from proposition XLI: "Pressure is not propagated in a fluid along straight lines if not when the fluid particles lie along a straight line". Basically, the proposition aims to say that in an indefinite fluid, when the pressure exercised at one point is intercepted by an obstacle, it will continue beyond the obstacle by moving sideways. This occurs, Newton adds, in the propagation of sound that even moves behind hills and, entering a room through a window, spreads to all angles.

In the next proposition, XLIV, he examines the alternative motion of a liquid in a U-tube and demonstrates that the oscillations are isochronous. Proposition XLVI uses for the first time, at least in a technical sense, the common modern expression of wave-length (*undarum latitudinem).* Proposition XLVIII deduces the velocity of propagation of an elastic wave, equal to the square root of the ratio between the module of elasticity and the density of the medium. In the scholium to proposition L, Newton advises that the new propositions apply to the propagation of sound that is nothing more than air pulses. This circumstance leads to experimental proof of the theorems through the measurement of the speed of sound, considering that because of the variation of temperature, and therefore the density and elasticity of air, the speed of sound must be greater in summer than in winter. Newton calculates that in spring and autumn the speed of the propagation of sound is 1142 feet per second (= 348 m/sec). In 1636, Mersenne had experimentally shown it to be 448 m/sec; the academicians of the Cimento had measured it at 360 m/sec; in 1738, a committee appointed by the Académie des sciences in Paris, consisting of Gian Domenico Cassini, Giacomo Filippo Maraldi and Nicolas-Louis De La Caille, found it to be 337 m/sec. This experimental data could mislead about the accuracy of Newton's calculations. But the truth is somewhat different. Newton arrived at his theoretical value through the basic hypothesis that sounds are "pulses of air", that is collisions spreading from one air particle to another. With this concept, he believes that the dimensions of the air particles have an important role in the propagation and he assessed the diameter of a particle to be 1/9 of the interval between the centres of two successive particles, increasing by 1/9 the speed of theoretical propagation, claiming that in the interval between two particles propagation is instantaneous. In addition, he increases by 1/20 the correct theoretical speed, to take account of the vapours present in the atmosphere (to the measure, in his opinion, of 1 to 10 compared to pure air), that contribute nothing, or little, to the motion of propagation. Now, all this deductive cavilling raises the doubt that Newton, seeing that the theoretical value calculated using his theory was too distant from the experimental values then known, tried, by introducing fanciful hypotheses and inventing coefficients, to bend his theory to the experimental results, to which he made reference generically, without saying whether they were his or belonged to others. Without the mentioned corrections, the speed of sound deduced by his theory is 979 feet per second, that is approximately 298 m/sec.

In the period between the first and second editions of the *Principia,* the memoirs on acoustics written by Joseph Saveur (1653-1716) were published by the Paris academy from 1700 and 1707. Saveur's excellent work starts from an examination of a phenomenon well-known to organ makers: if two pipes produce two sounds slightly different to the unison at the same time, periodic increases in the sound may be heard, like a drum roll. Saveur attributed the phenomenon to the periodic concordance of the vibrations of the two sounds. For example, when one of the two notes is produced with 32 vibrations per seconds and the other with 40, the finales of the fourth vibration of the first coincides with the finale of the fifth vibration of the second, thereby producing, after that period of time, a reinforcement of the sound. Based on this phenomenon and these considerations, Saveur established a method to determine the number of vibrations per second, that is sound frequency. From tubes, Saveur progressed to the experimentation of the vibrations of a string, introducing the use of the "paper knight", observing that the "knots" and "bellies" of vibrations (the terms still used in science are his) and noting that the basic note is accompanied by notes that have a wave-length 1/2, 1/3, 1/4 of the fundamental. He called these notes, in terms destined to remain in science, "higher harmonic tones". Last, Saveur was the first to examine the limits of hearing in relation to the frequency of vibrations. He set 25 vibrations per second as the lower limit for the perception of lower sounds and 12,800 vibrations per second as the upper limit for the perception of higher notes.

In the second edition of the *Principia,* Newton, using Saveur's experiments, initiated his first calculations of the wave lengths of sound, arriving at the conclusion, today well-known to physics, that in every open tube the wavelength of the sounds emitted is equal to double the length of the tube.

Newton concludes this part of his work thus: "*Et haec sunt praecipua phaenomena sonorum",* in the satisfied knowledge that he had reduced scientific acoustics to a chapter of mechanics, the position it still occupies today.

6.10 Confutation of Cartesian vortexes

The ninth and final section of the second book is dedicated to the confutation of the Cartesian theory of vortexes (§ 5.7) and interestingly opens with an "hypothesis", that is with one of the propositions that, according to Newton, *pro nihilo sunt habendae.* The proposition goes thus: "The resistance arising from a scarce fluency of the parts of a fluid is proportional, *coeteris paribus*, to the speed at which the parts of the fluid are separated one from the other". In other words, in viscous fluids, the different layers slide over each other and the resistance is proportional to the speed of the slipping of the layers.

Now, if the planets are moved by vortexes of thin matter, as Descartes claims, they should offer resistance to the motion of the thin matter. Supposing that this resistance follows the above law of viscous fluids, Newton demonstrates the impossibility of deducing Kepler's third law. Newton does not hide his pleasure about this result and in the scholium that closes the second book declares: "It is therefore evident that the planets are not transported by bodily vortexes [...]. The theory of vortexes goes against all astronomical phenomena and leads not so much to explain but to obscure celestial motions. From the contents of the first book, it may be understood how these movements may occur without vortexes, in free space; *System of the world* shall demonstrate this better".

The *System of the world* is the title and subject of the third book that follows immediately and to which we shall return in paragraph 6.12.

Many historians consider the second book of the *Principia* to be at variance with the general architecture of the work; more experimental compared to the others and poorer in conceptual contents, and compromising the organic nature of the work. We think that this judgement is unacceptable. On one hand, it was certainly necessary to give experimental proof of the credibility of the new dynamics: criticisms against Galilean dynamics (§§ 4.16-17) and post-Galilean dynamics (§ 5.3) were quite recent and not so completely withdrawn to be ignored. Basically, it had to be explained why mechanical phenomena occur in reality in ways not conforming to the laws of the first book. On the other hand, the Cartesian vortexes had been accepted by a majority of the leading scientists. The heavens needed to be cleaned up in order to proceed to a new interpretation of planetary motions. Finally, we do not believe that the second book of the *Principia* is out of place, although we admit that, while it is a fundamental text for physics, it is less rich in contents than the other two.

6.11 Digression on the concept of attraction

It might be useful to recap, albeit briefly, history to demonstrate that the accidental fall of an apple, a phenomenon already used by Kepler as an example, was not sufficient for Newton to be illuminated by the idea of "universal attraction" (we would point out that this expression is not to be found in the *Principia*). Friends and relatives have retold the episode, affirming to have heard it from his own mouth; Voltaire popularised it, but the episode, even if true, should be taken in another context.

A tendency to unite like with like had already been postulated by the early Greek schools of philosophy (Empedocles, Anaxagoras, Democritus); the idea persisted throughout the Middle Ages and the Renaissance, supported by the phenomenon of magnetic attraction that, in a certain sense, proved it or at least demonstrated it. The theory that attributed the cause of tides to the influence of the Moon and the Sun has classical origins too (around the III century B.C.), and was supported by many Renaissance scientists such as Cardano, Scaligero, Porta, and Kepler. The application of the idea of astrophysical attraction is unequivocal in one of Plutarch's (ca. 46-120 A.D.) dialogues that states that the Aristotelian tendency of bodies to their origin "proves that the Earth is not the centre of the universe but, with respect to objects that have been launched far from the Earth and return, they possess a certain community and natural affinity for the Earth. As the Sun attracts all its component parts, so the Earth attracts stone and makes it part of itself".[243] Even the Moon could fall to Earth, "but is guaranteed not to fall thanks to its motion and the momentum of its revolution exactly as stones in a sling cannot fall out due to the whirling circular motion".[244]

The question became particularly significant with the spread of the heliocentric system. In 1609, Kepler published the first two laws of the observation on planetary motion, followed by the third in 1618. But even before the discovery of these laws, he had asked himself what caused the movement of the planets around the Sun and of the Moon around the Earth. In the *Prodromus continens mysterium cosmographicum* (1596), he attributes

[243] Plutarch, *De facie quae cernitur in orbe lunae*, 324 E.
[244] *Ibid.,* 923 D.

the motion of the Moon to terrestrial attraction and affirms that the movement is incomprehensible without admitting the tendency to rest for all matter; the cause of movement is a *vis immateriata* that fights against the inertia of matter. In the case of planetary motion, the mechanical cause, the *virtus movens,* resides in the Sun and is diffused not spherically like light, but only along the plane of the solar equator, therefore decreases proportionally to distance. Kepler's theory became much clearer in his principal work, the *Astronomia nova seu physica coelestis* (1609), sometimes referred to by historians as *De stella Martis,* that states "Gravity is a bodily affection, reciprocal between two related bodies [*cognata*] that tends to unite or join together (that, in the order of things, is also a magnetic quality), so that it is more the Earth that attracts the stone than the stone that attracts the Earth. Heavy bodies (especially if we place the Earth at the centre of the world) are not led to the centre of the world as the centre of the world, but as the centre of a related round body, that is to the Earth. Therefore, wherever it is placed, that is wherever the Earth is transported by its animal faculty, the heavy bodies would move towards it. If the Earth were not round, the heavy bodies would not move according to the radius from any point to the centre of the Earth but would move to various points from different parts. If two stones were to be placed close together in any part of the world, outside the sphere of activity of a third connected body, similar to two magnetic bodies, they would meet at an intermediate point, each advancing towards the other of as much space as is comparatively the size of the other. If the moon and the Earth were not held in their orbits by an animal force or by some other equivalent force, the Earth would rise towards the Moon for a fifty-fourth part of the distance and the Moon would descend towards the Earth for around fifty-three-parts of the distance, and there they would stop, if they had the same substance and density. If the Earth should stop attracting its waters, all sea water would rise and flow into the body of the Moon".[245]

We are not yet at Newtonian attraction. The magnetic analogy is dominant is Keplerian thinking, whereby the attraction occurs only between related bodies, and therefore between the Sun and the planets, that are not related bodies, there is no attraction. In this reasoning, attraction is not the internal tendency of a body to join another, but a force exercised on the body from the outside.

It would seem that Newton did not know of Kepler's *Astronomia nova* before formulating the law of attraction (it is said that Newton, like Galileo, was not a great reader, but that is not plausible as the catalogue of his library,

[245] G. Kepler, *Astronomia nova [...] seu Physica coelestis, tradita commentariis de motibus stella Martis ex observationibus G.V. Tychonis Brahe, Introductio,* in Op. Cit., *Gesammelte Werke,* Beck, Munich 1937, Vol. 3, pp. 17-18.

compiled immediately after his death and discovered around 1920, lists 1896 books). In the *Principia* he cites Ismael Boulliau, Borelli and Hooke. In his *Astronomia philolaica* (1645), Boulliau argues against Kepler and denies that the Sun emanates a force, observing that if Kepler's thesis were true, the force would spread from surface to surface like light and therefore vary in inverse relation to the square of the distance from the Sun.

However, Borelli's theories were much more important to Newton. These considerations set out in a precise mathematical formulation of centrifugal force and gravity would immediately be transformed into a synthetic theory of planetary motion.

The relations between Hooke and Newton are more complex and still not completely clear. We shall limit ourselves here to a brief summary. As early as 1666, Hooke reported to the Royal Society some of his experiments to demonstrate, in line with magnetic attraction, the variation of the weight of bodies according to height; he later attempted to apply this idea to motion of planets, that he had realised must be subject to a continuous force. In 1674 Hooke published an essay of the motion of the Earth that towards the end states: "I will expound a cosmic system that differs in many details to every other system hereto known, but corresponding in every way to common laws of mechanics. The system is based on three hypotheses: I) all heavenly bodies, without exception, exercise an attraction or gravitation towards their central point, so that they not only attract their components and prevent them from moving away, as we observe on Earth, but also other heavenly bodies falling within their sphere of action. It follows, for example, that not only the Sun and the Moon exert an influence on the form and motion of the Earth - that in turn influences them - but also Mercury, Venus, Mars, Jupiter and Saturn with their attraction influence the motion of Earth [...]; 3) the forces of attraction are greater when the bodies that they influence are closer to the centre of attraction.".[246]

In a letter of 1680, Hooke informed Newton that he had been led to change the latter law of simple inverse proportionality and suppose that the attraction between two bodies was inversely proportional to the square of the distance. When Newton sent the manuscript of the *Principia* to the Royal Society, Hooke claimed priority rights in the law, but Newton reacted briskly, affirming that twenty years previously he had known of the law of inverse squares, that he had informed Huygens, through Oldenburg, secretary of the Royal Society, and that it was precisely from these letters that Hooke had come to know the law. Newton also played down Hooke's achievements, accusing him of using Borelli's work. Only later, through the

[246] R. Hooke, *An attempt to Prove the Animal Motion of the Earth from Observations,* London 1674, pp. 27-28.

friendly pressure of Halley, was he persuaded to recognise that it was a letter of Hooke that gave him the opportunity to determine planetary motion and he agreed to quote him in the *Principia,* precisely in the scholium to proposition IV of the first book in which we read that also Wren, Hooke and Halley had deduced from Kepler's third law the inverse of the squares for centripetal force. Newton's judgement is too harsh and still weighs heavily on the fame of the Boyle's old assistant. Hooke was a difficult character (and Newton was no better), but he had a rare inventive bent that led him to understand the fundamental laws of dynamics that support the heliocentric system, but he was not able to expound them properly because of the inconstancy in his character and his poor mathematical ability.

6.12 The system of the world

The third book of the *Principia* is prevalently astronomical and opens with a short introduction in which the scientist claims to have substituted a popular treatise already composed (that today is usually published as an appendix to the *Principia*)[247] by a scientific book, to avoid possible arguments between incompetent people. There follow the rules on the philosophy, already analysed (§ 6.2).

The true treatise begins with an exposition of phenomena, that is essentially the statement and illustration of Kepler's laws (§ 5.29). The propositions in the first book on centripetal force (§ 6.7) allow the scientist to give a dynamic interpretation of Kepler's laws, as today basically used in common treatises on mechanics, and to conclude that Jupiter's satellites, the primary planets, and the Moon are held in their orbit by a direct force towards the centre, respectively, of Jupiter, the Sun, the Earth, in inverse proportion to the square of the centres of the orbiting and the orbited bodies.

Proposition IV is especially significant, not only because of its historical value, but also because it helped Newton, as we shall explain later, to give an almost experimental proof of the earlier conclusion. It sets up the calculation to prove that the force maintaining the Moon in its orbit is the same, weakened by distance, that causes bodies to fall to Earth.

Brought up to date and a little simplified in terminology, Newton's calculation is the following. Let *T* be the centre of the Earth, *L* the centre of the Moon, *LM* the space that the Moon would travel through inertia in a minute (Fig. 6.2); conduit *MT* that crosses the lunar orbit in *N*, arc *LN* is almost equal to *LM* due to the reduced angle of *LTM* and easily calculated by knowing the radius of the lunar orbit (that Newton set at 60 terrestrial

[247] I. Newton, *The system of the world*, It. trans. by M. Renzoni, Boringhieri, Turin 1959.

radii) and the period of the Moon's revolution around the Earth. The Moon effectively travels across arc *LN* and therefore *MN*, almost equal to *LO* due to the very small angle LTM, represents the "fall" of the Moon towards the Earth in a minute. From the right triangle *LNP,* one can easily calculate, using a well-known theorem of Euclid,

$$LO = (LN)^2 : LP$$

and, as chord *LN* may be confused with the corresponding arc, all the elements may be derived to calculate *LO*. Newton calculated *LO*=15 Parisian feet, 1 inch and 1+4/9 lines (= 490,292 cm).

Now, if the force exercised on the Earth's surface is of the same nature, its intensity becomes 60^2 times greater; but as in falling of the bodies the spaces are proportional to the squares of the times, at the Earth's surface, the space travelled must be 60^2 times lower, that is exactly 15 feet, 1 inch and 1+4/9 lines. Huygens had discovered that in Paris a weight falls in a second 15 Parisian feet, on inch and 1 + 7/9 lines: the agreement between the calculation and the experiment (that improves, taking into account the movement of the Earth around the Sun and the Moon around the Earth) is so good that the first and second rules of philosophizing lead to the conclusion that the force holding up the Moon is of the same nature of that "we commonly call gravity".

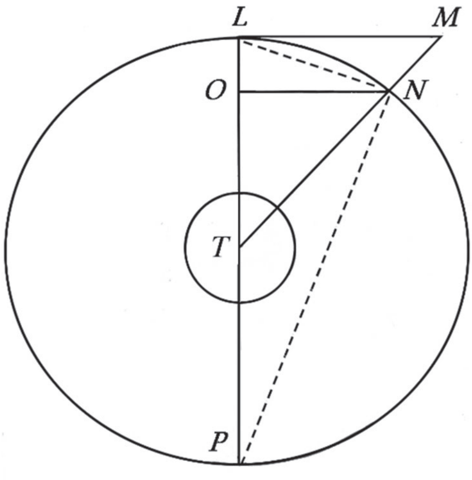

Fig. 6.2

The scholium to proposition IV provides a second demonstration based entirely on the first and second rules of the philosophy; if a small moon were to orbit the Earth close to the tops of the mountains, its centripetal force must be that of gravity because, otherwise, either the small moon would have not gravity or could fall, if its orbital motion would unexpectedly stop, with a velocity double that with which heavy bodies fall to Earth.

With reference to the above calculation, up until the XIX century, the following anecdote was recounted, and is still believed: around 1670 Newton, having guessed at the law of gravity, tried to prove it by comparing the gravity on the Moon with the gravity of Earth, but the calculation, based on an erroneous knowledge of the Earth's radius, resulted in an approximation that raised doubts in Newton over the exactness of the law of attraction; but in 1682, at a meeting of the Royal Society, he learnt of the new measurement of the degree of meridian made by Picard in France, he made new calculations and found a perfect correspondence between gravity on the moon and on the Earth.

The anecdote does not seem believable, as it is difficult to believe that Newton had to wait until 1682 to know the value of the Earth's radius later used in the *Principia*. That value was in fact given by Snell as early as 1617 and was contained in an edition of the *Geographia generalis* by Bernardo Varenius (died. 1660), published posthumously in Amsterdam in 1664 and re-published in 1672 in Cambridge, edited by Newton himself.

The conclusion arrived at in 1927 by the American astronomer J.C. Adams after a close examination of a large number of Newton's unpublished letters and manuscripts appears more probable. The main reason for the delay in announcing the discovery of the law of attraction can be found in the fact that for a long time Newton was unable to determine the attraction exercised by a spherical body on an external point. On the contrary, his letter to Halley demonstrates that at the time he did not acknowledge that the sphere attracts the external point as if all the mass were concentrated in the centre; while he himself made this demonstration later (§ 6.7).

However, following the above calculation, in proposition VII Newton states: "Gravity is present in all bodies and is proportional to the quantity of matter of the single bodies". But if attraction is a general property of all bodies, why do we not experience it in our everyday life? Newton foresaw the objection and replied in the first corollary to the proposition: "If somebody objects that all bodies found around us, according to this law, must gravitate one towards the other, while in reality that gravitation does not occur, I say that the gravitation towards these bodies, in comparison to the gravitation towards the Earth, is given by the ratio of the mass of these bodies to the Earth's mass, and therefore is too small to be noted".

But many of Newton's contemporaries judged the answer to be an easy way out, so that there persisted for some time doubts on the universality of Newton's Law and the lack of experimental proof was one of the reasons for the delayed diffusion of Newton's mechanics (§ 6.13). Only in 1798 did Henry Cavendish, using a torsion balance, exactly measure the attraction between two small spheres and confirm Newton's idea that in our common experience the attraction is so small that it is not noticed. Cavendish's method, with subsequent adjustments, led to the numerical determination of the universal gravitational constant in the XIX century. If *m* and *m'* are the mass, in grams, of two bodies, *r* their distance in centimetres, the attractive force between the two masses, expressed in dynes, may be given as

$$f = 6{,}67 \cdot 10^{-8} \frac{mm'}{r^2}$$

Returning to the summary of the third book of the *Principia,* proposition VIII, after confirmation of the theorems of the first book on the attraction between two spheres, begins with four corollaries on the calculation to determine the acceleration of gravity, mass and the density of each planet. Proposition XX studies how gravity varies at the surface of the Earth, concluding that accelerations of gravity at the poles and the equator are in the ratio 230/229 (slightly less that the approximate value 983/978 accepted today). This provides an opportunity for a brief history of gravimetric measures, from Richer in 1672 (§ 5.13) to Louis Feuillée (1660-1723) in 1704, with some acute criticisms such as the influence of temperature on the length of the pendulum (not then compensated) used to determine the acceleration of gravity.

Proposition XXIV affirms that tides are caused by the combined attraction of the moon and the Sun; the next proposition deals with the question set by Alexis-Claude Clairaut (1745), later known as "the three bodies problem", that has troubled mathematicians from Newton to our own times: how to determine the motion of three bodies (in Newton's problem, the Sun, Earth and Moon) mutually subject to Newton's law of attraction. The following propositions up to number XXXIX are dedicated to the study of the motions of the Moon. The last part of the treatise deals with the theory of comets, for thousands of years believed to be simple sub-lunar meteors. Tycho Brahe had demonstrated their nature of celestial bodies; Kepler assigned them a rectilinear trajectory; Johannes Hevel, or Hevelius, (1611-1687), Hooke and others theorised a curved trajectory around the Sun. In the last three propositions, Newton shows that comets are celestial bodies that obey gravitational law like the planets so that their trajectories are elliptic or parabolic, according to the resultant of the forces being exercised, in which the Sun is one of the focal points.

The work closes with a famous general scholium, introduced in the second edition and retained in the third, that returns to the polemic against Cartesian philosophy with a counter-argument of the “physics of principles”, as we have already mentioned in paragraph 6.2, that assigns to science the responsibility of describing phenomena, not discovering their causes. It is a stance already taken by Galileo and would be repeated in the Enlightenment and Positivism of the 19th century. But the predominant motive of the general scholium is the doctrine of God: his presence and influence in the universe.

6.13 Acceptance of the “Principia”

On the publication of the *Principia*, a profound and difficult text, the scientific world immediately had the sensation of finding itself faced with a great work, so complex that even professional mathematicians found it difficult to read. Some fundamental concepts, principally attraction, met with strong opposition. Leibniz and the Cartesians criticised the idea of attraction: this attribute is inherent in bodies, they basically claimed, that acts at a distance, is a return to the occult qualities of the Scholastics. Cotes replies, in the preface to the second edition of the *Principia*, reproduced in the third, that a cause may not be termed occult when its existence is clearly proved by observation; on the contrary, occult are the causes adopted by those who claim that the motions of the planets depend “on vortexes of a purely fictional matter and completely unknown to the senses”.

The reply is polemic, but not convincing: up until Einstein, gravitation remained a scientific dogma, one of many incomprehensible phenomena, as Mach put it. Newton himself found action at a distance absurd without the intervention of an agent, but he always refused to take a public stand on the nature of the force of gravity.

According to a note by David Gregory (1661-1710), dated 21 December 1705 but published only in 1937, Newton had found an answer to the problem. It was a mystical-religious solution that appears at the end of the *Principia* and the *Opticks* in terms that hereto had been translated, literary expressions of a religious spirit (“the motions of bodies feel no resistance in God’s omnipresence”, in the *Principia;* “God is everywhere, even in things”, he adds in the *Opticks*). Gregory’s note shows that Newton’s answer would be: the mediator of distant action is God, present as much in the space free of bodies as where there are bodies.

This is not physical or metaphysical hypothesis, this is pure theology! No for nothing do some modern critics (René Dugas, Louis De Broglie) judge Newton to be a great visionary, immediately adding that "ce sont les visionnaires qui créent (visionaries are those who create)".[248]

But let us return to how the *Principia* was received. Locke, whose knowledge of mathematics was modest, after asking Huygens about fairness of the mathematical proofs contained in the *Principia* and getting reassurance of their correctness, became a fervent Newtonian, followed by a large part of British scientists. They were divided into two movements: the great majority, following the doctrine set out by Cotes in the preface to the second edition of the *Principia,* believed attraction to be an inherent or "essential" property of matter, thereby throwing a line, as the saying goes, to opponents of the occult qualities; others, such as Pemberton and the mathematician Colin Maclaurin, stayed with Newton's prudent position (*hypotheses non fingo)* while still affirming that gravity is a "universal" property of matter.

In continental Europe, on the other hand, the anti-Newtonian polemic was thriving and combined with the "great argument" between Newton and Leibniz over priority in the discovery of infinitesimal calculus. Showing exemplary loyalty, Huygens recognised Newton's huge merits, even while not sharing many points of view. Maupertuis supported the principle of attraction, invoking the omnipotence of God, but Johann I Bernoulli opposed him using a curious objection. He represented attraction as due to an emanation coming from each particle of matter along straight lines; these rays hooked, like retractable elastic threads, the other particles they encountered. In this representation, the attracting force of a body on a particle of matter should change in inverse proportion to the cube of the distances, because the number of rays emanated by an attracting body and that hook the particle varies with the mass of the body and not the surface. To conclude, the law of the inverse of squares is incompatible with the law of the proportionality of the masses. The Newtonians replied to Bernoulli's objection using metaphysical considerations that left the question as open as ever.

Finally, for more than fifty years after the publication of the *Principia*, the schools of continental Europe continued to teach Cartesian physics that, in addition to being easily understood, required fewer mathematical means. The Newtonian vision of the world was established through its operative fertility that fascinated the Enlightenment, the real proponents of Newtonian thinking. In the second half of the 18th century, it became fashionable to

[248] L. De Broglie, Nouvelles perspectives en microphysique, Michel, Paris 1956 p. 334.

judge scientists to be progressive if they were Newtonian and conservative if Cartesian: a classification that, if still sometimes adhered to, is purely formal and without any intrinsic value

OPTICS

6.14 The mirrored telescope

Newton's biographers agree that the scientist's interest in optics can be dated to 1664. Newton himself recalls that in 1666 he had procured a prism to "test the famous phenomena of colours". Probably, he obtained his first successes during his voluntary retirement to the peaceful countryside of Woolsthorpe that we have already mentioned (§ 6.1). In 1668, his teacher Barrow recognised his expertise in optics and trusted him the revision of his own work *Lectiones opticae et geometricae* (Londini 1674, but permission for publication dates it at 1668). The collaboration confuses many of Newton's biographers who ask why he made no comment on Barrow's antiquated ideas on colours. Some of them conclude that, despite the explicit contrary affirmations in the first memoir of 1672, with which we deal later, in 1668 Newton had not obtained any fundamental results on the nature of colours. In our opinion, this is a hasty conclusion, as to revise the work of another, especially when this other person is your teacher, does not mean to substitute your own ideas for those of the author.

In 1669, after being elected to the Lucasian chair at Cambridge, Newton began teaching precisely optics. His *Lectiones opticae*, published posthumously in 1729, date from this period and come after the great success of the treatise on optics. They are divided into two parts: the first deals with refraction, the second with the theory of colours. In the second, truly innovative part, the young scientist first ridicules the "idiotic" theories of philosophers on the nature of colours (§ 5.30) to pass onto an exposition of his own, which we will treat on paragraph 6.16. He closed the course of lessons with an invitation to geometricians to study natural phenomena and natural philosophers to study geometry: thus, posterity will promote an understanding of nature through exact demonstrations.

It seems that the *Lectiones opticae* were not keenly followed by the students, and even less understood, so that they contributed little or nothing to the diffusion of Newton's invention, that came about by other means.

Towards the end of 1671, encouraged by his friends, Newton sent King Charles II a present of an example of his new type of telescope. The King asked the Royal Society to examine it. Its significance was recognised and Newton was deemed worthy of being elected a member. On 6 February

1672, Newton sent the Royal Society a note, drawn up by Oldenburg as a letter, the first part of which is dedicated to the genesis of his invention.

The critical examination of the dispersion of light, of which we will talk in paragraph 6.16, led Newton to believe that "light is made up of differently refractable rays" so that to an object-point in a lens does not correspond an image-point. This conclusion persuaded him that it would be vain to try and increase the performance of the telescope, improving machining of lenses or making non-spherical lenses, according to the current popular research and that even Newton had begun to pursue. It would have been to turn his attention to mirrored spyglasses, or simply telescopes as Newton preferred to call them.

As we know, these are tools in which the image of an object, given by a concave mirror, is observed through a magnifying glass. This type of instrument had already been studied by Sarpi and Porta (§ 3.17); Galileo had discussed it with his pupils; Cavalieri had expressed a negative opinion; and Nicola Zucchi (1586-1670) had described it in his *Optica*[249] and it would seem that he had built an example in 1616. In Zucchi's telescope, the rays reflected by a large metallic parabolic mirror are received by a small co-axial glass mirror and observed through a lens placed in a hole in the centre of the large mirror. Mersenne had constructed two parabolic mirrors and suppressed the lens: the resulting image from the small mirror could be looked directly through the hole in the large mirror. This principle was the inspiration for James Gregory's (1638-1675) telescope of 1663.

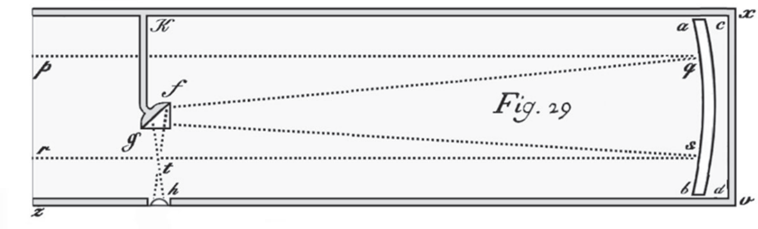

Fig. 6.3 - Newton's telescope with a full reflecting prism: in proximity to the focus of the mirror *a q s b* there is the total reflecting mirror *g f* that sends the rays to the ocular. The prism does the same job as the flat mirror, but was recommended by Newton to reduce the length of the telescope.

But Newton probably knew nothing of these previous discoveries and through his own personal research over two years, in 1668, he managed to

[249] N. Zucchi, *Optica philosophia experimentiis et ratione a fundamentis constituta,* Londini 1652, part. I, pp. 34-35.

construct his first telescope that differed from preceding ones for a simple and ingenious detail. In previous instruments, the piercing of the large mirror at its central point suppressed the most active part of the incidental rays. On the contrary, Newton collected the image of the concave mirror on a small flat mirror inclined at 45° to the axis of the telescope and observed it through a magnifying lens placed laterally to the telescopic barrel (Fig. 6.3): a very simple arrangement, although rather inconvenient.

The first small model, 15 centimetres long with a 25 millimetres diameter mirror, had a magnification of 40, that allowed the satellites of Jupiter to be observed but it also gave dim images. He stopped working on the project only to return to it in the autumn of 1671, constructing a larger model described by Newton in a second note of 1672,[250] because the first, as we have mentioned above, was almost entirely dedicated, as the title indicated, to the theory of colours, then repeated word for word in the treatise of optics (§ 6.15).[251]

Following the publication of these two notes, the polemics we mentioned in paragraph 6.1 began and from 1672 to 1676 the "Philosophical Transactions" published dozens of argumentative letters by Newton to Oldenburg, without adding anything to his work on optics. But we know that Newton dedicated himself to perfecting the telescope for at least fifteen years, studying different alloys to build the mirror and, above all, introducing a new smoothing method.

The reflecting telescope, the starting point for all of Newton's scientific activity, was the instrument that, improved by Lomonosov around 1762 and independently by Friedrich Wilhelm (later Sir William) Herschel (1738-1822) in 1789, was part of the entire development of instrumental astronomy down to modern times: the mirror gradually passed from the 25 mm of the first example built by Newton to 200 inches (6.08 m) of the gigantic reflector installed in 1940 on Mount Palomar in the USA.

The reflecting microscope proposed by Newton in a note of 1672 but never built was rather less successful. Giovan Battista Amici (1786-1863) made one in 1827 and it was used only in the XX century for some special researches.

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250 *An account of a New Catadioptrical Telescope Invented by Mr Newton,* in "Philosophical Transactions", 7, 1672, pp. 4004-09. The note also refers to a very flattering comment by Huygens.

251 *A letter of Mr. Isaac Newton [...] Containing His New Theory About Light and Colours,* in "Philosophical Transactions", 6, 1671-72, pp. 3075-87.

6.15 The treatise on optics; definitions

In a letter to Oldenburg of 18 November 1676, Newton, disgusted by the continuing arguments, suggested abandoning natural philosophy for ever, and he would pursue it only for his personal satisfaction without permitting any publicity until after his death. And he added that a man must choose between proposing nothing new or becoming a slave to defending the changes announced.

The scientist was talked out of this sullen attitude by his friends (Gregory, Halley and others) and by a sad event: in 1703, Hooke, his most dangerous adversary, died after a long illness. The pressure of his friends and the death of Hooke led Newton to publish, in 1704, a fundamental text: *Opticks; or a Treatise of Reflextions, Refractions, Inflexion and Colours of Light;* the text was followed by two papers on mathematics (omitted in subsequent editions) that inflamed the argument with Leibniz on the priority of the invention of differential calculus.

In the preface, Newton states that a large part of the work was written in 1675 and sent to the secretary of Royal Society who read it out to the Meeting. Twelve years later, Newton added the rest to complete the theory, and later added a third book. Newton himself edited a second edition in 1717 and a third in 1721. The book was translated into Latin in 1706 by Samuel Clarke with the permission, and under the control, of the author who was so happy with the result that he paid the translator five-hundred guineas, a huge sum at the time. In 1720, it was translated into French by Pierre Coste and revised by Desaguliers. In the XVIII century, Clarke's Latin edition was very popular and reprinted several times. We will follow this edition.

The work consists of three books. The first, divided into two parts, deals with reflection, refraction and dispersion (analysis and synthesis of colours) with an application to an explanation of the rainbow and a digression on reflecting telescopes. The second, in four parts, deals with the colouration of thin sheets. The third contains a brief experimental study of diffraction and closes with thirty-one theoretical "questions".

The text opens with a declaration of faithfulness in the experimental approach and a promise to describe phenomena without advancing theories: "In writing this book - the author states - I have not set out to make any hypotheses and from these to explain the properties of light, but to prove these properties simply through reason and experimentation. To this end, I set down the following definitions and axioms".[252]

[252] I. Newton, *Optice, sive de reflexionibus, refractionibus, inflexionibus et coloribus lucis, libri tres,* Lausanne et Genevae 1740, p. 1.

We cannot honestly say that Newton stuck to this programme. In fact, immediately after these words, the reader comes across the first definition that either has no meaning or acquires a meaning in a frankly corpuscular theory. The definition states: “When I say rays of light, I mean their minimum part, both those subsequent on the same line and those contemporary in different lines”.[253] And what means that the “ray of light is the minimum part of light”? It means that for Newton the ray of light is no longer a trajectory, invented and refined by Greek geometricians, but is that *minimum lumen aut minima luminis pars,* as the elucidation that follows the definition says that “by itself, without the remaining light either may be intercepted or be subject to some action that the rest of the light does not exercise at the same time”. In short, Newton, a victim of a recurrent delusion in all experimenters, at the same time as declaring his attention to following facts rather than theories, bases the interpretation of his experiment on a new theoretical concept of light rays: the corpuscular concept or, if we want to use a Galilean term that became current use in the XX century, quantistic.

A further eight definitions follow, some as obscure as the first, and eight “axioms” that summarise the elementary geometric optics of the time: laws on reflection and refraction, and the formation of images by reflection or refraction.

6.16 Dispersion of light and the nature of colour

The experimental part that follows has been proved and has basically remained at the foundations of our physical optics. Any emphasis on the genius of the concept, the skill of execution, the accuracy of the measuring, may seem rhetorical. We should only underline the leap forward Newton made with the study of refraction in prisms to which innumerable physicists dedicated their studies, from Maurolico onwards, or even, we might add, from Seneca.

A first group of very simple experiments, consisting of observing through a prism a two-colour - red and turquoise - piece of paper, illuminated by sunlight, led the scientist to a fundamental conclusion: “Lights differentiated into colours are also differentiated by grade of refrangibility”.[254] The proposition was not entirely new; Marci had stated it in 1648 (§ 5.34) and maybe Newton knew of it because in the note of 1672 and the treatise he demonstrated that diverse refrangibility could not be attributed to the diverse incidence of the rays of light coming from different points of the

[253] *Ibid.,* p. 2.
[254] *Ibid.,* p. 13.

Sun: it is this exact interpretation that Marci gave in his own experiments with prisms.

Newton's interpretation is very different, confirmed in a set of experiments already described in the note of 1672. After making a small circular hole in the aperture of the camera oscura, Newton let sunlight enter on a very dispersive glass prism and collected the "spectrum" (the technical term is his) on the facing wall, about six metres away. He acutely observed that the best experimental conditions were obtained when the prism was, as we now say, in a position of minimal deviation, that is easily reached by rotating the prism around its axis. In this disposition, a first set of experiments, the most outstanding of which was the two crossed prisms (that is with perpendicular corners) that gave a shifted, but not enlarged or altered, spectrum, proving to Newton that the colours were present in sunlight and the prism had no other effect than separating them. In another experiment, that in the note of 1672 Newton called the *experimentum crucis,* he decomposed light using the prism, collected the spectrum on a screen, then opened a small hole and directed the emergent light to a second prism that deviated it but no longer broke it down. This group of phenomena, fundamental in spectroscopy, gave Newton the idea to homogeneous light: "Each homogeneous light has its own colour corresponding to its refrangibility, that cannot be altered by any reflection or refraction".[255] This therefore established a correspondence between refrangibility and colour, with a consequent correction of the Cartesian law of refraction: the index of refraction is indeed constant for two specific materials, whatever the angle of incidence, but changes with the colour of the incident light and Newton proved the result with experiments identical to those repeated in schools today. It was this observation (§ 6.14) that led him to abandon working on improvements to telescopic lenses.

At this point, Newton makes a critical examination of the question of the purity of the spectrum and describes an experimental tool made up of a lens and a prism, that is nothing more than Fraunhofer's spectroscopic collimator. How is it, then, that Newton did not see the black lines of the solar spectrum? Perhaps because his sight was failing and he needed the eyes of his assistant in his observation. The circumstance was providential because the dark lines would have been a complication that Newton would have found difficult to explain.

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[255] *Ibid.,* p. 87

6.17 Research into dispersion

In proposition VI of the first part of the first book, Newton returns to the experiment on crossed prisms from which he believed to deduce, experimentally and theoretically, that when the index of refraction is known for one colour of the light, it is known in every other case. In other words, the proposition signifies that dispersion is proportional to refraction: this is the famous “Newton’s error”. He reached this conclusion in 1672 when he measured the length of the spectrum obtained with his prism whose refracting angle was 63°12'. He found it five times the width and believed that with any other substance the ratio would be the same, as long as the refracting angle of the prism was the same. That meant that, according to Newton, the entire spectrum may be more or less deviated by prisms of different substances, but given equal geometric conditions, it will always retain the same length and the various coloured parts will always have the same width.

In 1676, the Belgian physicist Antoine Lucas sent a letter to Oldenburg through the “Philosophical Transactions” of that year, stating that he had repeated and found correct Newton’s experiments of 1672, apart from one quantitative detail: his own 60° refracting prism gave a spectrum three or three and half times longer than wide instead of the five times found by Newton. The latter, instead of repeating the experiment, replied that the different result obtained by Lucas must be attributed to the difference in the refracting angle of his prism or the different luminosity of the day of the experiment or, finally, why not, the poor accuracy of the measurement. He therefore invited Lucas at the end of a reply addressed to Oldenburg to repeat the experiments more attentively. The arguments are too captious not to raise doubts that Newton’s obstinacy was not preconceived. Consequently, some historians believe that his attitude was due to a certain analogy he saw between the spectral prism and a musical octave. Maybe, more than to this analogy. the “Newton’s error” was due to casual unfortunate circumstances: he happened to be working with glass prisms that had very similar optical qualities and with water prisms that used rain water rendered clearer with the addition with lead vinegar partly dissolved in water, bringing it close to the optical qualities of glass.

If Newton’s theory were true, achromatism would be impossible and the “disturbance of colour” would be inevitable in spyglasses. As in the note of 1672, as in the treatise on optics, this argument led Newton to describe reflecting telescopes. The first part of the first book on optics closes with a description of the instrument that differs from that described in 1672 in that the concave mirror is not metallic but glass made reflecting on the convex side, while the flat mirror is replaced by a totally reflecting prism. Consequently, this modest but priceless tool entered into the history of optical instruments.

"Newton's error" delayed the advance of optical instruments for around forty years, especially in the construction of telescopes, as we shall describe better later, but for the moment we shall add that it is well-known that the length of the spectrum depends on the material of the prism. In particular, in 1836 Augustin-Louis Cauchy (1789-1857) set down the formula of the index of refraction *n* for a given wavelength λ

$$n = \frac{A}{\lambda} + \frac{B}{\lambda^2} + \frac{C}{\lambda^3} + \cdots$$

where *A, B, C...* are constants that depend on the nature of the material, therefore it is possible, using Cauchy's formula (or others of the same type), to calculate the length of the spectrum for each material, set the other geometrical conditions.

On the contrary, Newton believed that he was able to posit a general relation between the index of refraction and the density of the medium, theorising an attraction of the bodies on light. Proposition X of the third part of the second book of *Optics* states: "When the light is faster in the bodies than in the vacuum [...] the forces of the bodies in the refraction of the light are more or less proportional to their density, except for the oily and sulphurous bodies that produce stronger refractions than other bodies of the same density".[256] Newton demonstrates the proposition by considering a ray of light *IC* (Fig. 6.4) grazing a reflecting surface *AB* and refracted in *CR*.

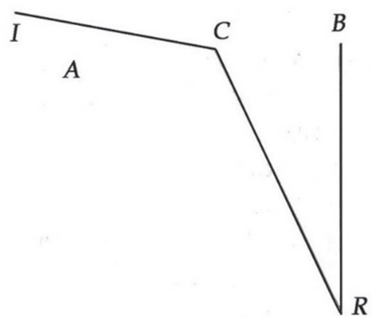

Fig. 6.4

[256] *Ibid.,* p. 209.

Dividing the speed of light into two components, one parallel to the refracting surface that remains unchanged and the other normal to the surface that varies to take into account the attraction of the refracting body on the ray of light. Newton concludes: "I declare that the square of line *BR* (supposing *CB* to be the unit of measurement) and consequently the refracting force of the body is approximately the same as the density of the body".[257] Taking l as the limit of the angle for the material under study and d its density, the proposition easily translates into the formula

$$\tan(90° - l) = kd$$

and as $\sin l = 1/n$, after some simple transformation, we obtain

$$\frac{n^2 - 1}{d} = k$$

The formula, named of Laplace as the French scientist wrote the formula in symbols, is today replaced by the following formula written by the Danish Ludwig Lorenz (1829-1891) in 1867 and deduced independently in 1880 from the Maxwellian theory of light by the Dutch physicist Hendrik Antoon Lorentz (1853-1928):

$$\frac{n^2 - 1}{(n^2 + 2)d} = k$$

Their formal similarity notwithstanding, Newton's and Lorenz/Lorentz's formulae are conceptually very different. According to Newton, k is a constant for all bodies except for "oily and sulphurous" ones (Newton tries to prove his proposition through an extended experimental table) while for Lorenz and Lorentz, k is constant in every body whatever its states of aggregation, but changes from body to body. In other words, there is no general relation between the index of refraction and the density of the bodies, as Hooke had already experimented (§ 5.34).

6.18 The theory of colours

In a note of 1672, Newton summarised his theory on colours in thirteen propositions that remained basically unchanged in the second part of the first book of the treatise on optics, where they are more broadly described

[257] *Ibid.,* p. 210.

and are supported by more experimental proofs. The fundamental proposition, that revolutionised centuries-old tradition, is that it is not the bodies on which light falls that produce the colours, nor *philosophice et proprie* are the rays of light that are coloured, but they have a certain disposition that gives us the sensation of this or that colour. Following a secular tradition Newton excludes black and white from colours and lists seven colours (red, orange, yellow, green, blue, indigo, violet).

And after an analysis of an admirable set of experiments, Newton synthesises colours. Some of these experiments remained classic and are still referred to in physical text-books, such as that of the comb that, moved rapidly backwards and forwards in front of a spectrum, makes it look white because of the persistence of the images, that Newton very accurately explained; as he did with the recomposition of white light with a rotating disc ("Newton's disc"). In the last experiment, the scientist returns to the optical-musical analogy. In fact, he states that the circular sectors, coloured with the various colours of the spectrum into which the disc is divided, have width proportional to the seven intervals of an octave.

The Newtonian theory necessarily leads to a net distinction between monochromatic light and the sensation of colour. The perception of colour becomes a psycho-physiological phenomenon, that is "sensory", with no unequivocal correspondence between the sensation of colour and the simplicity or complexity of light that produced it. In fact, the sensation of colour is so subjective as to be produced by causes other than light, as can be seen in dreams or the flashes resulting from a punch in the eye or the colours perceived by pressing the eyeball, and so on.

The preceding properties allowed Newton to give a new and more complete explanation of the rainbow and to interpret the colour of bodies as the effect of selective absorption of the rays striking them. But this latter part is not free from criticism. An experiment demonstrated to Newton that the colours resulting from absorption behave differently from the colours of the spectrum. Notwithstanding that, he believes he could apply the rules of the mixing of coloured pigments to the colours of the spectrum and states, for example, that green in the spectrum is obtained through a mixture of yellow and turquoise pigments.

6.19 "Rings", thin layers and absorption

The first part of the second book of the *Optics,* divided into four parts, describes a series of masterly experiments, that have remained classics, conducted with rare ability, such as to constitute a masterpiece of the experimental art. Newton returns to the study of thin layers begun by Hooke

(§ 5.34). But while Hooke experimented with strips of the same thickness that he tried to measure without success, Newton returned to Boyle's genial idea of experimentation with strips of continuously variable thickness. The classic experiment is well known: a flat-convex lens, slightly curved, supported on the flat part by another bi-convex lens. If the face of the lens is struck by white light, Newton, as Boyle before him, and all students of physics after him, noted, by observing the reflection, that is from the same point of arrival of the light, a dark spot corresponding to the point of contact of the two lenses, surrounded by a succession of concentric rings alternatively dark and iridescent. Accurate measurements allowed him also to determine the thickness of the corresponding layer of air in each ring, that proved to be some tenths of a millionth of an inch, that is a few millionths of a millimetre.

In addition to white light, Newton observed the phenomena in monochromatic light. Qualitatively, the phenomenon is identical, but while in white light Newton managed to observe only eight of nine rings, in monochromatic light, he saw dozens of them. The phenomenon is one of his most spectacular observations: if the rings obtained in white light are observed through a prism, each iridescent ring appears to be made up of a system of numerous rings of various colours, each separated from the other.

A number of repeated experiments on these magnificent phenomena and precise measurements permitted Newton to discover diverse laws that are still valid, such as: the rays of the rings (clear and dark) grow as the square root of their order number, as the fourth ring has a ray twice as big as the first and the ninth three times; the rings become stricter as the light becomes more refractable, that is the radii of the rings of the same number regularly decrease with the variation in colour, from red to violet; the dark rings are always formed in correspondence to the thickness of the layers multiples of a given minimum value that varies with the colour; the width corresponding to the red rings was 14/9 of the correspondent width of the violet ring of the same order; the rings become closer when water is introduced between the two lenses. With regard to the ratio between the width of red and violet rays, Newton returns to the optical-musical analogy: he sustains that the thickness of air corresponding to the seven subsequent rings coloured with the seven colours of the spectrum have the same relation as the cube root of the square roots of the lengths of a musical string, that gives the notes of an octave. There follows a study of the colouring of soap bubbles and thin layers.

It is a set of quantitative experimental results that can only excite the greatest admiration and that cannot but prove a periodic phenomenon characteristic to each colour. Newton therefore felt constrained to give at least a formal interpretation of periodicity. To this end, he begins by observing that matter should be considered porous, that is composed of a few grains drowned in empty space, in the way mist is made up of drops of water separated by air. It follows that the reflection of light cannot be due to the elastic collision of luminiferous corpuscles against the matter; and many luminous phenomena confirm, in Newton's view, this opinion. And so? … So "each ray of light passing through any refracting surface assumes a determined temporary disposition, or condition, that always returns at equal time intervals during the crossing of the ray; each time this condition is repeated, it induces the ray to cross the refracting surface; in the interval until the return to the same condition, the ray is reflected".[258]

Once the "fits" (*vices* in the Latin translation) have been defined for the reflection or refraction as a periodic return of the disposition of the ray to reflection or refraction and the period of the fits as the time interval between the two successive accesses, Newton asks why the light striking the surface separating two media is in part reflected and in part refracted. He replies: "Light has its entries of easy reflection and easy transmission before it falls on the transparent bodies. And it is very probable that these entries arise when light is emitted by luminous bodies and maintains it throughout its passage".[259]

But, in any case, are these fits twinned with the light, intrinsic at the same moment it is emitted or accidental, acquired at the moment the light crosses the bodies, as he will claim later (§ 6.21)? Obviously, they are related or acquired, whichever suits you best. Newton was aware of the contradiction and his uncomfortable position, but insisted in saying that he did not invent hypotheses and the fits are a fact, whatever their nature. But he immediately adds that those who like a representative hypothesis of the Cartesian type may imagine that, like stones falling in a pond cause a certain wave motion, so luminous corpuscles colliding with reflecting and refracting surfaces generate waves that spread more rapidly than the corpuscles and therefore precede them, strike them and determine and condition the fits to easy transmission and easy reflection.

But he did not question whether this theory was true or false. He concludes: "It is enough for me to have found that rays of light are such that, for whatever reason, sometimes they are more easily activated to reflect, sometimes to refract".[260] In any case, the theory gave an interpretation, albeit formal, of a simple phenomenon that had seemed inexplicable for thousands of years: why does light, hitting a surface dividing two media, partly reflects and partly refracts?

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[258] *Ibid.,* p. 216.
[259] *Ibid.,* p. 219.
[260] *Ibid.,* p. 218.

The contradictions, doubts and changes of mind notwithstanding, we believe that the theory of fits is a profound idea that today, in the light of wave mechanics, may be understood better and evaluated more accurately.

The scientist asks another question, linked to the preceding one: “What is the relation between the phenomena of thin layers and the colours of natural bodies?”.[261] As we have seen, he had established that a body observed in reflection, reflects some colours and the resulting colour appears to the observer. But why does it reflect those colours? Newton asks. He replies with a rather curious theory: almost all natural bodies are, due to their constitution, transparent, as can easily be observed by reducing them to thin layers. The opacity is due to the multiple reflections affecting the light on the small particles making up the bodies. To sum up, according to Newton, there is no true absorption. Now the bodies, at their surface, are made up of layers that, depending on their different thicknesses, reflect the rays of a certain colour and allow those of another colour to pass through; moreover, the colour presented by the natural bodies allows us to deduce the thickness of the their superficial layers that is equal to the size of the constituent corpuscles of the plate, and perhaps “by means of a microscope that enlarges three or four thousand times, we may reveal all the corpuscles [...] but if we get that far, I fear that it is not the extreme limit which sigh can perceive; because it seems absolutely impossible, due to the transparency of the corpuscles, to discover what is the most secret and exquisite work of nature”.[262] Given Newton’s intuition that corpuscles are our atoms, these words appear to us the words of a prophet; for his contemporaries, the words of a madman.

6.20 Diffraction and double refraction

The first part of the third book of *Optics* contains a small number of experiments on the phenomena discovered by Grimaldi, but Newton tried hard to avoid the word “diffraction”, using in its place “inflection”, as he had done in the sub-title of the work. Grimaldi’s experiments (Hooke is not mentioned) are repeated by passing a beam of sunlight in a *camera oscura* through a hole with a diameter of 1/42 inches (=0.6 mm) and placing small obstacles in its path, such as needles, straw, hair; the fringes of diffraction, in both white light and monochromatic light, are formed on a screen placed at some metres away from the obstacle. These are experiments that are far below, in terms of concept and interpretation, from the experiments described in the first two books and are prearranged to include diffraction

[261] *Ibid.,* p. 186.
[262] *Ibid.,* p. 201.

in the attraction of matter on luminous corpuscles: that is, rays of light passing close to the borders of the bodies are attracted and therefore inflected. Newton understood the experimental and theoretical weakness of this part of the work and honestly concluded by confessing: “When I made the preceding observations, I proposed to repeat most of them with greater care and to add others to explore how and why rays of light inflect on passing close to the borders of all bodies, so as to form those fringes without intermediate lines. But from these studies I was distracted by chance, and I cannot now return to the interrupted studies. Therefore, not having finished this part of my project, I will conclude by advancing some questions with which others may later be encouraged to continue this subject”.[263]

Newton had even less time to study double refraction than diffraction and limits the argument in the *Questioni,* that is, as we shall soon see (§ 6.22), to the part of the work to which he could not have had accommodation. Questions XXV and XXVI are given over to the phenomenon described by Bartholin (§ 5.35) and “more exactly” by Huygens, who, after this fleeting reference, is not mentioned again in the treatise. Having described the phenomenon, Newton sets down a law, irreparably wrong and too long to be described here, to construct the extraordinary ray issuing from the incident ray, and deduces that when two crystals of Iceland spato are placed equally in line, the ordinary rays emerging from the first crystal are refracted in the second according to the ordinary law of refraction, while the extraordinary rays emerging from the first crystal are refracted in an extraordinary way in the second. The result led him to believe that “there exists in rays of light an original difference in virtue of which some rays are constantly refracted ordinarily and others constantly refracted extraordinarily”.[264]

In the next proposition, Newton asks what this original difference may be: does it nor perhaps consist in the fact that the rays of light have *diversa latera* with different properties? And he repeatedly states: each ray may be considered as having four opposing sides or edges, two of which “with extraordinary refraction”, opposite to each other, that refract the ray extraordinarily whenever the beam is turned towards the side of the extraordinary refraction; the other two “with ordinary refraction”, even if towards the zone of extraordinary refraction, emit a ray able to be refracted in an ordinary way.

Huygens had made an impeccable construction of the extraordinary ray in double refraction (§ 6.24) but had not been able to explain the modification of the light issuing from a spato. In Question XXVIII, Newton

[263] *Ibid.,* p. 269-70.
[264] *Ibid.*, p. 288.

highlights the insufficiency of Huygen's concept and adds that the phenomenon is inexplicable in wave theory. It may therefore reasonably be supposed that it was not an aversion to wave theory and the insufficient explanation to lead Newton to introduce, in a false rule, the concept of "laterality" or, as Heinrich Magnus will put it, the polarisation of light: a concept that would not be further analysed neither by Newton nor by physicists of the 18th century.

6.21 Corpuscular theory

In the early years of his scientific career, Newton seemed inclined towards a wave theory of light. In a reply of 1672 to the criticisms of Hooke, he writes: "The broadest vibrations of the ether impart a sensation of the colour red, the smallest and shortest dark violet, intermediate ones an intermediate colour".[265] According to this expert polemical reply, the vibrations in the ether caused by the collisions of luminous particles against refracting or reflecting surfaces strike the eye and produce the sensation of colour. In this way, Newton adds, the phenomena of the colouring of thin layers observed by Hooke may be explained. In sum, light is always granular, but the pulsations of the ether have a fundamental place in visual perception.

But the following meditations, instead of bringing him closer to the idea of vibration, took him further away up to the point that he was one of the firmest opponents of wave theory. The reasoning is well known, as Newton himself clearly sets it out in question XXVIII. The basic problem is the incompatibility, in his opinion, between rectilinear and wave motion. "If light consisted of pressure or motion induced through a fluid, instantaneous or lasting over time, it shall be bent in shadow":[266] just as the waves of the sea break against obstacles and the chiming of a bell is distorted behind a hill. "But never has light been seen to follow twisted routes or be inflected in the shadow."[267]

Newton has often been accused of not having understood that Grimaldi's experiments in fact demonstrated the inflection of light behind obstacles, but it is forgotten to remember that at the time neither Hooke nor Huygens, the advocates of wave theory, understood that the diffraction is a phenomenon of the inflection of light. With hindsight, criticism of the

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[265] *Isaac Newton's Answer to Some Considerations upon His Doctrine of Light and Colours,* in "Philosophical Transactions", 7, 1672, p. 5088
[266] Newton, *Optics,* op. cit., p. 291
[267] *Ibid.,* p. 292

scientists' work is far too easy, even if there is some pleasure to be enjoyed in correcting brilliant men!

Really, it is not necessary to come to question XXIX, that is almost the end of the volume, to understand Newton's hypothesis of the nature of light: it is clear from the first pages of the treatise, as we have already mentioned (§ 6.15), and is present throughout the work, despite Newton's insistent claims that it was based on experiments only and his denial of every hypothesis. Consequently, the interrogative form used in the fundamental proposition of question XXIX, that expounds the theory of light is, in fact, rhetorical: "Are not rays of light perhaps tiny corpuscles propelled by luminous bodies?".[268] The scientist continues by saying that this hypothesis explains the rectilinear propagation of light, reflection, refraction and total reflection. A simple explanation of refrangibility can be found by supposing the corpuscles to be of different thicknesses: the largest are more difficult to deflect from their rectilinear path and when they strike the bottom of the eye, they give the sensation of red; the smaller ones are more easily deviated and when they strike the bottom of the eye, they give the sensation of violet.

Explanation was still needed for the commonest and most difficult question: the contemporary reflection and refraction of light on the surfaces separating two media. Newton goes back to the theory of "fits". But what is the root of fits? Newton explains that an admission that luminous corpuscles "due to their attractive power or because of another force", excite vibrations in the medium that provoke fits through the confused mechanism already described (§ 6.19). The double refraction of Iceland spato can finally be explained, as previously noted in the text, by the existence in the luminous corpuscles and the spato of *certa latera.*

To sum up, Newton's theory of light is complex and complicated, often obscure and contradictory, corpuscular and undulatory at the same time. If it were ever true that Newton never put forward hypotheses in mechanics, in optics he made up for ten doubles. But maybe obscurity and repentance hide the great intuition: the double nature of light. Barrow, Newton's tutor, had already wisely suggested, with the sole scope of clarity and with no pretence of describing reality, to interpret some optical phenomena by supposing them to be caused by the issue of particles and others by continuous impulses. Newton's unease would be repeated, in 1923, with Louis De Broglie, who found insufficient both the photonic theory and the electromagnetic theory of light - versions of the XX century related to the corpuscular and wave theory - thus he hoped to synthetise in wave mechanics both the corpuscular and the wave aspects. Might it be that the

[268] *Ibid.,* p. 298

brilliant ideas of De Broglie had been influenced by the authentic Newtonian tradition?

A tradition that, in truth, was altered by 18th-century physicists who replaced it with a stylization that maintained the fundamental concept: light is constituted of minute corpuscles, released by the source at great speed in all directions; they move in a straight line at an increasing speed according the density of the medium because the density increases the attraction of the matter on the corpuscles. Having expounded the basic theory, the other optical phenomena that had so bothered Newton are described, without explaining them. This attitude of 18th-century physicists is of no great service to Newton, nor to the science of optics.

6.22 The "Questions"

The first and sole part of the third book closes with 31 questions, or rather "Queries", that "serve - Coste says, interpolating in the English and Latin texts - as a conclusion to the entire work". The interpolation is infelicitous as the questions, far from being a conclusion, are a prologue, a stimulus to take the research even further.

Newton asks questions that do not concern only optics, but also other phenomena, in particular gravitation and the composition of matter. It is rough material that was emended, reordered and enlarged in successive editions. The first edition contained 16 questions, the Latin version twenty-three; the second and third English edition thirty-one. Sometimes the new questions contradict the preceding ones, but Newton, ignoring the incoherence, published both the new and old questions almost as if he wished to leave a history of his intellectual torments and indicate possible alternative directions for research in physics.

There follows a short description of this part of the work. The first three questions, in the interrogative form like all the others, re-exposing the concepts already set out in the *Principia* (Book I, proposition XCVI), frankly propose an attraction of bodies on light, that increases as the distance decreases and as the refrangibility increases. In addition, in passing close to the extremities of the bodies, the rays of light are bent several times so that they take on a sinuous movement similar to eels. These are hypotheses of convenience that fit very well the interpretation of diffraction. The attraction of luminous rays by the bodies, foreseen in Einstein's theory of relativity and proved in 1919 and after, has little relation to Newtonian attraction, because in the experiments of Grimaldi and Newton the mass of the diffracting bodies has no influence on the phenomenon.

The fourth question, in line with the results set out in the *Principia,* asks whether the reflection or refraction of light rays does not begin before the rays hit the surfaces of the reflecting and refracting bodies.

Questions V-XI make up Newton's theory of the relation between light and heat: bodies act on light and light acts on bodies, causing vibration of particles and therefore heating them, because "heat consists of a vibration of the particles in a body".[269] Dark bodies are heated more by light as this, not being sent back, is reflected and refracted several times in the body until it is extinguished. The bodies, heated above a certain temperature, emit light, produced by the vibrations of the body parts. The agitation of the particles, however it is caused (by heat, friction, percussion, putrefaction or any other cause) produces light. Newton provides a number of physical and chemical examples, among the first of which are the experiments on "phosphoric light" and those of Hauksbee with the rotating glass globe. The conclusion is that fire is also a body heated to the point of producing light and the flame is a "vapour" heated to the point of becoming bright. Question XI expounds an interesting theory of self-heating of very large bodies: heating a body beyond a certain limit, the light emitted is so great that, reflecting and refracting in the body itself, increases its temperature until it equals that of the Sun. The theory explains the constant emission of heat by the Sun and the fixed stars, attributed not only to the dimensions of the bodies but also to the cited interaction between light and matter.

Questions XII-XVI constitute a theory of sight based on the fundamental idea that "light rays hitting the back of the eye excite vibrations in the retina that spreading along the solid fibres of the optic nerves, reach the brain and then excite the sense of sight".[270] The breadths of the vibrations vary according to the different types of rays: the shortest, provided by the more refrangible rays, excite the sight of violet; the longest, provided by more refrangible rays, provoke the sight of red. The vibrations of the retina, that last a second (by which Newton explains the persistence of the images), may be excited also mechanically, such as by the pressure of a finger or a punch in the eye. We note that the theory differs from that of 1672: then (§ 6.21), it was about vibrations of the ether, this time vibrations of the fibres of the optic nerve.

Questions XVII-XXIV are dedicated to the vibrations of a hypothetical ether diffused throughout the universe that penetrates bodies and has an immense elastic force (700,000 times greater than the elasticity of air) and a density 700,000 times lesser, that increases with its distance from the Sun. This elastic force of ether may be sufficient to push bodies from the heavier

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[269] *Ibid.,* p. 271
[270] *Ibid.,* p. 276

parts to the lighter parts with a force that we call gravity: these are not the Cartesian vortexes, but we are not far off! The vibrations of the ether - and Newton comes back to the theory of 1672 - excite in the optic and acoustic nerves, respectively, perceptions of sight and hearing; furthermore, the vibrations of the ether provoked in the brain by the force of will might be responsible for the animal muscular response.

But this attempt at a single, unitary explanation, that could interpret both optical and gravitational phenomena, is immediately called into doubt in questions XXV and XXVI, dedicated to double refraction (§ 6.20), and even more so in questions XXVII-XXVIII, in which Newton piles up arguments against the existence of ether, even invoking - a rare event in his writings - the authority of ancient Greek and Phoenician philosophers. In our opinion, this strident contradiction demonstrates the scientist's perplexity and his desire to leave the problem open for future investigation.

Question XXIX is dedicated to the corpuscular theory of light (§ 6.21) and XXX asks why a reciprocal transformation between light and matter might not occur. The possibility is suggested by the fact that a sufficiently heated body emits light and the light, in turn, is arrested when it meets the bodies and identifies with them. On the other hand, the reciprocal transformation is congenial with nature "that seems to enjoy the transformations", as demonstrated by the large number of physical changes and animal metamorphoses adopted by the scientist to confirms his assertion. And, so, "among all these different and strange transformations, why does nature not change bodies into light and light into bodies?"[271] It would be wrong to align these views to the modern relativistic theory of equivalence between matter and energy. Newton's thinking substantially remains based in tradition: for the atomists, both ancient and modern, atoms of light are different to atoms of matter only because of their size, and even Antoine-Laurent Lavoisier will number light among the chemical elements, while his own theory of combustion will replace the doctrine, professed by 18th-century chemists of the influence of light on chemical reactions, directly taken from Newton.

6.23 Microcosm, cosmogony and methodology

The last question, number XXXI, is almost as big as all the others put together, and it is certainly the most important. In the first part, Newton basically asks if the law of gravity, valid for the macrocosm, could not also be analogous in the microcosm. As we have already seen (§ 6.1), from his

[271] *Ibid.,* p. 303

early years Newton was interested in studies and experiments in chemistry, that he continued for virtually all his life. The catalogue of his library lists hundreds of books in chemistry and alchemy and there exists at least a thousand manuscript pages dealing with chemistry, unfortunately only partly published or even studied.

His great interest in, and the great amount of time spent on chemical experiments, led him to ask fundamental theoretical questions: how can we explain the compactness of solids, their cohesion and adhesion, solutions, chemical reactions? Many phenomena suggest the granular make up of matter, with "solid, massive, hard, impenetrable" particles that "never wear out or break".[272] These particles may be held together by mutual attractive forces, similar to gravitational forces and not yet experimentally proved as they do not extend over appreciable distances. This introduces into chemistry the concept of affinity, that would continue throughout the XIX century and beyond.

This is a new type of force that Newton continually calls "attraction" to distinguish it from the force of gravity. Why some critics identified it with gravity is hard to understand, as Newton himself supposed that "attraction" follows different laws to that of the simple law of gravity. In fact, he says: "The smallest particles of matter may be united by very strong attractions and compose bigger particles whose attractive force is weaker. Many of these larger particles may join together to create larger particles with an increasingly lesser attractive force [...] and so on in a continuous series [...] until they create bodies of sensible size".[273] Who wants, can compare this theory with the modern ideas of nuclei, electronic distributions and nuclear combinations. We instead, who are not searching for precursors at all costs, believe that this comparison is not important; what is important is Newton's intuition of the complexity of the structure of a perceptible particle. According to Newton, only such a complex structure can explain hardness, elasticity, malleability and fluidity.

But as in algebra negative quantities start when positive quantities finish, in mechanics the repulsive force must occur when the attraction ceases. The repulsion seems to appear from reflection or diffraction, from the emission of light, evaporation, solutions and the elasticity of air. Inertia is a passive principle that produces no earthly movement; gravity, attraction and repulsion are, on the contrary, active agents, without which there would be no corruption, generation, vegetation, life, and planets, and comets would not remain in their orbits.

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[272] *Ibid.,* p. 325
[273] *Ibid.,* p. 320

At this point, Newton gives way to his cosmological romantic imagination, so full of mysticism. In the beginning, God created matter with particles with the described properties, thereby ensuring the constancy of nature to the end of time. The particles obey the passive laws of motion, but are also subject to certain active principles, such as gravity and cohesion and the fermentation of bodies, Newton affirms, repeating an idea expressed in the Principia: "I consider these principles not to be occult, as considered the result of the specific forms of things, but as universal laws of nature from which things are formed. Natural phenomena demonstrate that these principles truly exist, even if their causes have yet to be discovered. The qualities are certainly manifest, only the causes are occult".[274] Through the active principles, the Omniscient placed the particles in order and created all things, and it is impossible that a blind fate could create such order for inanimate things and the bodies of animals. And as God created this world, he could create others supported by different laws of nature.

In order to understand the laws of our world, as created by God, Newton advised following the method already proposed in the *Principia*: "As in mathematics, so also in physics, research of difficult things using the so-called analytic method must always precede the so-called synthetic approach. The analytic approach consists of experimentation, observation of phenomena and then deduction to arrive at general conclusions, without accepting any objection that does not derive from other experiments or from other ascertained proof. In fact, in the experimental approach, theories are to be taken as valueless (*pro nihilo sunt habendae*). And even if deducing from observations and experiments certainly does not demonstrate the general conclusions, this is still the best reasoning that nature offers for objects and the more general the induction the better it is founded. And if no exceptions arise from the phenomena, a general conclusion may be inferred. If, as a result of experimentation, there is a contradiction, then the conclusion must be set out including the exceptions. Using this type of analysis, one may pass from complex to simple things, from motions to the forces that produce them, and, in general, from effects to their causes and from particular to general causes, finally arriving at the most general causes. This, then, is the analytic method. The synthetic approach consists in assuming known and proven things as principles and use them to explain the resulting phenomena and prove these interpretations."[275]

While he followed the methods set out in the study of optics, Newton optimistically concludes that the progress of physics would broaden moral confines and would encourage the recognition and veneration of God, the

[274] *Ibid.,* p. 326
[275]*Ibid.,* pp. 328-29

only Lord. Thus closes with a metaphysical note a great treatise, drawn up without any theological concern.

The *Opticks*, a classic example of brilliant scientific popularisation, written in the national language to facilitate its diffusion, is easy and pleasurable to read. The mathematical apparatus, demonstrations and calculations are almost non-existent. While only the professional mathematicians may profit from a reading of the *Principia*, the *Opticks* could also be usefully read by an amateur as well as the scientist. The *Queries*, too, despite their contradictions, and perhaps even because of the contradictions, astounded and interested learned and uneducated men. The popular character of the work was the cause of its wide diffusion in Europe and contributed to the solid foundation of the new concepts and to the growth of the scientist's standing. The *Opticks* remain a milestone in the history of physics.

6.24 Huygens' wave theory

In the short preface to his *Traité de la lumière* (Leyde 1690), Huygens states that he had completed the treatise in 1678 and in the same year he informed the Académie des Sciences of Paris of the paper. He had not published it because, written in bad French, he hoped to translate it into Latin (and in fact his papers contain a translation of the opening pages) and to publish it together with the treatise of instrumental optics, but, with the failure of both ideas, he published it as it standed, so that it would not be lost.

As we have already seen (§ 6.14), Newton's fundamental memoirs had already been published, subsequently put together in the *Opticks.* Huygens thinking was influenced by them. For this reason, the chronological order of publication notwithstanding, we will now deal with Huygens optics. He, in fact, had appropriated Hooke's theory of colours (§ 5.34), after Newton's work, the experimental part of which he admired but did not agree with the theoretical interpretation, and concluded with believing that the colour phenomenon was still a mystery "due to the difficulty in explaining by mechanical physics the consistency of the different colours". He therefore judged it prudent not to deal with colours in his treatise.

The brief *Traité,* taking up only 77 pages in the complete works of Huygens, is divided into six chapters. The first deals with the propagation of light; the second with reflection; the third with refraction; the fourth with atmospheric refraction; the fifth with double refraction; the sixth with the forms of lenses.

The treatise opens with a critique of the theories of Descartes, Grimaldi and Newton: if light is made up of corpuscles, how is it possible that it moves in a rectilinear way in matter without being broken down? And how is it possible that two bands of light, that is two sets of corpuscles, cross without being disturbed by the reciprocal collisions? The simple explanation is that light is produced by fire and flame, that is by bodies in very rapid movement that, concentrated by a mirror, burn the objects, that is disintegrate them, “which certainly indicates motion, at least in proper philosophy”; that vision is obtained by exciting the terminal part of the optic nerve; that based on the laws of collision, two or more movements may cross without disturbing each other; that the propagation of sound occurs through movement: It is sufficient, states Huygens, to consider all these facts to arrive at the peremptory conclusion: “There can be no doubt that light consists of the movement of a certain matter, placed between us and the luminous body”.[276]

Well, one cannot say that Huygens’ arguments are very convincing!

But what is the means that drives motion? Huygens, repeating the similarity between sound and light, and observing that the means cannot be air because the pneumatic machine had demonstrated that, unlike sound, light propagates also in a vacuum, postulates the existence of an ethereal substance that fills the universe, penetrates bodies and is so extremely thin that it escapes any weight analysis, but it very hard and very elastic. Enough: Descartes had led the way!

With the example of this not very convincing mechanical model, Huygens again describes Marci’s experiment (§ 5.11): if, in a line of elastic spheres in contact, the first collides with another, the last will rebound. There has therefore been a propagation of motion, but not the translatory motion of the single balls. Ethereal particles are small elastic bodies (perhaps not spherical) that cause motion even without assuming translatory movement. In sum, the mechanical model representing wave movement led to the guess of longitudinal waves. Huygens starts from the example of a flame (Fig. 6.5): each point of the flame communicates motion to the surrounding ethereal particles, that is it emits a wave and each particle of the ether touched by the wave becomes the centre of another tiny wave. Movement is propagated in this way from particle to particle by secondary spherical actions, in the same way that fire spreads. What may appear strange and almost incredible is that the undulations produced by the movements and such small corpuscles may extend over such large distances such as those separating us from the stars. Huygens assures: “But if we stop

[276] Ch. Huygens, *Traité de la lumière,* Leyden 1690, then in *Ibid. Oevres completes*, op. cit., Vol. 19, p. 461.

to be amazed, considering that at a certain distance from the luminous body, an infinity of waves, even if emitted from different points of the that body, are united so that they sensibly compose a single wave that consequently has enough force to be felt".[277]

This principle of the envelopment of waves is the most brilliant concept expressed in the *Traité.* The scientist illustrates in Figure 6.6, that the reader will have seen in modern treatises of undulatory optics.

It can be seen that, in the undulatory concept, the Greek idea of the luminous ray no longer applies, and Newton's light ray also disappears.

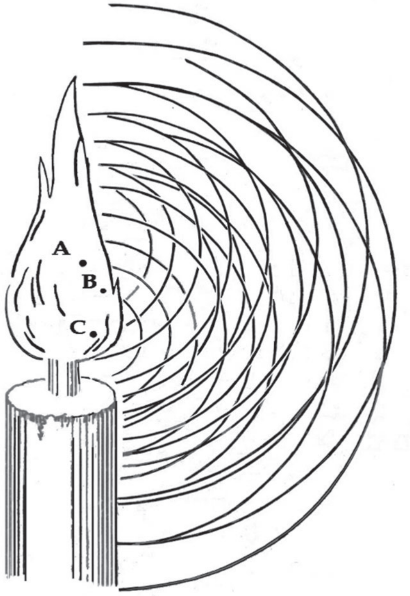

Fig. 6.5 - Example of a flame.
Source : Huygens, *Oeuvres complètes*, op. cit.

Leibniz immediately understood the importance of the new concept and wrote to Huygens on 22 June 1694: "Assuredly, Mr Hooke and Father Pardies would never have arrived at the explanation of the laws of refraction through the image they had of oscillations. It all consists in the way that you

[277] *Ibid.,* p. 475.

have considered eah point of the ray as radiating, and to compose a general wave from all these auxiliary waves".[278]

Huygens showed that his predecessors in wave theory, Hooke and Pardies, were not able to advance the theory precisely because they had not understood the principle of envelopment. As with its predecessors, the immediate intuition of the rectilinear propagation of light escapes this new concept. Huygens attempts an explanation, affirming that, behind a diaphragm, elementary waves propagate without enveloping, and therefore remain insensible; therefore, according to him, "the rays of light may be considered as straight lines":[279] an explanation that is reduced to an affirmation.

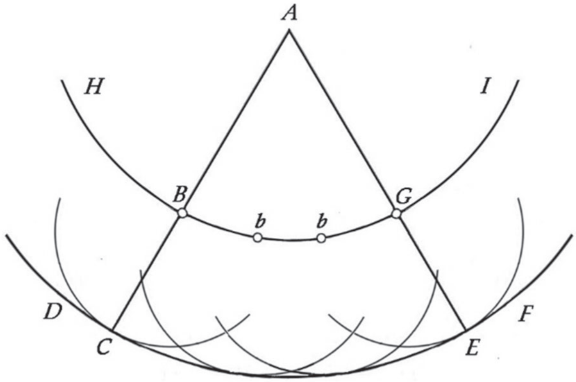

Fig. 6.6

Huygens returned to this unsuccessful explanation, clearly demonstrating with his mechanism, in the form substantially repeated in modern treatises, partial reflection, refraction and total reflection: phenomena that had constrained Newton to muddle his theory by heaping hypotheses on hypotheses.

Huygens, too, was forced to draw up new hypotheses to interpret refraction, and more generally the transparency of bodies. How can ethereal waves be propagated in solids? One may simply think that the waves of ether make vibrate the particles constituting the bodies, but it may also be assumed that the ether penetrates the bodies and occupies the interstices

[278] *Ibid.,* Vol. I0, p. 643.
[279] Huygens, *Traité de la lumière,* op. cit., p. 477.

between particle and particle. The second hypothesis stays closer to facts, as proved by Torricelli's chamber, filled with ether that has crossed the walls, as light propagates. From the second hypothesis follows that the velocity of the wave inside the bodies must be less that in the free ether, because of the small jumps of the ethereal particles on the material particles of the bodies. In sum, the speed of light diminishes with the increase of the density of the medium, as simple good sense would seem to suggest. This different velocity is the cause of the refraction and Huygens demonstrates that $n=v_1/v_2$, where v_1 and v_2 are the speed of light respectively in the first and second medium and n the index of relative refraction.

After having demonstrated that the vibratory mechanism he invented led to Fermat's principle (§ 5.33), with a demonstration much simpler than the that of the French mathematician, dealt with in the fourth chapter on atmospheric refraction, Huygens comes to the fifth chapter that was always, from Leibniz on, the most admired part of the treatise. He examines the phenomenon of double refraction (§ 5.35) that Huygens discovered to be present also in quartz. He measures with greater precision the geometric elements of Iceland spato, determining the *principal section* and the *axis* of the crystal (the terminology, that continues in science, is his). He proves that the index of the refraction of the extraordinary ray varies with the position of the plane and the value of the angle of incidence.

As there are two refracted waves in the spato and the quartz, Huygens presumes that one is produced, as in ordinary refraction, by the vibrations of the ether, and the other by the vibrations impressed by the ether on the particles that make up the body. The two waves, propagated by different means, therefore have different velocities: that corresponding to the ordinary ray is equal in every sense in the crystal and therefore gives rise to spherical enveloping waves; the one corresponding to the extraordinary ray varies in the different directions and therefore the enveloping wave is not spherical. He supposes it to be helicoid and, in this hypothesis, calculates the behaviour of the extraordinary ray in the various conditions of incidence, obtaining results admirably coherent with the experimental data: this agreement appeared to him the triumph of theory, so much so that he noted in his manuscripts the date of the discovery (6 August 1677) and Archimedes' exultant cry: "eureka"!

But Huygens was ahead of his time. Perhaps attracted by the parallel between sound and light, from where he had started, he had presumed the vibrations of ether to be longitudinal, even if Grimaldi and Hooke had already postulated transverse vibrations. This hypothesis of longitudinal vibrations does not include in the wave interpretation some modes of the phenomenon: for example, the behaviour of light rays that, issuing from a first crystal, meet a second one. Huygens discovered this deficiency of the theory and tried to minimise it by affirming that he had discovered the phenomenon after writing the treatise, while he had certainly discovered it earlier, in 1672 or 1673, and confessed he could not properly explain the new phenomenon, and he put his trust in the future: "I will add another marvellous phenomenon that I have discovered after writing the preceding pages. Although I have not yet discovered the causes, I do not wish to ignore it so that others may search for it. It would seem that other theories other than mine are needed, that would not for this cease to preserve their likelyhood, after having being confirmed by so many proofs".[280]

After this premise, Huygens describes the phenomenon and shyly mentions a new explanatory theory. The phenomenon is now well known: if one of the two rays of light emerging from an Island spato meets a second spato, it refracts or bi-refracts, according to the orientation of the second spato in respect to the first. The particularities of the phenomenon, says Huygens in ending this part of the treatise, leads to the conclusion that light waves, after having passed through the first crystal, acquire a certain form or disposition as a result of which, when they meet the structure of the second crystal in certain positions, may move the two different types of matter responsible for the two refractions, while when they meet the second crystal in another position they are able to move only one of these two different types of matter: as a representative hypothesis it is rather good. It is a shame that Huygens did not deepen the study of the nature of the particular disposition that the ray assumes issuing from the spato, and immediately abandons the argument.

He quits the subject in favour of talking about the constitution of crystals. How did they acquire the regularity and constancy of their form? And why are they so easily flaked off in certain planes? The scientist proposes the hypothesis, that will find wide support by experiments and applications only in the XX century, that regularity "derives from the orderly disposition of the small invisible equal particles that make up the crystals".

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[280] *Ibid.*, p. 517.

7. THE EIGHTEENTH CENTURY

THE SCIENTIFIC ENVIRONMENT

7.1 The spread of Newtonianism

The most serious obstacle to the spread of Newton's ideas was Cartesianism, which had taken root in the minds of many scientists, even in Britain. Locke, who despite being Newton's friend was so mathematically ill-equipped that he had to ask Huygens for his opinion on the soundness of the *Principia*, was perhaps the first Newtonian philosopher, and his battle against the Cartesian *a priori* without a doubt provided important support for the Newtonian cause. However, scientists reproached Newton not so much for his empiricism, which was tempered by a much broader context of methods (§ 6.23), but for reintroducing occult qualities in physics. Roger Cotes, in the second edition of *Principia*, after reaffirming the physical and not metaphysical nature of the principle of attraction and criticising the theory of vortices as mechanically impossible, responded to the Cartesian qualms with an accusation of impiety. This reaction did more harm than good: the continental Cartesian and Leibnizian circles hardened in their position. The physics text by Jacques Rohault, translated into both English and latin, continued to be used in French and British schools: only in 1723 did Samuel Clarke add critical annotations inspired by the Newtonian doctrine.

Newtonianism established its first bridgehead on the European continent (if our extended military metaphor can be permitted) in the Netherlands: Willem Jacob s'Gravesande and Pieter van Musschenbroek were its first heralds. Their influence was particularly significant in French culture, which had been firmly entrenched in Cartesianism for the first decades of the century under the direction of the eloquent Bernard Le Bovier de Fontenelle (1657-1757), who was far from simply a slavish interpreter of the Dèscarte's thought. Maupertuis, who in his *Discours sur les différentes figures des astres* (1732), claimed to be the first scientist in France to dare to at least examine the principle of attraction, later confessed to have attracted the enmity of many of his compatriots.

Cartesianism still dominated in the Académie des sciences of Paris when, in 1734, François-Marie Arouet de Voltaire (1694-1778), in his *Lettres* *philosophiques*, declared himself a Newtonian. In 1738 he published *Elements de la philosophie de Newton*, a work aimed at the general public, marking the beginning of the Newtonian counteroffensive in the learned circles of Europe. Once it had taken hold, the rise of this new scientific philosophy was rapid: by the time Voltaire wrote a preface to the French edition of *Principia* (1756), translated by his friend the marquise Emilie du Châtelet (in collaboration with Alexis-Claude Clairaut), Newtonianism had been accepted by all scientists in Europe save a few insignificant exceptions.

Even in Italy, where Cartesianism had found a curious *modus vivendi* with the more rooted Galilean tradition, the introduction of Newton's ideas was slow and hard-fought. The Italian translation of Pemberton's *A View of Sir Isaac Newton's Philosophy* (published in Venice in 1733) was perhaps the first serious exposition of Newton's work in the peninsula. The name of the translator was omitted and a twice-repeated "warning" informed the reader: "In this work we do not suppose that the Earth is in motion, which here is taken as a principle to facilitate explanation of natural phenomena and for greater logical coherence with the other parts of this work, or the contrary hypothesis. Therefore, wise readers shall be able to judge what they find here only relatively, and not in an absolute sense because of the nature of this issue." The absence of the translator's name and the "warning" reveal the additional obstacle that the spread of Newtonian ideas faced in Italy: the Inquisition was still feared or at least brought about a general prudence.

Francesco Maria Zanotti (1691-1777) of Bologna is typically listed as one of the first Newtonians in Italy. However, the efficacy of his work is questionable because of his record as a Cartesian, the suspicion of his contemporaries that his aim, more than to defend Newtonianism, was to ridicule it, and his recourses to Platonism. A more significant work was that of his student and friend Francesco Algarotti (1712-1764), who in 1737 published *Il newtonianismo per le dame* (Newtonianism for ladies): the numerous editions of the work and the translations that immediately followed into French, Russian, English, German, and Portuguese evidence the great success of the work, whose stated aim was to "tame Newtonianism, and agreeable render its austerity."[281]

It was indeed a popular work, in which the author relates six imaginary dialogues with the marquise of E. regarding Newtonian doctrines juxtaposed with Cartesian ones, which in the end are sufficient to convert the docile marquise from Cartesianism to Newtonianism with "no small advantage in truth."

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[281] F. Algarotti, *Il newtonianismo per le dame, ovvero dialoghi sopra la luce, i colori e l'attrazione,* Naples 1739, dedication to Fontanelle, p. XIX n.n.

The cited Naples edition the work ends with a letter, signed N.N., in which the optical theory of Giovanni Rizzetti (died in Treviso in 1751) is refuted. In those years, Rizzetti had gained underserved fame in Europe for a series of anti-Newtonian papers on light, beginning with a 1726 note that appeared in "Acta eruditorum". John Desaguilers challenged Rizzetti's first theory by repeating Newtonian experiments in front of the Royal Society of London; the anonymous letter (perhaps written by Algarotti himself) confuted it with a well-developed argument. In the fourth "dialogue", however, Algarotti attributes the origin of Rizzetti's theory, without mentioning him directly, to a nationalistic prejudice, as "there are some for which a mountain range, a sea, or a river that lies between them and the truth presents an insurmountable difficulty." The marquise responds that it is senseless to view a scientific truth as foreign. And Algarotti: "You are, I responded, Oh Madame, a citizen of the world."[282] The internationalism of science and in general an Enlightenment-era cosmopolitanism are recurring themes in the work of Algarotti.

At the same time as in Italy, Newtonian ideas entered Germany, principally through the renovated Academy of sciences of Berlin, while in Russia their arrival had occurred a good deal earlier due to the work of Javov Bruce or Brius (1670-1753), a Muskovite of Scottish origins who in 1698 accompanied Peter the Great on his trip to England. Bruce, a military man with deep scientific interests, disclosed the Copernican theory in Russia, translated many western scientific books, and had a large part in founding the Saint Petersburg Academy of sciences. Moreover, he left a vast library that included all of Newton's important scientific writings, as well as works glossing upon the primary sources and works written by his students and friends: this was the nucleus from which Newtonianism radiated through the then nascent Russian school of science.

7.2 *Scientific dissemination*

The work of Algarotti discussed in the previous section fit into a larger European movement of scientific dissemination, which on one hand was the natural extension of the scientific journalism that had arisen in the previous century (§ 5.28), and on the other hand was made necessary by the difficulty of Newton's treatise on mechanics, which was accessible only to (some) mathematicians. The father of this literature of scientific dissemination, which was not very successful in Italy, but thrives to this day in France and English-speaking countries, was Fontenelle, a man of letters who was

[282] *Ibid.*, p. 173.

elected secretary of the Académie des sciences of Paris and succeeded in remaining in this position for 42 years (1699-1741). He debuted in 1686 with his *Entretiens sur la pluralité des mondes*, a popular account of Cartesian theory. Yet it was not only men of letters like Fontenelle, Algarotti, and Voltaire who were active in this literary genre, scientists by profession also took part: Pierre-Louis Moreau de Maupertuis (1698-1759) wrote *Vénus physique* (1742) and *Lettre sur le progrès des sciences* (1752); Euler published three volumes (1768-72) of *Lettres à une princesse d'Alemagne*; the marquise du Châtelet wrote *Institutions de physique* (1746); Joseph Aignan Sigaud de la Fond (1730-1810) wrote *Dictionnaire des merveilles de la nature* (1781, 2 volumes); the Abbé Jean-Antoine Nollet (1700-1770) wrote the popular *Leçons de physique expérimentale* (1754, 6 volumes); and Joseph-Jerôme de Lalande (1732-1807) wrote *Astronomie des dames* (1785).

The list is long enough that we can conclude that the literary genre was born in the milieu of the French Enlightenment, which in the 18th century became the main propeller of scientific progress.

In parallel to the blossoming of scientific dissemination, and partially as one of its consequences, there was an explosion in enthusiasm for public experimentation, which from the classic physics laboratories of Britain (later imitated in the Netherlands and France) spilled over into courts, salons, and town squares, where performers brought merriment, learning, and at times trickery. Popular science and public experimentation do not directly contribute to scientific progress, but they attract talented youths to science and put pressure on public governance to increase funding for scientific research. In any case, extensively equipped scientific laboratories arose in Britain through the work of Desaguliers, John Keil, and Hauksbee; in the Netherlands due to s'Gravesande with the help of his collaborator and friend Johannes van Zusschenbroek, brother to the more famous physicist Pieter; in France because of Abbé Jean-Antoine Nollet, famous in the eighteenth century for his predilection for theatrics; in America through the work of Benjamin Franklin, in Italy because of the work of Giambattista Beccaria, Felice Fontana, and Alessandro Volta. With the scientific laboratory, a new industry arose: the construction of scientific instruments, which initially did little to help the modest budgets available to scientists, who nevertheless were beginning to be able to live off of their work. The methods of scientific instruction were revolutionized: speaking was replaced, or better, accompanied, by doing.

In short, the period was characterized by the entry of science into popular culture and thus social life. This new cultural atmosphere is captured by the title of the encyclopaedia edited under the direction of

Diderot and d'Alembert, *Encyclopédie ou dictionnaire raisonné des sciences, des arts et des métiers*, which first appeared in Paris in 1751.

Scientific work in the eighteenth century was primarily concerned with assimilating existing knowledge, systematization, and criticism. With the organization of physics laboratories came an improvement in the construction of instruments, prior experimental results were subjected to further tests, and the ideas of the previous century were spread. Compared to the seventeenth century, it was certainly a less exciting period; no new big ideas surfaced (except at the end of the century with electrology) and no eightenth century scientist is comparable in fame to Galileo, Huygens, or Newton.

However, the ideas of the great scientists of the seventeenth century were like isolated summits. It was the work of the eighteenth century to connect these scientific peaks with a continuous and organized framework obtained through the systematic application of mathematical analysis in the study of physical phenomena: this was the primary contribution made by the century to the advancement of science.

One could concisely summarize the progress in physics in the 18th century as follows: Newtonian mechanics was reaffirmed and transformed in the course of the century from geometric to analytic; along with and, in a certain sense, as a consequence of celestial mechanics, the discipline of mathematical physics arose; thermometry was improved and calorimetry was invented; Newtonian optics was accepted without substantial change; and in the second half of a century a new science arose: electrology.

MECHANICS

7.3 Living forces once again

The controversy surrounding living forces (*vis viva* in Latin, § 5.14, ekinetic energy in modern terminology) was lifted from the metaphysical fog in which it had remained for the entirety of the existence of Leibniz by Giovanni Poleni, a Venetian mathematician and hydraulic engineer. Poleni restated the problem in experimental terms, inspired by the practical need to determine the effects produced by the collision of river water with the banks surrounding an offshoot canal. To delimit the area of study, he began with a definition: "If a body in motion acts on another and, in acting, exhausts all of its motion, the effect produced by the acting body shall be called an 'entire effect' of the moved body. And the cause of the entire effect shall be called the 'living force'."[283] Because of the ancient maxim revived

[283] G. Polemi, *De castellis per quae derivantur fluviorum aquae*, Patavii 1718, p. 46.

by Leibniz, *causa aequat effectum* (the cause equals the effect), Polemi considered equal the living forces of two bodies of the same external diameter if when falling onto tallow cakes, they create identical hollows. The experiments, which he repeated many times and with great care, allowed him to conclude that living forces are the product of the masses and the squares of the velocities[284], confirming Leibniz's theory.

Poleni's experiments, which, though not original (§ 5.14), were experimentally well-conducted, were studied and repeated by 18th century physicists for several decades. Indeed, Willem Jacob s'Gravesande (1688-1742), to repeat them with more ease and accuracy, devised and built a special machine that he described in his *Physices elementa mathematicae*, published in two volumes in 1720-21. With the help of the machine, s'Gravesande confirmed that the indenture produced by different weights falling onto soft clay were equal when the heights of the fall were inversely proportional to the respective masses.

There was no scarcity of criticisms raised by Cartesians and Newtonians, partly also due to the fame that Poleni had acquired among scholars, which had led to him succeeding Nikolaus Bernoulli (1695-1726) in 1719 as the mathematics professor at the University of Padova.

Particularly interesting was the Pemberton's position, as his criticism was held in high regard by Newton and John Teoph Desaguiliers (1683-1744) because his intervention demonstrated the lack of clarity of the object of dispute. Pemberton held that Poleni's experiments showed "the great unreasonableness, if not the absolute Absurdity" of Leibniz's opinion because supposing the resistance of the tallow to be constant, it follows from Newton's laws that the two (different) incident bodies dig identical indentures in different times, and therefore their forces are not equal.[285] This confutation earned Newton's respect, who consequently wanted to collaborate with Pemberton for the third edition of *Principia*. Desaguilers replied[286] with two other experiments, of which one had nothing to do with living force, as Poleni objected,[287] and the other (number of sheets of paper placed in parallel and apart from each other are punctured by a falling ball) was not very accurate, demonstrating that Desaguilers did not have a clear idea of what his experiment should have proved. Indeed, he wrote "if I have understood correctly, they [supporters of Leibniz] call *vis viva* a force whose

[284] *Ibid.*, pp. 57-58.
[285] "Philosophical Transactions", 1722-23, pp. 57-58.
[286] "Philosophical Transactions", 1723, pp. 285-90.
[287] G. Poleni, *Epistolarum mathematicarum fasciculus*, Paravii 1729, with no numbering of pages. The passage can be found in paragraph 38 of the letter written to the Abbé Antonium Co. de Comitibus.

effect is sensible, like the force of gravity when it accelerates bodies in their fall." Clearly, he had not understood correctly.

In 1724, the Académie des sciences of Paris organized a competition whose theme was the collision of perfectly hard bodies. Johann I Bernoulli (1667-1748) participated with his *Discours sur les loix de la communication du mouvement*,[288] published in 1727, which earned the praise of the Académie but not the prize, which was given to a certain Father Jean-Simon Mazières (1679-1761). Bernoulli began the paper discussing the concept of hardness, on which his ideas were not entirely clear. The existence of perfectly hard bodies whose parts, according to the definition in use at the time, cannot be separated by any force "is absolutely impossible and cannot coexist with the law of continuity", which is a general law of nature. Indeed, two such equal bodies that collide at their centres with equal and opposite velocities would come to a sudden halt, passing "immediately from movement to rest, from being to not being, which is prohibited by the law of continuity." Ultimately, Bernoulli held that bodies are all more or less elastic and in an addendum to the *Discours* he interpreted this general property of matter as a necessary consequence of the circulation of the thin matter, of Cartesian memory. If perfectly hard bodies do not exist, Bernoulli followed with an argument that predates the one put forth by the neopositivists by two centuries, the Cartesian claim to measure force from momentum is meaningless, as this rule is based on the physically unrealisable assumption that two perfectly hard bodies colliding centrally with equal and opposite momenta come to an immediate stop.

As early as 1694, Bernoulli began an epistolary exchange with Leibniz, who later became his teacher and friend, but on the issue of living forces Bernoulli did not subscribe to his ideas, viewing Leibniz's evidence as indirect and not always persuasive. However, after much thought he was convinced by Leibniz's argument and supported his position for the rest of his life.

In *Discours*, after recalling Poleni's experiment without citing him by name, Bernoulli also compared the forces and the effects they produce, which for him are the velocities acquired by the bodies subjected to the forces under consideration. To make the comparison, the scientist considered a series of identical and equally compressed springs attached end to end. The first spring in the series is fixed to a fixed object, while the last spring acts on the object being examined. Now, four of these springs, stretching, act on a body Q with four times the force that a single spring would produce on an identical body P by an equal stretching, because the force of each spring must cause its effect and no part of the force can be

[288] J. Bernoulli, *Opera omnia*, Lausannae et Genevae 1742, Vol. 3, pp. 1-107.

lost. However, the final velocities imparted to the bodies Q and P are in a 2:1 ratio, therefore the forces must be proportional to the squares of the velocities. Even a few specious considerations on oblique collisions allowed Bernoulli to fortify Leibniz's framework. In reality, the arguments contained in *Discours* were such that they stirred controversy more than resolved it, and therefore the appearance of Bernoulli's article fuelled the atmosphere of debate and transformed it into a "civil war", as Jean Montcucla called it.[289] The leading scientists of the time took part: Joseph Liouville, De Mairan, Sterling, Clarke, Jurin, and Maclaurin in favour of Descartes, while Johann I Bernoulli and his son Daniel, Hermann, Bilfinger, Wolff, Richter, s'Gravesande, Musschenbroek, and Madame du Châtelet (though curiously not her friend Voltaire) were in favour of Leibniz.

In a paper read to the Académie of Paris in 1728, Joan-Jacques De Mairan (1678-1771) showed that in the course of the controversy, every example given of a collision obeyed the law of conservation of momentum, as long as momentum is treated as a vector (as it is), but for the same examples the mechanicists also admitted the conservation of living forces. The curious pattern across the fifty years of debate was the following: when faced with a concrete problem, all scientists reached the same conclusions, even if they belonged to opposite camps (Cartesian or Leibnizian). Perhaps it was this observation that motivated Jean-Baptiste Le Rond d'Alembert (1717-1783), in the preliminary discussion to his *Traité de dynamique* (1743), to revisit De Mairan's considerations and conclude that the entire controversy had simply been a question of language.

7.4 The Italian phase of the controversy

In our view, however, it was not merely a question of language and words, as can be seen from the Italian phase of the controversy that is often ignored by historians, which we recall here. In the interest of brevity, we will leave out the minor actors in this clash and, simplifying, posit that the controversy was polarized between the Padova school and the Bologna school. The former followed Poleni and included Jacopo Riccati (1676-1754) and his son Vincenzo (1707-1775), who, however, counterintuitively taught at the University of Bologna; belonging to the latter school were Francesco Maria Zanotti, his nephew Eustachio (1709-1782) and the Manfredi brothers: Eustachio (1676-1730), Gabriele (1681-1761), and Eraclito (1682-1759). The Padova school championed Leibnizian theory, though more under the influence of Bernoulli than Leibniz himself, while

[289] J. Montucla, *Histoire des mathématiques*, Paris 1758, Vol. 2, p. 464; the historical exposition, however, can be found in Vol. 3, Paris 1801, pp. 629-43.

the Bologna school was Cartesian. A unique position was taken by Ruggero Boscovich, as we will discuss shortly.

The work of Vincenzo Riccati,[290] which in structure and even in style of writing emulates the dialogues of Galileo (as can be seen from the title), is the most complete representation of the Padova school, which had already clashed with the Bolognesi in the *Commentarii* of the Accademia di Bologna, where Zanotti acted as secretary. With his work, Riccati sought to bring the debate out of the restricted academic environment and to the larger public. Zanotti replied in another volume,[291] which he wrote in the form of an indirect dialogue, using a deliberately literary style to contrast with Riccati's, which he considered coarse. A full analysis of the two volumes would require us to recount all of the minute details of the history of the disagreement and repeat the arguments and misunderstandings of both sides; we will therefore limit ourselves to identifying the salient points of the works and highlighting a few original observations made therein.

The structure of Riccati's work is decidedly physical-mathematical, with a few notable technical results that we will discuss in the next paragraph. On the other hand, in writing his book, Zanotti endeavoured (as the subtitle states) "as much as [he] could to advance the question with solely metaphysical discourse, only assuming from geometry and mechanics the most common and well-known propositions." Moreover, the book, which ends with an ode to metaphysics, "light of intellect, endless supply of reason, divine and celestial teacher of all things,"[292] does not conceal a certain disregard for mathematics and mathematicians, who according to him "would have it that, when talking about their science, one must always speak standing up and holding his hat," as he wrote in the introduction.[293]

Riccati's work introduced an important new definition of living force, on which the rest of the work is based. According to Riccati, the living force is "the ability, belonging to the force of inertia, to exert a continuous and successive action, or rather reaction against the action of any dead force working to change the state of the body, until to the state of rest this be brought."[294] More precisely, this relationship between living force and dead force can be encapsulated in the formula

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[290] Vincenzo Riccati, *Dialogo di Vincenzo Riccati della Compagnia di Gesù dove ne' congressi di più giornate delle forze vive e dell'azione delle forze morte si tien discorso*, Bologna 1749.
[291] F. M. Zanotti, *Della forza de' corpi che chiamano viva*, Bologna 1752.
[292] *Ibid.*, p. 309.
[293] *Ibid.*, p. X.
[294] Riccati, *Dialogo* cit. p. 26.

$$r\,ds = -\,m\,u\,dt$$

where r is the constant resistance with which the tallow acts on a body of mass m and incident velocity u (in experiments similar to those of Poleni [§ 7.3]), and ds is the infinitesimal displacement of the body in the time dt. It is perhaps unnecessary to add that the formula above translates, in differential terms, to the theorem of living forces (kinetic energy theorem) in a more explicit form than the one employed by Leibniz and almost identical to the modern formulation due to Lagrange (§ 5.14).

For Zanotti, on the other hand, the term living force meant "a power or force, or quality, or virtue, or however one may wish to call it, that produces movement in bodies."[295] It is evident that Zanotti referred to Newtonian and not Leibnizian force: the conventional error thus persisted.

The methodologies of the two scientists were radically different, if not completely opposite. According to Riccati, "to resolve a physical question there is no better means than experiment." In this specific case, however, "one must have a precise grasp of the effect that the living force produces, one must first be sure that all of the force is expended, consumed, and used in the production of this effect, which itself must be measured."[296] Yet Zanotti observed that scientists did not have a clear idea of the effect because from one cause "many properties, modalities, qualities, relations, and affections… follow, which the simplest scholars sometimes take as effects, where they need not be called nor are such things."[297] Only the philosopher has "the most perceptive understanding", which lacks in the experimenter, "who uses little reason, only using eyes and hands."[298]

The discord was extreme on another point as well: the continuity of natural laws. Riccati, like all modern mathematicians starting from Cardano, was obsessed with the concept of geometric continuity, extended into the idea of continuity of physical laws, a cornerstone of Leibniz's scientific philosophy (§ 5.14). Zanotti went on the offensive against this metaphysical prejudice, using arguments that were later improved by the mathematical physicists of the 19th century. Continuity, Zanotti said in substance, exists only in the minds of geometers, who find continuity in all curves for the simple reason that they only study continuous curves; Bernoulli's reasoning for the continuity of physical laws does not hold, and is famous only because the man who wrote it is famous.[299]

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[295] Zanotti, *Della forza de' corpi* cit., p. 17.
[296] Riccati, *Dialogo* cit., pp. 47, 54.
[297] Zanotti, *Della forza de' corpi* cit., p. 15.
[298] *Ibid.*, p. 25.
[299] *Ibid.*, pp. 215-34.

In reality, Bernoulli's argument against Cartesianism (§ 7.3) was already a sort of loophole, though justified by a deeply held conviction: much like Leibniz, his followers felt that there was something indestructible in nature, which they identified as living force. If a living force disappears, it must leave a trace either in the living force transmitted to another body or in "contusion", as Riccati called it, namely the deformation of a body, or perhaps in some other way. "Living force," wrote Riccati with a general and peremptory affirmation, "can never extinguish itself … living force never destroys living force, but when they point in opposite directions, both contribute to producing a greater contusion, breaking, or other kind of effect."[300] It was this "other" that Leibnizians could not find in many physical phenomena and seemed to be entirely absent in the collision of perfectly hard bodies. To avoid admitting that an indestructible quantity of nature can disappear without a trace, it is therefore necessary to suppose that perfectly hard bodies to not exist.

Throughout this lengthy controversy over living forces, and especially in Riccati's work, a general unease among Leibnizians is evident, as they realized that the current state of physics still did not allow for an explanation of the entire physical process of a collision. Furthermore, even d'Alembert attested to the impotence of the physics of the time when, in both the first and second edition of *Traité de dynamique*, clearly having in mind the collision of an inelastic body with a fixed obstacle, he wrote that there exits "invincible obstacles that annihilate all movement of a body, whatever it may be… and these cannot serve to find the force of the body."[301]

In our view, reaffirming the historical interpretation given by d'Alembert and repeated by later historians would be inconsistent with the facts. It is true that there were also disputes over words and terminology. However, words express ideas, and a dispute over words must necessarily always involve conceptual questions. The controversy over living forces did not end with d'Alembert, nor with Lagrange. After the publication (1788) of Lagrange's *Méchanique analytique*, Lazare-Nicolas Carnot had observed that in real collisions there is a loss of living force, and that in any machine

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[300] Riccati, *Dialogo* cit., pp. 354.

[301] J.-B. d'Alembert, *Traité de dynamique*, Paris 1743, p. XIX. The second edition, which appeared in 1758, is cited more often when discussing living forces because the *Discours préliminaire* (or *Préface* in the first edition) includes two additional pages dedicated to supporting the idea affirmed in the first edition, that the controversy over living forces was simply a dispute over words. Besides this, the presentation of the problem is identical in the two editions, save for the omission in the second edition of a passage concerning the Marquise du Châtelet, who in the meantime had died.

there is a loss of the same magnitude due to the effect of “passive forces”, such that, as Carnot wrote, “if bodies are spurred by any motive force, the sum of the living forces shall be reduced to zero.”[302] The controversy was essentially resolved in 1847 with Hermann von Helmholtz’s classic paper, *On the conservation of force* ([303]), which cemented Leibniz’s intuition into a pillar of physics: there exists in nature an indestructible quantity, energy.

7.5 From living forces to the parallelogram of forces

The parallelogram theorem for forces can be traced back to Aristotle, whose rule for the composition of motions can be extended, remaining in his mechanical framework, to the composition of forces, as Duhem pointed out. The Aristotelian procedure was more or less followed in modern times by Stevin, Descartes, Torricelli, Roberval, Wallis, and Varignon (§ 5.10), and finally reduced to its most perspicuous form in the first corollary of the law of motion in Newton’s *Principia*.

Tracing the composition of forces to the composition of motions had therefore appeared perfectly legitimate for centuries, until Johann I Bernoulli observed that it does not make sense to talk about the velocity of a point that, under the action of a collection of forces, remains still: how could velocity be introduced in a system at rest? In his view, “those that conflate the composition of forces with the composition of motions err. Indeed, force, or power, consisting only in the stress or effort that generates motion, in the case where the object on which it acts is immobile, certainly does not produced any velocity, not even of infinitesimal magnitude. Where there is perfect equilibrium there can be no motion… I do not see, therefore, how one could introduce a velocity where perfect stillness reigns.”[304]

In the two pages before the passage cited above, Bernoulli gave a novel demonstration of the composition of forces that, without relying on the composition of motions, was only based on the ability to transport a force’s point of application along its line of action, the existence of a resultant force for a system of forces, and Archimedes’ rule for the equilibrium of a lever. The demonstration, which when compared to Newton’s may appear

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[302] L.-N. Carnot, *Principes fondamentaux de l’équilibre et du mouvement*, Paris 1803, pp. 149, 246. The observations are also contained in the first edition of the work, which appeared in 1783 with the title *Essai sur les machines en général.*

[303] *§2.10 in: M. Gliozzi, A History of Physics over the Last Two Centuries.* Cambridge Scholars Publishing, in press 2022.

[304] J. Bernoulli, *Propositiones variae mechanico-dynamicae*. *De compositione et resolutione vrium*, in Id., *Opera omnia* cit., Vol. 4, p. 256.

excessively long, is certainly nice, even if Mach's criticisms are kept in mind.

With its legacy of metaphysical discussions, the controversy over living forces revived an ancient question from Galileo's time: how is it possible that the resultant of two forces at an angle is smaller than their sum? If the component forces are the cause and the resultant the effect, then the effect would result smaller than the cause, in opposition to the ancient decree *causa aequat effectum.* Physicists replied that the correct application of the metaphysical principle required equality between "full cause" and "full effect". Now, the full effect is not only the resultant because the two component forces partly oppose each other, so if this contrast effect is added to the resultant force, then the cause is equal to the effect. The argument introduced a century earlier by Johannes Marcus Marci was thus essentially repeated to justify the rule of composition of motions.[305] Yet to many it seemed that, although the justification was legitimate for composition, the reasoning was flawed when it came to the decomposition of one force into two. In this case the original force is the cause and its components the effect, but the components cannot exist without the force, implying that all the parts of the components, both those that oppose each other and those that do not oppose each other, are the effect of the single original force: therefore, the effect is greater than the cause. To overcome this philosophical predicament, which was made worse by the obfuscatory terms employed (cause, effect, force, action,…), De Mairan, followed by others, claimed that there was no contradiction in maintaining that the resultant is not the sum of its components in the same way that there is no contradiction in saying that the product of multiple factors is not equal to their sum.[306] Ruggero Boscovich overcame the obstacle with the strange affirmation that the decomposition of forces never occurs in nature and decomposition is simply an artifice used by mechanicists for illustrative purposes: "decomposition never occurs, and is only conceived of by reason; in reality only composition exists."[307]

A new metaphysical concept, no less confusing than its predecessors, was born out of the dispute: the true causes of mechanical effects are not force in and of themselves but their "actions". Yet what is the action of a force? For Cartesians it was given by the product of the intensity of the force and the duration for which it acts: in this case, however, forces decrease in composition and increase in decomposition. Georg Bernhard Bilfinger

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[305] Marci, *De proportione motus* cit., f. 38.

[306] J.-J. De Mairan, *Dissertation sur l'estimation et la mesure des forces motrices des corps*, Paris 1741, pp. 92-93. The dissertation first appeared in the "Mémoires de l'Académie royale des sciences" in 1728.

[307] R. V. Boscovich, *De viribus vivis dissertatio*, Rome 1745, p. XIX

(1693-1750) observed that if actions are measured in the Leibnizian manner with the product of the force and the space over which it is applied, then in the composition of perpendicular forces the resultant, by the Pythagorean theorem, is equal to the sum of the components, while for forces at other angles Bilfiger was forced to fall back on the considerations of contrasts between the component forces.

Riccati, though inspired by Bilfinger, tackled the problem from another point of view in a paper read (1746) to the Accademia delle scienze of Bologna. His argument is based on the following definition: "The actions [are] proportional to the forces and to the spaces through which they act."[308] Riccati thus gave the term "action" a precise meaning, using it to express the same concept that, at the same time, Johann I Bernoulli indicated with the term "energy" and that later would be called "work".[309]

For convenience, we will adopt this last term in lieu of "action" in the following discussion. Riccati considered stretched elastic cords and supposed that for each further infinitesimal lengthening (or shortening), the force exerted by the cord is constant and the elementary work performed is proportional to the force and the stretching. He further supposed that work is additive and called one force "equipollent" to one or more other forces if the work it performs is equal to the sum of the work performed by the others to move a body the same distance. From these hypotheses and the definition of action, he derived the parallelogram rule without recourse to the composition of motions. This approach naturally led the scientists to demonstrate, in two different ways and perhaps for the first time, that the work done by the resultant force is equal to the sum of the work done by its components. In the interest of brevity, we omit the other demonstrations[310]

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[308] Riccati, *Dialogo* cit., p. 211.

[309] The term "work", used in a technical sense, appeared much later. J.-V. Poncelet *Introduction à la méchanique industrielle physique ou expérimentale*, Metz-Paris 1839, p. X: the first edition is from 1829) claimed to have used it starting in 1826, and more consistently from 1827, after being verbally encouraged by Coriolis, who used the term in the lessons he gave at the École polytechnique of Paris. Poncelet added (*ibid.*, p. 15) that he uses the term "work" to indicate the quantity that was called mechanical power (Smeaton), quantity of action (Coulomb, Navier), dynamical effect (Monge, Hachette), or moment of activity (Carnot). However, the expression that was most popular with physicists had been "quantity of action", though it could be confused with the quantity introduced by Maupertuis (§ 7.10). Even Coriolis, on his part, quietly laid his claim for the invention of the term "work" (*Traité de la méchanique des corps solides et du calcul de l'effect des machines*, Paris 1844, pp. IX, 36-37: the first edition is from 1836).

[310] The other demonstrations are reported in M. Gliozzi, *Teoremi meccanici di Vincenzo Riccati*, in "Physics", 9, 1967, pp. 293-300.

to present, as an example, the most general demonstration of the theorem above. Let *AB* and *AC* be the component forces and *AD* the resultant; let *AL* be the direction of the displacement (Fig. 7.1). Project the points *B*, *C*, and *D* onto the segment *AL* to the respective points *M*, *N*, and *L*. The segments *AM*, *AN*, and *AL* will then be the respective components of *AB*, *AC*, and *AD* in the direction *AL*. The theorem will be proven if we show that,

$$AM + AN = AL$$

or, in other words, that $AM = NL$.

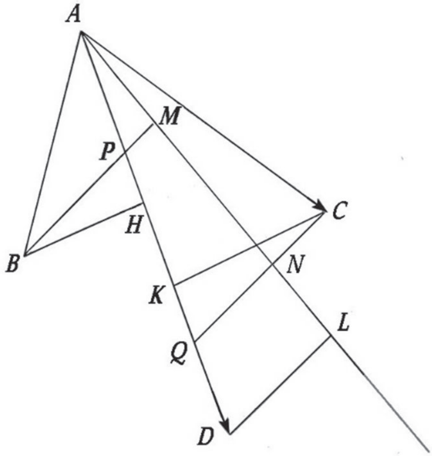

Fig. 7.1

We can draw normal segments at *B* and *C*, called *BH* and *CK*, respectively, that connect to *AD*, extend *CN* to *Q*, and call *P* the intersection of *BM* and *AD*. Now the triangles *BPH* and *CQK* are similar as they have parallel sides, and because $BH = CQ$ they must also be equal, therefore $PH = QK$. Yet $AH = DK$ and thus $AP = DQ$. Because *DL*, *CQ*, and *PM* are parallel, it follows that $AM = NL$, "therefore both the action of forces *AB* and *AC* and the action of the force *AD*, acting in the direction *AL*, will be … equal."[311]

[311] Riccati, *Dialogo* cit., p. 239.

Many years later, Lazare Carnot, following the same proof technique, the theorem to an arbitrary number of forces:[312] it does not appear, however, that he knew of Riccati's work.

7.6 *Ruggero Boscovich*

In the Italian phase of the controversy over living forces (§ 7.4), Boscovich took a unique point of view. Informed and well-researched on the terms of the disagreement, he published a 50 pages volume[313] in 1745 written in an abbreviated and sometimes difficult Latin, in which he attacked the problem completely independently of any of the older approaches. According to Boscovich, there is only one *vis activa*, devoid of physical existence – instantaneous action, which transforms force, namely the cause whose action changes the state of the body, to action itself, and consequently generates a new velocity. Much like a surface is generated by a line in continuous motion, velocity is generated by the continuously applied action of a force; and like a line without thickness is an idealization that does not physically exist, force is idealized as an instantaneous cut in time. If one considers the action of the force applied for the entire duration of the motion it causes, then its measure is given by the product of mass and velocity. If, on the other hand, the action of the force is applied for the entire path traveled, it is proportional to the square of the velocity. Ultimately, Boscovich decided in favour of the conservation of the total momentum (in an algebraic sense); in the collision of elastic bodies, though, the principle of conservation of living forces à la Leibniz also holds. However, the fundamental law of nature must be more the former than the latter principle because the first conserves the direction of forces, whereas living forces, being proportional to the square of the velocities, are always positive and cannot have opposing directions. Boscovich concluded, like d'Alembert (unknowingly to him) two years earlier, that the controversy over living forces was an issue of language.

The problem of living forces naturally also led to the question of how one body acts on another: is the action mediated, as Descartes believed, or is it at a distance, as Newton's supporters maintained? Boscovich did not hesitate: for him the transmission of action occurs at a distance. Yet the Newtonian conviction had to be modified, in particular to avoid discontinuity in natural laws. According to Boscovich, continuity is the fundamental principle of all scientific inquiry, and Newtonian mechanics does not always

[312] Carnot, *Principes fondamentaux* cit., pp. 76-77.
[313] Boscovich, *De viribus vivis dissertatio* cit. The small volume was also republished in the "Commentarii" of the Accademia of Bologna (Vol. 2, pt. 3, 1747).

adhere to it. For example, it follows from propositions LXX and LXXI in the first volume of Newton's *Principia* that the attraction of a spherical surface of radius r on a point of matter at a distance x from its centre is proportional to $1/x^2$ if $x > r$ and is zero if $x < r$; thus, there is a sudden jump, a radical discontinuity in crossing the spherical surface. Another discontinuity occurs in the collision of elastic bodies; in short, much like the aforementioned issue with the sign of living forces, the square that appears in Newton's formula prevents the force from changing sign. To avoid discontinuity in physical laws, one must modify Newton's law and also allow for repulsion between two particles a small distance apart. In this way, from the study of the controversy over living forces, Boscovich was led to his famous oscillating law of attraction and repulsion, to which we will soon return.

De viribus vivis dissertatio, a work free from any sectarian prejudice and of ample scientific merit, unfortunately did not find success among Boscovich's contemporaries. No more recognizant were histories: Montucla, Mach, and more recently Dugas – the foremost historians on the controversy over living forces – do not even mention it. Even Riccati, his fellow Jesuit, wrote it off in a few lines, accusing his demonstrations of circular reasoning;[314] for the rest of the long volume (428 pages) he basically did not speak of Boscovich.

Ruggero Boscovich was born in Ragusa (modern day Dubrovnik in Croatia) on 18 May 1711 of a Croatian father and Italian mother. His first schooling occurred at the Jesuit college in his hometown, and in 1725 he moved to Rome, where he completed his studies at the Roman Collegio and later became a professor of mathematics almost uninterruptedly until 1756. In 1757 he was sent to Vienna as a representative for the government of Lucca; he then began a life of continuous movement between France, England, the Netherlands, Turkey, and Poland. In 1764, he was given a position at the University of Pavia, where he remained, with a few interruptions, for five years. Boscovich then moved on to the Brera astronomical observatory in Milan, which he left in 1773 for Paris. Old and in poor health, he retired to Monza in 1785 and succumbed to insanity due to a cerebral arteriosclerosis that had struck him around the age of sixty; he was transferred to Milan and died on 13 February 1787.

A man of restless and irascible temperament and an indefatigable worker blessed with boundless imagination and eloquent speech, Boscovich was interested, in addition to politics and literature, in every science of his time: philosophy, mathematics, physics, archaeology, geography, and engineering. His scientific investigations always had practical aims, though as his friend and biographer Joseph-Jérôme Lefrançais observed, his work was characterized

[314] Riccati, *Dialogo* cit., pp. 177-79.

by an attempt to fuse philosophy and natural science. Today his name is mostly remembered in the context of the aforementioned attractive-repulsive law, and for this result he can be considered a forerunner of the modern physicist.

Boscovich, "convinced that nothing better had ever been invented in physics,"[315] cared deeply and spoke as often as he could about his genera substitute for Newton's attractive law of gravitation. He stated it for the first time, as we mentioned, in *De viribus vivis* in 1745, and again (without significant changes) in *De lumine* (1748), *De continuitatis lege* (1754), *De lege virium in natura existentium* (1755), and *De divisibilitate materiae & principiis corporum* (1757), and finally restated the law in its definitive form, along with all of the physical and mechanical applications that he derived from it, in his most famous work on mechanics, *Theoria philosophiae naturalis redacta ad unicam legem virium in natura existentium*, first published in Vienna in 1758 (and republished several times).

According to Boscovich, "The primary elements of matter are perfectly indivisible and non-extended points; they are so scattered in an immense vacuum that every two of them are separated from one another by a definite interval; this interval can be indefinitely increased or diminished, but can never vanish altogether."[316] Therefore, as Boscovich expressed many times, "I do not admit the idea of vacuum interspersed amongst matter, but I consider than matter is interspersed in a vacuum and floats in it." Having presupposed the law of inertia for these material points, Boscovich held that "any two points of matter are subject to a determination to approach one another at some distances, and in equal degree recede from one another at other distances. This determination I call 'force'; in the first case 'attractive', in the second case 'repulsive'; this term does not denote the mode of action, but the propensity itself, whatever its origin, of which the magnitude changes as the distances change; this is in accordance with a certain definite law, which can be represented by a geometrical curve or by an algebraic formula, and visualized in the manner customary with mechanicians… The law of forces is of this kind; the forces are repulsive at very small distances, and become indefinitely greater and greater as the distances are diminished indefinitely, in such a manner that they are capable of destroying any velocity, no matter how large it may be, with which one point may approach another, before ever the distance between them vanishes. When the distance between them is increased, they are diminished

[315] A. Fabronio, *Vitae Italorum doctrina excellentium qui saeculis XVII et XVIII floruerunt*, Pisis 1839, Vol. 14, p. 281.
[316] R. Boscovich, *Theoria philosophiae naturalis*, Venice 1763, Latin-English Edition, trans. J. M. Child, § 7: this is the (least error-prone) third edition of the work.

in such a way that at a certain distance, which is extremely small, the force becomes nothing. The as the distance is still further increased, the forces are changed to attractive forces; these at first increase, then diminish, vanish, and become repulsive forces, which in the same way first increase, then diminish, vanish, and become once more attractive; and so on, in turn, for a very great number of distances, which are all still very minute: until, finally, when we get to comparatively great distances, they begin to be continually attractive and approximately inversely proportional to the squares of the distances. This holds good as the distances are increased indefinitely to any extent."[317]

Based on the curve described in detail by Boscovich, the Newtonian law of attraction is always approximate, even at very large distances, with its accuracy increasing as the distance between the attracted bodies grows. After disappearing for a century, the "Boscovichian" curve returned to scientific circles thanks to Lord Kelvin, who used it to explain the qualitative differences of chemical substances and Joseph John Thomson, in the early years of "modern" physics, who adapted it for the study of electron orbits in his model of the atom.

Boscovich's idea was profoundly innovative compared to the traditional corpuscular theories of matter. Such theories had always conceived of matter as constituted by extensive corpuscles, atoms, or individual entities of some other name that are enveloped by space and held together in bodies by some unspecified coagulate. Traditionally, it is bodies that host phenomena; space is simply a passive and indifferent container – ever *simile* and *immobile*, as Newton had written – in which bodies were arranged.

In Boscovich's conception, on the other hand, the system of corpuscles disappears, or, more simply, is reduced to a system of points that act as centres of force. The so-called properties of a material become properties of these systems of forces, or "atmospheres of force", the plastic expression later used by Faraday. Therefore, space is no longer the passive container of Newton but the place where phenomena occur. The essence of Boscovich's entire theory could perhaps be summarized by saying that he transferred the location of phenomena from bodies to space and filled the entire universe with matter, namely force fields.

Let us quickly finish our exposition of Boscovich's magnum opus, which is divided into three parts. In the first, as we mentioned, he presented a dynamical interpretation of matter. In the second and third parts, Boscovich demonstrated how this theory could explain "all" physical and mechanical phenomena: impenetrability, expansion, collision, gravity, cohesion, hardness, density, capillarity, optical phenomena, chemical

[317] *Ibid.*, §§ 9-10.

actions, and whatever else one may dream. The work ends with addenda, two of which are concerned with space and time.

It is worth noting that doubts on the exactness and generality of Newton's law of (attractive) gravitation were also expressed by many other scientists, astronomers, and physicists. The first critics pointed to the irregularities in planetary motions, from which, according to Laplace, "one can doubt that solar gravity falls exactly in proportion to the inverse of the square of the distance."[318] Later, faith in Newton's law was shaken by the study of molecular motion, which forced scientists to introduce new types of forces that some called "non-mechanical" and wanted banned from physics. There were also other formulas proposed to replace the Newtonian one. The most famous of these attempts was the one discussed by Laplace:

$$F = k\frac{m_1 m_2}{r^2}e^{-\lambda r},$$

where λ is a constant and the other variables have their usual meaning. Yet this approach never let to acceptable results, and the Newtonian theory remained, despite recurring controversies, unopposed and mysterious until the advent of relativity.

7.7 Analytical mechanics

Newtonian mechanics, as we discussed at length (§ 6), was originally geometric in nature. Through much patient work, the 18th century gradually transformed the geometric treatment into an analytic one. This metamorphosis did not occur without opposition, not so much because of its technical difficulties, which were exacerbated by the fact that the methods of infinitesimal calculus were still not fully digested, but because of the lack of clarity in the new dynamical concepts, like mass, inertia, and force (with all the varieties defined by Newton, § 6.4), and the metaphysical concepts that accompanied them, like cause, effect, action, power, etc.

To appreciate the difficulty of this work, one only has to examine the analytic translation of the second law, which was perhaps even formulated independently of Newton's proposition: "the variation of motion [*variationem motus*] is proportional to the motive force impressed, and occurs along the straight light according to which the force has been impressed."

In 1906, in a paper on relativity, Max Planck (1858-1947) observed that it is convenient to define force in such a way that the impulse (or energy) theorem takes the simplest possible form. Following this observation, 20th century historians claimed that with the expression *variationem motus*,

[318] P.-S. Laplace, *Expositon du système du monde*, in Id. *Œuvres*.

Newton did not mean the variation in motion, but rather the variation in momentum, such that the correct symbolic translation of Newton's second law would be

$$d(m\vec{v}) = \vec{F}\,dt.$$

This purported translation is incorrect, as can be seen from the simple observation that Newton's law makes no reference to time: the Newtonian force impressed (*vis impressa*) is not impulse, nor can the variation of motion be taken as a variation of momentum, meaning that we must also reject the translation $F=m\ v$ proposed by Laplace and repeated by some modern historians.[319] If in the twentieh century translations could be so mistaken, imagine what occurred in the eighteenth!

The first mathematical formulation of the second law of motion, equivalent the ours $F=ma$, can be found in a paper by Varignon published in 1700, which curiously did not deduce it from the second law of motion but from falling bodies and thus without any reference to mass.[320] Varignon observed that if x indicates the space coordinate traversed and t indicates the time elapsed to traverse it, one has, by the definition of velocity, $v = dx/dt$. Furthermore, because the distance traveled by a body moved by a constant force, like the common example of a falling body, is the product of the force y and the square of the time elapsed, one has $d^2x = ydt^2$, that is, $y = d^2x/dt^2 = dv/dt$, without any regard for rigorous mathematical manipulations. The formulas resolve all of the issues raised in the title of the paper and immediately give proposition XXXIX in the first volume of Newton's *Principia* (Supposing a centripetal force of any kind, and granting the quadratures of curvilinear coordinates; it is required to find the velocity of a body, ascending or descending in a right line, in the several places through which it passes; as also the time in which it will arrive at any place: and vice versa).

In a later paper also published in 1700, Varignon extended the two formulas to planar curvilinear motion, and in a third paper (still from that same year) he deduced the second law of motion from the Galilean laws for freely falling bodies using the following reasoning. Let a body of mass m and weight p fall vertically from a height l in time t, and let the

[319] *Ibid.*, p. 482: "Here are, therefore, two laws of motion, the law of inertia and that of the proportionality of force and velocity, which are given by experiment."

[320] P. Varignon, *Manière générale de déterminer les forces, les vitesses, les espaces, et le temps, une seule de ces quattre choses étant donnée dans toutes sortes de mouvements rectilignes variés à discretion*, in "Histoire de l'Académie royale des sciences avec les mémoires de mathématique et de physique. Mémoires.", 1700, pp. 22-27.

corresponding Greek letters indicate the same quantities for a different body. Supposing that the weights remain constant during the motion, the laws of free fall allow one to write

$$ml\pi\tau^2 = \mu\lambda pt^2.$$

Now, it the weights remain constant only for infinitesimal time intervals *dt* and *dτ*, according to Varignon one still has

$$m\pi\, dl\, d\tau^2 = \mu p\, d\lambda\, dt^2,$$

which remains valid even if the motion involves variable acceleration because "instantaneous central forces are always uniform and constant at each instant." To Varignon, however, the first spatial differential must have seemed odd, because in substituting the weights with arbitrary forces, he changed the first-order differential into a second-order one using a segment of the associated diagram. He then wrote the previous formula in the following form:

$$m\phi d^2 l d\tau^2 = \mu f d^2\lambda dt^2$$

which is equivalent to

$$\frac{f dt^2}{m d^2 l} = \frac{\phi d\tau^2}{\mu d^2\lambda}$$

If, with an opportune choice of units, the righthand side is set to unity, one obtains

$$f = m\frac{d^2 l}{dt^2},$$

or also, ignoring the factor of *m* (which was a constant in the problems considered by Varignon), $f = d^2l/dt^2$, as he had found in his first paper.

In the form $f = m\, dv/dt$, Varignon's formula became relatively common: Jakob Hermann used it in his *Phoronomia* in 1716 and Johann Bernoulli used it in his *Discours* of 1724 (§ 7.3). Nevertheless, when stating Newton's second law, both Pemberton and Maclaurin felt the need to replace the force or *vis impressa* with power, a rather different quantity. d'Alembert, in *Traité de dynamique* (1743), formalized the second law in a similar manner to Varignon to avoid the notion of force.

Euler had used Varignon's formula as early as 1735 in his *Mechanica*. In a 1737 paper he repeated that if a force p is applied to body of mass A and velocity u in the direction of u, then the infinitesimal change in velocity du in the infinitesimal time interval dt will be (in absolute value),

$$du = p\frac{dt}{A},$$

which is Varignon's formula.[321] Despite these precedents, in 1750 he claimed to have discovered a new principle of mechanics, which he communicated to the Academy of sciences of Berlin.[322]

Without producing any justification for the "new principle", Euler stated it as follows: let an arbitrary force be applied to body of mass M. When the force is decomposed along the three orthogonal cartesian axes, let P be the component along the x axis. The "new" principle required that

$$2Md^2x = \mp Pdt^2$$

which is equivalent to

$$P = \mp 2M\frac{d^2x}{dt^2}$$

where the positive or negative sign indicates the vectorial nature of the force P and the factor of 2 appears because Euler calculated the time based on the distance between the initial position of a falling body and the surface of the Earth (and defined the mass of a body as its weight on the Earth's surface). "This formula alone," Euler said, "contains all the principles of mechanics," starting, as he demonstrated, from the first law.

Why did Euler consider his analytic expression a "new" principle? Perhaps he saw that the new formula finally gave a general definition of force, which Newton had divided into too many special cases that were not immediately useful in calculations; perhaps he realized that by defining

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321 P. Varignon, *Des forces centrales ou des pesanteurs nécessaires aux planètes pour leur faire décrire les orbes qu'on leur a supposés jusqu'ici*, *ibid*., pp. 224-43, and in particular pp. 241-43.

322 L. Euler, *Découverte d'un nouveau principe de méchanique*, in "Histoire de l'Académie royale des sciences et belles lettres de Berlin. Classe de mathématique.", 1770, pp. 185-217, later in Id., *Opera omnia*, edited by J.O. Fleckenstein, Lausanne 1957, series 2, Vol. 5, pp. 81-108.

certain physical quantities in terms of others, he could resolve the problem of equation homogeneity, which had forced Varignon, and later d'Alembert, to either neglect them or resort to long and difficult formulas in his calculations. In any case, the analytic translation of Newton's second law of motion required over sixty years of work and was presented without reference to Newton's original proposition.

Despite being less eventful that the translation of the second law, the analytic transcription of Newton's mechanics was a lengthy ordeal that began with the clear and systematic vision of Leonhard Euler, one of the most influential figures in eighteenth century science. Born in Basel on 15 April 1707, Euler received his first mathematical education from his father and later became a student of Johann I Bernoulli. During his studies, Daniel and Nikolaus Bernoulli became interested in his prospects and Euler was called to Saint Petersburg by Queen Catherine I on their recommendation. In 1733, he succeeded Daniel Bernoulli as then mathematics professor at the Academy of Saint Petersburg. In 1741 he left Russia for Berlin, where he had been summoned by Friedrich II, and was named the director of the mathematics class at the Berlin Academy. In 1766, he returned to Saint Petersburg, where he remained until his death on 7 September 1783. A mathematical genius, Euler contributed to almost every science of his time: rational mechanics, physics, astronomy, and navigation; he saw philosophy as inseparable from science, and his numerous popular science work were very successful in all of Europe.

Leonhard Euler set out to develop mechanics as a rational science organized by a few definitions and axioms, such that mechanical laws would appear not only certain, but as "necessary truths". Euler's dynamics was based on the primitive concept of force. He distinguished between external forces or powers, which cause changes in the motion of an object, and the force of inertia (*vis inertiae*), that is, the inherent tendency of an object to remain in its present state of either rest or uniform rectilinear motion. The force of inertia is proportional to the amount of matter and resides inside the body, both when it is at rest and in uniform rectilinear motion. It follows that force, that is, the cause that produces a change of state in a body, is always external to the body, while the force of inertia or inertia "exists in the body itself and is an essential property":[323] in substance, this was a new formulation of Newtonian concepts that permitted an easier analytic translation. In this framework, forces were compared only through their static effects, under the assumption that the static composition of forces is also valid for dynamical effects, a result that Euler erroneously believed to

[323] L. Euler, *Letters to a German princess*, trans. Italian, Boringhieri, Turin, 1958, p. 247.

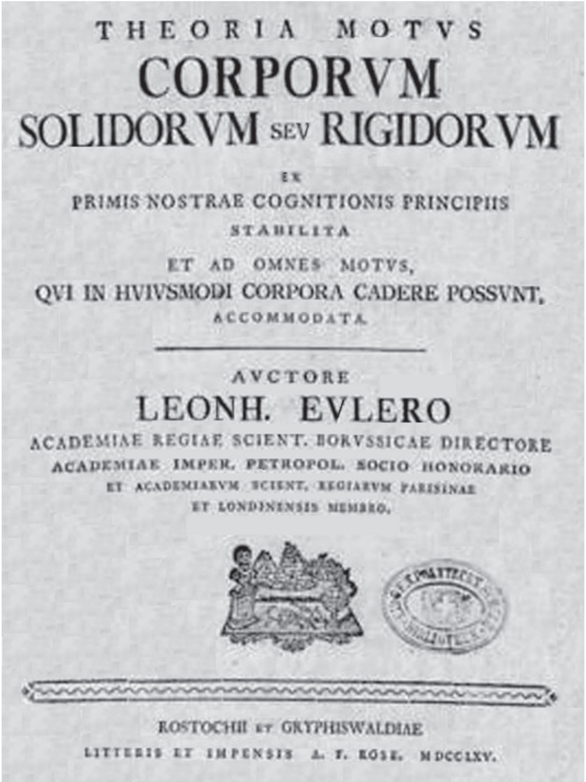
have demonstrated. This approach especially emphasized the impulse resulting from force and extended to the (mostly analytic) study of a great number of problems regarding the motion of a free or constrained point, on a curve or surface, in vacuum or a resistant medium.

The analytic transcription of mechanics continued with Jean-Baptiste le Rond d'Alembert, the natural son of an artillery general and one of the greatest French mathematicians of the first half of the 18th century. Refusing honours and monetary gain, he resisted the insistent invitations of Friedrich II and Catherine of Russia and maintained his scientific independence, remaining in his birth city of Paris his entire life. Despite the importance of his scientific work, today d'Alembert is known less for his work as a scientist and more for his collaboration with Denis Diderot (1713-1784): the famous *Encyclopédie*, for which he wrote the *Discours préliminaire* and the main sections on mathematics. d'Alembert's influence on the progressive cultural circles of Europe was so great that after Voltaire's death, he was considered the spiritual heir to the great philosopher.

d'Alembert's most significant achievement is his contribution to dynamics with the now-classic *Traité de dynamique*, which first appeared in 1743 and republished in 1758 with revisions and additions. In the *Discours préliminaire* (or *Préface* in the first edition), d'Alembert laid out his philosophy of mechanics. According to him, mechanics belongs to the purely rational sciences, that is, the sciences founded on principles that are necessarily true and not simply physical principles or hypotheses. As a purely rational science, mechanics must be purged of principles that are experimental in nature and must be constructed on the fewest principles necessary for the widest application: this was the programme for d'Alembert's mechanics. Later we will discuss the principle that bears his name (§ 7.9) and his contribution to the problem of vibrating strings.

Both Euler's *Mechanics* and d'Alembert's *Traité* only deal with the dynamics of a point. The dynamics of solids followed in 1760, detailed in Euler's *Theoria motus corporum solidorum seu rigidorum* (Fig. 7.2), in which he developed the theory of moments of inertia and systematically studied the motion of a free solid body. He went beyond the central motion passed down from Newton, generally treating all rotational motion and motion from an arbitrary force, thus laying the foundations for modern kinematics and kinetics. His analysis of the motion of a spinning top using the concepts of moment of inertia and inertial axis was particularly perceptive and is still valid today.

After the dynamics of solids, hydrodynamics too began to be *analitice exposita* (analytically expounded) in the work of Johann I Bernoulli and his son Daniel (who applied the theorem of living forces to the flow of liquids through an aperture); in *Théorie de la figure de la Terre tirée des principes de l'hidrodynamique* (1743) by Clairaut; in *Traité de l'équilibre*



THEORIA MOTVS
CORPORVM
SOLIDORVM SEV RIGIDORVM
EX
PRIMIS NOSTRAE COGNITIONIS PRINCIPIIS
STABILITA
ET AD OMNES MOTVS,
QVI IN HVIVSMODI CORPORA CADERE POSSVNT,
ACCOMMODATA.

AVCTORE
LEONH. EVLERO
ACADEMIAE REGIAE SCIENT. BORVSSICAE DIRECTORE
ACADEMIAE IMPER. PETROPOL. SOCIO HONORARIO
ET ACADEMIARVM SCIENT. REGIARVM PARISINAE
ET LONDINENSIS MEMBRO.

ROSTOCHII ET GRYPHISWALDIAE
LITTERIS ET IMPENSIS A. F. ROSE. MDCCLXV.

Figure 7.2 - Frontispiece of the second edition of Euler's *Theoria motus corporum solidorum seu rigidorum* (Greifswald 1790).

et du mouvement des fluides (1744) by d'Alembert; and especially in his later *Essai d'une nouvelle théorie de la résistance des fluides* (1752), in which, basing himself on Clairaut's results and the principle that bears his name, d'Alembert have the partial differential equations that describe the motion of compressible and incompressible fluids. This hydrodynamical "movement" also included the much-admired paper *Principes généraux du mouvement des fluides*, presented by Euler at the Academy of Berlin in 1755, the *Mémoire sur l'écoulement des fluides par les orifices des vases*

(1766) by the French scientist Charles de Borda (1733-1799), and two important treatises of mechanics by the general of the French revolution Lazare Carnot (1753-1823), *Essai sur les machines en général* (1783) and *Principes généraux de l'équilibre et du mouvement* (1803), which is in part a rewriting of the first.

The flaw of all these works and of others written in the same period lies in the fact that they were all based on a hydrodynamic theory distinct from the dynamics of solids. The fusion of hydrodynamics and dynamics occurred in *Mécanique analytique* by Lagrange, first published in 1788, which summed up all of the previous work towards a rational mechanics in the 18th century. This treatise derived all branches of mechanics from the same set of principles: statics, hydrostatics, dynamics, and hydrodynamics. Adopting the ideas and postulates of Galileo, Huygens, and Newton and studying the works of his contemporaries, Lagrange set out to unify the underlying principles and bring out a general analytic method to solve mechanical problems. In the *Avertissement*, Lagrange stated his goals as follows: "I propose to condense the theory of this science and the method of solving the related problems to general formulas whose simple application produces all the necessary equations for the solution of each problem. In addition, this work will have another use. The various principles presently available will be assembled and presented from a single point of view in order to facilitate the solution of the problems of mechanics. Moreover, it will also show their interdependence and mutual dependence, and will permit the evaluation of their validity and scope."[324]

However, Lagrange's chief concern was the elimination of any recourse to geometric representations: "No figures will be found in this work. The methods I present require neither constructions nor geometrical or mechanical arguments, but solely algebraic operations subject to a regular and uniform procedure. Those who appreciate mathematical analysis will see with pleasure mechanics becoming a new branch of it and hence, will recognize that I have enlarged its domain."[325]

Lagrange's mathematical brilliance and the clarity of his ideas permitted him to achieve the goals he had set for himself in an almost perfect work of classical mechanics: the discussion was based on d'Alembert's principle combined with the principle of virtual work, and led to the famous Euler-Lagrange equations and the fundamental equation of motion for the dynamics of a system, the basis of mechanics and modern classical physics.

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[324] J.-L. Lagrange *Méchanique analytique*, in Id., *Œuvres,* Gauthier-Villars, Paris 1867-92, Vol. 11, p. XI.

[325] *Ibid.*, pp. XI-XII

Much like Newton after the publication of *Principia*, the mental effort endured in writing *Mécanique analytique* exhausted Lagrange to the extent that he lost any interest in science, which he viewed even with repugnance. Fortunately, after some time he recovered and returned to his occupation as a leading Enlightenment scientist and citizen of the "universal republic of scholars". Lagrange's progressive and cosmopolitan views arose both from a deep personal conviction and from his lived experiences.

Born in Turin on 25 January 1736 in a French family that had been moved to the court of Savoy by the scientist's great-grandfather, Joseph-Louis Lagrange received his first scientific education from Giovanni Battista Beccaria. In 1755, as soon as he reached nineteen years of age, he became a professor of mathematics at the school of artillery of Turin; in 1757 he participated in the founding of a private scientific society, which later became the current Accademia delle Scienze of Turin. He wrote several papers in the five volumes of *Miscellanea philosophica* published by the society, attracting the attention of the leading scientists of the time, in particular d'Alembert, who soon became his friend and mentor. Through d'Alembert, in 1769 he was called by Friedrich II to succeed Euler in the Academy of sciences of Berlin, of whose scientific section he became the president. He remained in Berlin for eighteen years, experiencing the most productive period of his lifetime. In 1787, after the deaths of his first wife and cousin and Friedrich II, he accepted an invitation to move to Paris and receive a pension from the Académie des sciences. One of the more favoured scientists during the Revolution, he also received the highest honours from Napoleon, which culminated in Lagrange acquiring the title of count. He died in Paris on 10 April 1813.

7.8 Experimental mechanics

Along with the theoretical activity on 18th century mechanics, its experimental aspects were also refined, especially for didactic ends, because, as we have discussed, teaching had transformed from a discursive practice into an experimental one in the first years of the century.

Simple machines, hydrostatic and precision balances, Newton tubes, Galilean inclined planes, centrifugal force machines, devices to study collisions, hydraulic pumps, pneumatic pumps, areometers, barometers, different kinds of siphons, and a variety of levels –all made with care, sometimes cumbersome, and always expensive– constituted the traditional apparatuses commonly used in courses on experimental mechanics. To this list were added new devices created to verify, clarify, or disprove the theories of the previous century: Poleni's devices, which demonstrated

(1718) that the flow rate of a liquid through an aperture is 0.571 times the theoretical value given by Torricelli (§ 5.2) and increases considerably if a short cylindrical tube with the same diameter as the aperture is placed inside it; the anemometer (1734) of Count Pajot d'Ons-en-Bray (1678-1754); the barographs that became widespread in the last thirty years of the century; hygrometers, the most famous of which was the one shaped like a hat (1783) of Horace-Bénédict De Saussure; and many other devices. The largest and most expensive collections were owned by royal families for the scientific education of their scions.

Above, we have mentioned a few works of experimental mechanics here and there; now we turn to the specific study of the Atwood machine, research on friction, and the torsional balance.

In 1779, George Atwood (1746-1807), "an excellent mathematician who has an extraordinary talent for making the most abstract of phenomena practical," built a machine "to demonstrate nearly all of the theorems on velocity, force, acceleration, and the uniformity of rectilinear motion."[326] The first description of the machine was given by John Hyacinthus Magellan (1723-1790) in a letter addressed to Volta, which was printed in London in 1780;[327] Atwood's own description appeared in 1784,[328] though the fundamental idea behind the machine had already been employed in experiment XLV in a collection of experimental lectures that he published:[329] the motion of freely falling bodies is slowed with a device made up of two unequal weights hanging from the ends of a string that extends across a fixed pulley. Volta was one of the first to obtain and use the machine: Magellan had sent him a model from London, the third such machine to be constructed including Atwood's original. Not only was Volta satisfied with the machine, he was enthusiastic. "For some days," he wrote to the count of Firmian, "I have put the machine to the test, and so great is the satisfaction that I obtain from it that I can hardly concern myself with anything else. I have shown my experiments to my students and several other people, who have

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[326] Letter from 9 April 1779 from Magellan to Volta, in A. Volta, *Epistolario*, Zanichelli, Bologna 1949-55, pp. 339-40.

[327] M*achine nouvelle de dynamique inventée par M.G. Atwood au moyen de laquelle on rend très aisément sensible les loix du mouvement des corps en ligne droite, et en rotation*, London 1780. The letter, which belongs to the Bibliothèque nationale of Paris and the British Museum of London, is so rare that the editing committee for the collected works of Volta was not able to procure a copy to insert it in the *Epistolario* (cfr. Vol. 2, p. 15).

[328] G. Atwood, *A Treatise on the Rectilinear Motion and Rotation of Bodies*, *with a Description of Original Experiments relative to the Subject*, Cambridge 1783, p. 298.

[329] G. Atwood, *Collection of lectures in experimental physics*, 1776.

remained in awe of the simplicity and exactness observed."[330] The machine, modified by Johann Poggendorff in 1842 and 1854 and later by others, was used in teaching up until the first decades of the 20th century.

Towards the end of the 17th century, manufacturers of machines found themselves in a curious predicament: if they applied the theoretical rules of the novel mechanics that had been established between Galileo and Newton to their machines, the theoretical predictions were in marked contrast with the practical operation of the machines (a simple case was the gap between theory and practice for the force necessary to drag a body up an inclined plane). Calculations based on practical machines therefore became so widely discredited that, as Guillaume Amontons noted, "the term machine is often used in a negative sense and at times even becomes an object of contempt."[331] Amontons attributed the cause of this disrepute to the scant attention paid by the manufacturers to friction and the rigidity of ropes, that is, the ropes' resistance to being rolled up or unrolled by a cylinder.

His diagnosis was spot on: though the phenomenon had been known since antiquity and mentioned by Heron, its cause was not because of a lack of experimental studies (aside from those of Leonard da Vinci, which were still unknown). Amontons began them with an experimental setup made up of a flat surface (connected to a greased surface by a spring) pulled by a parallel force and measured by a dynamometer. Changing the planes "in all possible manners", Amontons found that the friction is proportional to the force with which one body presses upon the other, which is independent from the size of the contact area. For greased iron, copper, lead, and wooden sheets, this force is around one third of the pressing force, and in general the force is proportional to the relative velocity. Lastly, Amontons added that sliding friction is greater than rolling friction.[332]

The fact that friction is independent of the contact area surprised Amontons,[333] who attempted to derive the result theoretically, attributing

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[330] Volta, *Epistolario* cit., Vol. 2, p. 36.
[331] G. Amontons, *De la résistance causée dans les machines, tant pour le frottement des parties qui les composent, que par la roideur des cordes qu'on y employe, et la manière de calculer l'un et l'autre*, in "Histoire de l'Académie royale des sciences. Avec les mémoires de mathématique et de physique. Memoires", 1699, p. 206.
[332] *Ibid.*, p. 208-10.
[333] Amontons, using an unfortunate expression, wrote in the original French: *"La résistance causée par le frottement n'augmente et ne diminue qu'a proportion des pressions plus ou moins grandes suivant que les parties qui frottent on plus ou moins d'étendue*," (*ibid.*, p. 208). The statement can be translated into English as "The resistance caused by friction increases and decreases only in proportion to larger or smaller pressures whether the rubbing parts have greater or lesser extent," but confusion can arise from the use of "*suivant que*", which often is used to indicate

friction only to the roughness of the contact surface, where the sharp points of one surface fit into the indentures of the other. This independence also surprised Philippe de La Hire (1640-1732), who, however, felt the need to confirm it himself through new and more refined experiments than Amontons': on a horizontal wooden table, he slid other wooden tables with different surfaces but the same mass pulled by a weight and pulley system.[334] These experiments confirmed the independence of friction from the contact area, nevertheless, theoretical study of the phenomena led de La Hire to suppose that there should be cases in which friction is independent on the pressing force. More precisely, when the peaks on rough surfaces rub against each other and break, or when air is driven out from between two oiled surfaces, the two surfaces adhere due to the effect of atmospheric pressure, as many physicists at the time still believed. In such cases, the force of friction F would be, translating de La Hire's thinking into mathematical notation,

$$F = F_0 + fP,$$

where F_0 is independent of the pressing force P, and f is a coefficient that depends on the nature of the surfaces in contact.

The majority of mechanicists welcomed Amontons' conclusions, ignoring the observations of de La Hire and giving little weight to the reservations expressed by Pieter (Petrus) van Musschenbroek (1692-1761), who in 1729 found a partial dependence of friction on the contact area. However, as the sizes of the machines and the forces employed increased, naval engineers complained more and more of a disconnect between the theoretical predictions and the practical effects, leading the Académie des sciences of Paris to hold a competition in 1779, later repeated in 1781 with a doubled prize, for the development of the laws of friction, establishing that "the laws of friction and the examination of the resultant effects be determined on the basis of new large-scale experiments… and that the experiments be applicable to naval instruments like the pulley, the capstan, and the inclined plane."[335] Refining de La Hire's experimental apparatus,

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dependence. The proposition, which referred to his experimental apparatus, indicates the idea expounded in the text. Some historians, on the other hand, mistakenly interpreted the phrase as having the opposite meaning, namely that Amontons considered friction proportional to the size of the contact surface.

334 *Ibid.*, pp. 104-06.

335 C.-A. Coulomb, *Théories des machines simples, en ayant égard au frottement de leur parties et à la reideur des cordages…*, in "Mémoires de mathématique et de physique présentés à l'Académie royale des sciences par divers savant, et lûs dans ses assemblées", 10, 1785, p. 165.

Coulomb participated in the competition and won the first prize with the classic paper cited above, which was divided into two parts: the first dedicated to sliding, static, and dynamical (kinetic) friction; the second dedicated to the rigidity of ropes. The experiments he described were "large-scale" and performed with equipment from the port of Rochefort that had been made available to him.

It resulted from the experiments that if two surfaces that are in "bare" contact, or as Coulomb called it "dry contact" (an expression that has remained in science), slide across one another, "the ratio of the pressure to the friction is always a constant quantity and is negligibly affected by the size of the surfaces."[336] However, the scientist cautioned that if the contact area is small, then the force with which one body presses against the other is also small, and that friction varies in a highly irregular manner: herein lies the difference between large-scale experiments with pressures of "several quintals" (the French "quintal" was equivalent to 48.951 kg at the time) and small-scale experiments.[337] Furthermore, the force of friction depends on the nature of the surfaces in contact and the rest time, i.e. the length of time for which the surfaces remain in contact without moving. That static friction is greater than kinetic friction was known since 1722 due to François-Joseph de Camus (1672-1732) and reaffirmed (1763) by a respected contemporary of Coulomb, the Abbé Charles Bossut (1730-1814). Coulomb, however, conducted a quantitative study of the phenomenon, whose laws he believed could be summarized by the formula[338]

$$f = \frac{A + mT^{\mu}}{C + T^{\mu}},$$

where f is the force of friction, A, C, m, and μ are four constants that can be experimentally determined from two limiting conditions: for $t = 0$ one must have $f = A/C$, and for $t = \infty$ one must have $f = m$. Due to the length and subtlety of his experiments, however, Coulomb did not determine these constants.

The study of kinetic friction was undertaken using the same methods as the previous case, namely applying a constant force to the body being examined by attaching it through a pulley to a falling weight. The respective times taken by the sliding body to travel across the first and second half of a 6 foot (1.949 m) or 4 foot (1.297 m) distance were found to be approximately in a 100 to 42 ratio. The body, concluded Coulomb, is in

[336] *Ibid.*, p. 173.
[337] *Ibid.*, p. 192.
[338] *Ibid.*

uniformly accelerated motion, therefore, as the force applied to the body is constant, so must the resistance due to friction be constant, independently of the contact area of the surfaces. Yet a path of barely two metres was hardly enough to draw reliable conclusions, despite the abilities of Coulomb and his two collaborators, one who counted the oscillations of a pendulum and the other who shouted to announced every time the end of the sliding object crossed another mark on a ruler.

Using a straightforward procedure, Coulomb calculated the force of friction from the law governing the slowed fall of the weight: in some cases (for example, oak against oak), the kinetic friction resulted less than one-fourth of the static friction, while for metallic surfaces in contact the two frictional forces were nearly equal.[339] The experiments, accurate few in number, allowed him to conclude that kinetic friction is proportional to the force that pushes one surface against the other and is independent of contact area and relative velocity (but for heterogeneous bodies grows with increasing velocity).

These results led Coulomb to believe that friction is due to the roughness of surfaces and that "cohesion [adhesion] has very little influence… because it would necessarily need to act proportionally to the extent of the surface":[340] this was the reasoning repeated for over a century and a half until it was established that the effective contact occurs only at a very small fraction of the apparent contact surface.[341] On the other hand, continued Coulomb, the adhesion is not exactly zero, but only negligible in practical cases when the pressure per square foot is of several "quintals". Ultimately, Coulomb accepted de La Hire's formula with the caveat that the term F_0 should be attributed to adhesion and not atmospheric pressure.

Though the laws proposed by Coulomb were met favourably, criticism grew regarding the values that he gave for the coefficients of friction to the point that Arthur Morin (1795-1880), then captain of the artillery of the French army, set out to verify them by working on even larger scales, beyond the limits of the Navy's equipment. Morin reported his principal results in two long papers published in 1833.[342]

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339 *Ibid.*, p. 254.

340 *Ibid.*, p. 255.

341 A. Borsellino*, Considerazioni sull'attrito*, in "Giornale di fisica", 10, 1969, pp. 300-04: this work details several results obtained in the study of friction during the decades leading up to its publication.

342 A. Morin, *Nouvelles expériences sur le frottement, faites à Metz en 1831,* in "Récueil des mémoires présentés par divers savants étrangers à l'Académie royale des sciences de Paris", 10, 1833, pp. 1-128, 591-696; the papers were also published in two separate instalments (Paris 1832 and 1834), to which the following citations refer.

Morin's experiments, wider-ranging and more refined than those of Coulomb, were structured in a conceptually identical manner to their predecessors and perhaps did more harm than good to physics research. Indeed, while Coulomb presented his laws simply as approximations that are useful for practical application, thus leaving the field open to further experimental and theoretical investigations, Morin proclaimed that Coulomb's laws were exact and only his coefficients were incorrect (too small), explicitly excluding the dependence of friction on velocity or contact area,[343] "such that there is no need to account for adhesion, as Coulomb has done."[344]

After Morin, the laws of friction passed from book to book and became universally accepted, with the addition of a few misguided generalizations absent from the works of the classical authors: for example, none of the cited authors had ever written that the coefficient of friction is always less than one, they had only reported experimental values below one.

It is worth adding that in the first paper Morin described one of the first, if not the first, instruments for the continuous tracking of motion. In the device, which he used to study the motion of a sliding body, a known uniform motion (in his case, the uniform circular motion of a geometric compass) is combined with the motion being studied (in his case, the motion of the pulley axle to which the rope pulling the sliding object is attached). After a few years, Marin adapted the device into a machine to study falling objects that was then used in physics laboratories for over a century despite, in our opinion, its modest didactical efficacy.

Let us return to Coulomb, who in a 1777 paper on magnetization had proposed to suspend magnetic needles to a hair or a thread of silk to avoid friction in pivots. To study its elastic behaviour under torsion, he suspended a copper disk by attaching a wire through its centre and made the system oscillate in the horizontal plane of the disk by twisting the wire: he observed that the oscillations were isochronous and concluded that the torsional force is proportional to the angle of torsion[345] and that air resistance has a negligible effect on the phenomenon.[346] Unbeknownst to Coulomb, the

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[343] A. Morin, *Suite des nouvelle expériences sur le frottement faites à Metz en 1832*, Paris 1834, p. 39.

[344] *Ibid.*, p. 95.

[345] C.-A. Coulomb, *Recherches sur la meilleure manière de fabriquer les aiguilles aimantées…*, in "Mémoires de mathématique et de physique, présentés à l'Académie royale des sciences, par divers savants, et lûs dans ses assemblées", 9, 1780, p. 202.

[346] *Ibid.*, p. 212.

reverend John Michell (1724-1793) had reached the same conclusion using the same device in 1750.

After having published the paper on friction, Coulomb repeated his torsional studies with the aim of "determining the elastic force of torsion of iron and brass wires, in relation to their length, their thickness, and their degree of tension."[347] The experimental procedure consisted in hanging a cylindrical weight from a metallic wire along its axis and making it oscillate around this axis due to the torsion of the wire.

Taking, as he had experimentally found in his 1777 paper, the force of torsion to be proportional to the angle of torsion, a simple calculation allowed Coulomb to establish the period of oscillation *T*:

$$T = \pi\sqrt{\frac{Ma^2}{2n}}$$

where *M* is the mass of the cylinder, *a* is its radius, and *n* is a constant equal to $Pa^2/2\lambda$, with *P* being the weight of the cylinder and λ the length of the isochronous pendulum. From the period of oscillation one can then derive the force of friction *f*.

Experimenting with wires of different materials, different lengths, different diameters, and different tensions and comparing the consequent periods of oscillation, Coulomb arrived at his famous laws of friction, which can be summarized in the formula

$$f = \mu\frac{BD^4}{l},$$

where *B* is the angle of torsion, *D* is the diameter, *l* is the length, and μ is a coefficient that depends on the material and, as Coulomb showed, should be determined by experiment.

We add in passing that in the formula for the period of oscillation, $Ma^2/2$ is clearly the moment of inertia of the cylinder about its axis, but only in 1801 did Coulomb propose to experimentally find the moment of inertia of an arbitrary body by comparing its torsional period with the torsional period of another body with a known moment of inertia.

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[347] C.-A. Coulomb, *Recherches théoriques et expérimentales sur la force de torsion et sur l'élasticité des fils de métal. Application de cette théorie à l'emploi des métaux dans les arts et dans différentes expériences de physique*. *Construction de différentes balances de torsion, pour mesurer les plus petits degrés de force. Observations sur les lois de l'élasticité et de la cohérence*, in "Mémoires de l'Académie royale des sciences", 1784, p. 229.

"Based on the theory above," wrote Coulomb, "and the experiments on which it is founded, one will be able to measure minute forces, which require a precision that ordinary means cannot provide."[348]

He gave a first example by describing a torsional balance for the measurement of friction between a solid and liquid, and announced the construction of an electric balance and a magnetic balance, described and applied in seven papers published from 1785 to 1789 that made him famous (§ 7.33).

For a rather different endeavour, the determination of the density of the Earth, Michell proposed to use his own torsional balance, an idea that Cavendish later assured was independent of Coulomb's. This may well be true, but the fact remains that the theory of the torsional balance was given by Coulomb and not by Mitchell, who, busy with other matters, only got around to building the instrument shortly before his death and did not even have time to experiment with it. After his death, the instrument fell into the hands of the reverend William Hyde Wollaston (1766-1828), a professor at Cambridge, who, uninterested in experiments of that nature, gave it to Henry Cavendish (1731-1810). Cavendish modified the device in such a way that the experiment was not perturbed by air currents, even convective ones, and the experimenter could remain outside of the room housing the device and observe its movement with a telescope. In a paper published in 1798, Cavendish detailed the experiments conducted and results obtained.[349]

The experiment is simple to explain: a horizontal needle with two small spheres attached to its ends is hung by a thin metallic wire. The system's period of oscillation is found, giving the torsional constant of the wire. Then, two large lead balls are brought close to the small spheres and attract them, causing the wire to twist at a certain angle, from which the force of attraction can be deduced. Based on the geometric conditions, through Newton's law, one can then deduce the universal gravitational constant and the mass and density of the Earth. It is a very delicate experiment that requires numerous contrivances and corrections: Cavendish zealously conducted it with his characteristic skill, establishing the relative density of the Earth with respect to water as 5.48, with an uncertainty that, according to him, does not surpass 1/14. The gravitational constant, based on his experiments, came out to a value of $6.754(10^{-8})$ in c.g.s. units.

Cavendish had one last qualm: is Newton's law valid at any distance? It appears that Boscovich's reservations (§ 7.6) were also present in Cavendish's thought; however, the theory of Boscovich itself must have

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[348] *Ibid.*, p. 255.

[349] H. Cavendish, *Experiments to Determine the Density of the Earth*, in "Philosophical Transactions", 1798, pp. 469-526.

allayed his worries, because he answered his question by saying that the distance between the spheres in the experiments was not so small that one could suppose the force of attraction to have the same nature as cohesive forces.

Cavendish's research was novel in its experimental equipment, not in its results. As early as 1739, Pierre Bouger, from the observed attraction exerted by a large Andean mountain at the equator, measured by the change in period of a pendulum, had calculated the density of the Earth to be 4.7: this experiment, we note in passing, was a serious blow to the surviving Cartesianism. Bouger's experiment, modified by the astronomer George Biddel Airy (1801-1892) by comparing the periods of oscillation of a pendulum at the bottom and at the surface of a mine, gave a value of 6.565 for the Earth's density in 1854. In 1772, Nevil Maskelyne (1732-1811) devised a method to measure the deviation of a lead wire based on the attractive effect of a nearby mountain; from this measurement, in 1778 Charles Hutton (1737-1823) deduced the density of the Earth to be 4.5. In the course of the century, many other analogous measurements followed.

Cavendish's experiment was repeated by numerous other experimenters: here we mention Ferdinand Reich (1799-1822) in 1838; the British scientist Francis Baily (1774-1844) in 1842; Karl Ferdinand Braun (1850-1918) in 1896; down to the experiment conducted by Paul Heyl and Peter Chrzanowski in 1942, which gave a value of $(6.673 \pm 0.003) \cdot 10^{-8}$ c.g.s. units for the gravitational constant.

7.9 d'Alembert's principle

While Newton's principles were enough to treat any mechanical problem, in the course of the century it became convenient to introduce specific principles (of virtual work or velocities, of the conservation of the centre of gravity, momentum, area, living forces, etc.) that allow for an easier treatment of several particular types of problems. Among these principles, we feel that d'Alembert's principle and the principle of least action are particularly worthy of mention, and therefore we discuss them in the following two sections.

To d'Alembert, "accelerating force", a concept derived from the second law of motion and defined as the quantity proportional to the increment in velocity, did not appear a necessary truth or at least had not appeared as such from the evidence hitherto provided: accepting it, then, as a contingent truth would have meant reducing mechanics to "nothing but an experimental science." Furthermore, he noted, such a concept is not useful for mechanics as it can be replaced with a more general principle. d'Alembert first stated this principle in a 1742 letter to the Académie des sciences of Paris, expounding it the following year in the aforementioned *Traité de dynamique*, a relatively thin work of great historical importance, though difficult to read.

d'Alembert considered the general case of a constrained mechanical system and showed that there must be an equivalence between the real forces applied to it and the forces that would be necessary to produce the same motion if the constraints did not exist. If one writes this condition of equilibrium – which constitutes "d'Alembert's principle" – one finds that the constraint forces, which are usually unknown, are eliminated. It follows that every dynamical problem reduced, in a certain sense, to a problem of equilibrium, or statics, as Lagrange observed, though d'Alembert unequivocally affirmed that his principle, the principle of inertia, and the principle of composition of motion "are essentially different from each other."

In reality, d'Alembert's principle had been applied (1703) by Jakob Bernoulli to the study of the physical pendulum and derived from Newtonian concepts. d'Alembert's merit, however, was that he oversaw the principle's exceptional fruitfulness, and based his theory of dynamics on the three principles above, of which d'Alembert's, though simple to state, is the most delicate. Among the wealth of problems solved by d'Alembert based on these foundations, we note the collision problems, which he treated without resorting to the theorem of living forces, and the famous calculations (1749) of the precession of the equinoxes and the shift of the Earth's axis, despite the fact that the two calculations had both been performed by Euler (1745) without the use of d'Alembert's principle.

7.10 The principle of least action

While Huygens had attempted to remove any metaphysical value from Fermat's principle (§ 5.33), it was this very attribute that was prized by Leibniz, whose physics, as we have seen, was so imbued with metaphysics that it is hard to tell where one field ended and the other began. Leibniz was not satisfied with either the Cartesian derivation of the law of diffraction or Fermat's axiom, to which he objected that bodies in motion to not have the intelligence to choose the path of least time. After much thought and many preliminary studies, as attested by his manuscripts, in 1672 he published a brief article in the "Acta eruditorum" of Leipzig, in which he announced that he had derived all optical, catoptric, and dioptric phenomena from a single principle: light travels from the luminous point to the illuminated point along the path of least resistance, where resistance is defined as the product of the path length and the inherent resistance of the medium. This is still an economizing principle of sorts, but for Leibniz nature does not economize on time but on work.

Yet could not Leibniz's own criticism of Fermat's axiom also be applied here? The philosopher assured his readers otherwise, "*si rem recte consideres*" (if you may consider the matter correctly). He immediately added, "In propagating, light does not possess intelligence, but the Creator thus created light, so that from its nature that most beautiful event could arise. And thus, they err greatly, which I shall not say more seriously, who reject with Descartes the final causes in physics. Indeed, these not only permit us to admire the wonder of divine wisdom, but also offer a beautiful principle for discovering the phenomena whose intimate nature we do not yet know clearly enough to use the effective nearby causes to explain the mechanism employed by the Creator in producing the effects and obtain his ends."[350]

By this point, Leibniz was fully in the realm of metaphysics, and the situation worsened when he added, perhaps to adapt to the common belief of the time, that the speed of light increases with resistance, that is, with the refractiveness of the medium. The proposition, which he justified with a curious theory of dynamics, defies everyday experience and the most basic physical intuition, which sees "resistance" as an obstacle to motion and therefore a cause of velocity reduction. Yet there was more: to those who objected that in some well-known cases at the time involving reflection of curved surfaces, light follows the longest path and therefore the one of maximum resistance, the philosopher would have responded as Fermat had earlier: the least path is not to be evaluated with respect to curved surfaces but rather the tangent planes of these surfaces at the incident points of the light, because, as Leibniz informed his readers with certainty, "nature has no knowledge of curvatures, knowing only planes and tangent lines because it obviously acts consistently in accordance with fixed rules."[351]

Because of his self-imposed duty to reconcile the physics of Aristotle with the physics of his time, his scientific views oscillated often. Even for refraction did his faith in his own principle (which he never attempted to extend to other kinds of motion) waver, eventually disappearing after the appearance of Huygens' *Traité de la lumière*. "In the 'Acts of Leipzig' of another time," Leibniz wrote to Huygens, after having read his treatise with

[350] Leibniz, *Opera omnia* cit., Vol. 3, p. 146.
[351] Unpublished fragment, published by E. Gerland, *Leibnizens nachgelessene Schriften physikalischen, mechanische und tecnischen Inhalts*, in "Abhandlungen zur Geschichte der mathematischen Wissenschaften", 21, 1906, p. 62.

admiration, "I had said something on refraction, but your hypothesis seems the more plausible to me."[352]

However, though Leibniz appeared to have renounced the postulate of least work in favour of Huygens' wave hypothesis, minimum problems continued to fascinate him, both for the metaphysical meaning that he saw in each minimum, which for him revealed a preordained harmony, and for impressive efficiency of the algorithm he invented to find minima. In 1687, he proposed the problem of finding the isochrone curve, the path for which the time taken by a mass sliding under the influence of gravity (to the endpoint of the curve) is independent of its starting height. The problem inaugurated an interesting period in the history of mathematics that saw mathematicians dedicated to seeking curves with particular mechanical properties (isochrone, catenary, elastic curve, etc.).

Within this current of research, Johann Bernoulli proposed an important problem in 1796: finding the brachistochrone curve (Bernoulli's neologism), the path of fastest descent between two points *A* and *B* that are neither on the same vertical line nor on the same horizontal line, which Galileo had misguidedly thought to be an arc of a circle (§ 4.16).

The challenge proposed by Bernoulli was accepted by his brother Jakob, Leibniz, Newton, and Guillaume François de l'Hôpital (1661-1704), all of whom demonstrated, independently from each other, that the brachistochrone curve is a cycloid. Before announcing his challenge, the challenger had found two solutions to his problem. Leibniz advised that he publish only one of the two solutions, the one based on Fermat's principle.

This episode, which has been historically verified, can be considered indirect evidence that Leibniz truly had given up on his minimum principle. In the solution preferred by Leibniz, after having assumed Fermat's principle, one supposes that a ray of light moves in a medium whose refractivity varies with the velocity of a falling body, that is, with the square root of the fall height. The ray of light therefore travels along a path that is the brachistochrone curve of the falling body. In this way, Bernoulli's problem was reformulated as a problem of calculus of variations. Its solution was highly admired at the time and widely imitated, giving rise to a proliferation of similar problems. Unfortunately, the problem was also the origin of a bitter war, fuelled by unrelenting hate, between the two Bernoulli brothers, one of the darker pages in the history of mathematics.

However, to remain with Leibniz in the "best of possible worlds," we note that the rivalry between the brothers had a positive aspect: minimum problems became commonplace in the Bernoulli family. They were also

[352] Letter from Leibniz to Huygens on 26 April 1694, *Œuvres complètes* cit., Vol. 10, p. 602.

studied by Daniel (1700-1782), son of Johann, who, encouraged by his initial successes, gave Euler the problem of determining the trajectory of a planet through a minimization property that selects between the various possible orbits. Euler became very invested in the problem, dedicating himself to it for several years and publishing the solution in 1744 in his treatise on isoperimeters. His solution was based on the following new principle: in motion due to central forces, the integral of the products of the velocities of mass elements and the length element they travel is always a maximum or a minimum. With this statement, minimum principles became extremal principles, destroying the anthropomorphic concept of economization. Those who wish to maintain metaphysical meaning in Euler's principle would have to say that nature prefers excess: at times it is thrifty and at others it is wasteful.

The rationality of extremal principles would not survive for long: Euler freed them from the metaphysical swamp, as Huygens had attempted to do earlier, but Maupertuis brought them back with a 1740 paper, and more firmly with a later 1744 paper that was also read to the Académie des sciences of Paris. Maupertuis began by correctly observing that Fermat's principle, to agree with experiment, requires that the speed of light be smaller in more refractive media, while, according to him, there is no doubt that it is greater, as Newton's optics established. Therefore, concluded Maupertuis, the entire structure built by Fermat was destroyed.

"After meditating deeply on this matter," he continued, "I thought that light, when it passes from one medium to another, having abandoned the shortest path, which is the path of the straight line, indeed also could not take the path of least time. In fact, why should it prefer time over space? Now, light cannot at one and the same time go through the shortest path and do so in the shortest time. But why should it go one path rather than another? It wakes neither of the two. It takes the route that offers a real advantage: the path for which the quantity of action is the least."[353]

Nature's parsimony for this strange quantity, for which have neither intuition nor a representation, certainly deserved a few more explicatory words! Yet Maupertuis quickly moved on to demonstrating, imitating Leibniz's procedure, that if the propagation of light obeys the new principle, then the law of refraction is the same as Descartes'.

In a later paper, read in 1746 and published in 1757 by the Academy of sciences of Berlin, of which Maupertuis had become the president in the meantime, he also extended the principle of least action to the study of

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[353] P.-L. de Maupertuis, *Accord des différentes loix de la nature, qui avoient jusqu'ici paru incompatibles*, in "Histoire de l'Académie royale des sciences, avec les mémoires de mathématiques et de physique", 1744, p. 423.

central collisions. If the principle had been introduced rather arbitrarily, its extension was certainly unfounded. Nevertheless, its success was undeniable, and therefore Maupertuis did not hesitate in stating a "general principle": "When some change occurs in nature, the quantity of action necessary for this change is the minimum possible one."[354] By quantity of action, Maupertuis meant the product of the mass, velocity, and space traversed by a moving object.

The formulation was imprecise, but Maupertuis boldly defended it with arguments on the economy of nature, the wisdom of the Creator, and the harmony of the world. A period of heated debate ensured, initiated in 1751 by Samuel König (1712-1757), to whom were allied, in opposition to Maupertuis, Voltaire, Nicolas Malebranche, Wolff, and others. The controversy had everything: accusations of plagiarism, counter-accusations of falsification, disputes over priority, academic regulations, appeals to the public, and a caustic and libellous pamphlet by Voltaire (*Diatribe du docteur Akakia, médecin du pape*), who could not stand the arrogant president of the Berlin Academy. To avoid a lengthy digression, however, we limit ourselves to noting that while the majority of the controversy was centred on philosophical problems (preordained harmony, final causes, the existence of God), the dispute began with König's observation that in certain cases the action proves to be a maximum instead of a minimum. We also relate the selfless but strange behaviour of Euler, who aligned himself in defence of his Academy president, recognizing his priority in formulating the principle despite his inexact statement, while Euler's own formulation was the correct one.

Pierre-Louis Moreau de Maupertuis, born in Saint-Malo on 17 July 1698, dedicated himself to mathematical and astronomical studies after a brief military career. In 1736-37 he led a scientific expedition to Lapland with the purpose of measuring the length of a degree of arc of the meridian. The measurements allowed Maupertuis (and Clairaut, who was also part of the expedition) to confirm Huygens' theory on the squashed form of the Earth at the poles and reject the ideas of Cassini, who held that the Earth was elongated at the poles. The expedition brought great renown to the scientist, and Friedrich II summoned him to Berlin in 1741 to join the Academy; from 1744 to 1753 he was its president and oversaw a reorganization. Sick and embittered by the relentless attacks and insults from Voltaire (which followed the initial boundless praise because of a personal grudge), Maupertuis retired to Basel with the Bernoulli brothers, who had remained his friends, and died there on 27 July 1759.

[354] P.-L. M. de Maupertuis, *Recherches des loix du mouvement*, in Id., *Œuvres*, Lyon 1766, Vol. 4, p. 36.

The principle of least action, formulated incorrectly by Maupertuis, remained imperfect even in the mechanics proposed by Lagrange, who however cautioned that he saw the principle of least action "not as a metaphysical principle, but as a simple and general result of the laws of mechanics."[355] As early as 1760, in a paper published in the *Miscellanea* of Turin, Lagrange had applied Euler's extremal principle to solving very difficult problems in dynamics. The exact formulation of the principle of least action, however, would come only in 1830 with William Hamilton, as we will later see ([356]).

7.11 Acoustics

In 1715, the British mathematician Brook Taylor, inspired by the experimental studies of Saveur (§ 6.9), tackled the mathematical treatment of the problem of a vibrating string, thus launching the field of 'proper' mathematical physics. He was able to calculate the number of vibrations per second n of a string as a function of its length l, density d, and tension T:

$$n = \frac{1}{2l}\sqrt{\frac{T}{d}}$$

The problem immediately became famous and preoccupied almost all the mathematicians of the 18th century, causing extended (and worthwhile) debate. Among others, the problem was studied by Johann Bernoulli and his son Daniel, Giordano Riccati (1709-1790), and d'Alembert. The latter found (1747) a partial differential equation describing the small oscillations of a homogeneous string:

$$\frac{\partial^2 y}{\partial x^2} = a^2 \frac{\partial^2 y}{\partial t^2}$$

where y is the transverse shift of a molecule of a string oriented along the x-direction and is a function of x and time t. d'Alembert succeeded in integrating the equation with a method still studied by mathematicians today. The problem immediately brought about a dispute between Euler and Daniel Bernoulli that was ended by a young Lagrange with his paper in the first volume (1759) of the *Miscellanea* of the private society of Turin. In

[355] J.-L. Lagrange, *Méchanique analytique*, Paris 1787, p. 139.
[356] §1.4 in: M. Gliozzi, *A History of Physics over the Last Two Centuries.*Cambridge Scholars Publishing, in press 2022.

addition, Lagrange observed that d'Alembert's equation can also be applied to the vibrations of air in a tube.

The most important contribution, however, was made by Euler, to whom physics owes the full theory of waves in strings, first detailed in 1739 with the work *Tentamen novae theoriae musicae* and expanded in many later papers. In particular, it results from Euler's theory that the velocity of propagation of waves in strings is independent of the wavelength of the sound produced.

Euler also conducted theoretical investigations on the vibrations of rods, sheets, and bells. In some cases, however, the theoretical results were not in agreement with the experimental tests done by Ernst Chladni (1756-1827), who is considered the father of experimental acoustics. Chladni's research was expounded in a treatise published in 1802 in his native German and later translated into French by Chladni himself on Laplace's suggestion.[357] The work, equipped with an extensive bibliography, provides a broad picture of the evolution of acoustics throughout the entire 18th century.

The treatise is divided into four parts, of which the first concerns scientific problems in music and the last discusses human and animal hearing organs. The second part is a study of vibrations in strings (transverse, longitudinal, torsional), stretched membranes, air columns, rods, plaques (rectangular, square, circular, elliptical, hexagonal, semicircular, triangular), bells, and vases; it also includes a demonstration the various types of vibration can coexist in a sound-emitting body.

Chladni was therefore responsible for initiating the study of longitudinal waves and torsion in solids, as opposed to the transverse waves in a bar, produced by different methods of excitation (collision, rubbing, etc.). His central discovery was that the longitudinal vibrations of a rod obey exactly the same laws as the longitudinal vibrations of air inside a tube, if a fixed or free end of the rod is taken to correspond to a respectively closed or open end of the tube. As a particular case of the vibrations of a rod, Chladni described the modes of vibration of a diapason exactly for the first time, demonstrating that it behaves like a rectilinear rod with a fixed centre). Studies on longitudinal waves were continued experimentally by Félix Savart (1791-1841) and theoretically by Laplace and Siméon-Denis Poisson.

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[357] E.F. F. Chladini, *Traité d'acoustique*, Paris 1809.

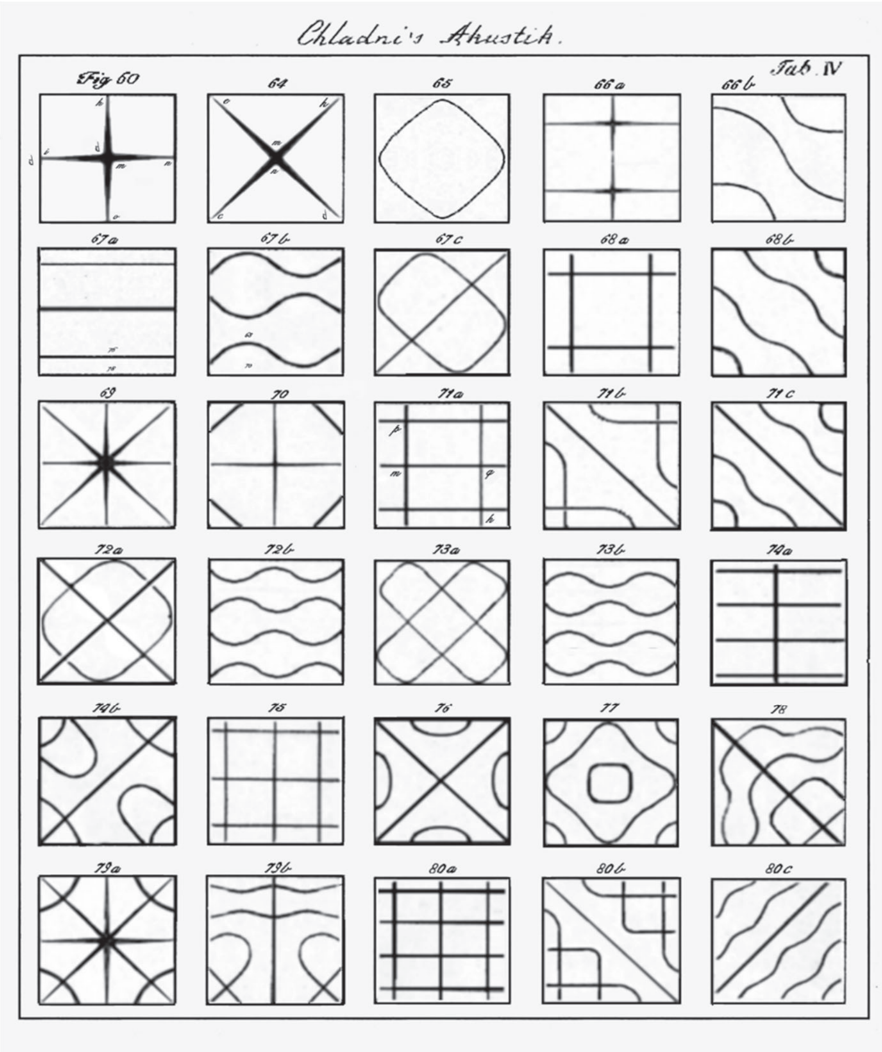

Figure 7.3 - Different Chladni figures of the same vibrating square sheet, experimentally observed by Savart: the lines traced on the patterns are nodal lines.

Also famous were Chladni's studies (1787) on the vibrations of a sheet, which form beautiful *acoustic figures* (Fig. 7.3) that are named after him. Such patterns are formed, to use his expression, by *nodal lines*, which can be observed by covering the vibrating sheets with sand, later replaced with lycopodium powder by Savart. A new problem in mathematical physics arose from these experimental researches: the study of a *vibrating drum*.

With Chladni's work, the need to calculate the number of vibrations per second of different sound-emitting bodies became even more pressing. His solution, certainly an improvement over that of Saveur, still left much to be desired. He proposed to calculate the frequency of a sound by comparing it to the sound emitted by a vibrating rod fixed at one end. He pre-emptively adjusted the length of a rod such that it would vibrate four times per second (counted by eye). The length of the rod was then halved, and Chladni made it vibrate in such a way that a node was present at around a third of the rod's length away from its free end: this bar then emitted a sound corresponding to one hundred vibrations per second. Further decreasing the rod's length, other sounds of calculable frequency were obtained. Through this process, Chladni conducted new measurements on the limits of human hearing; he set the upper limit at 12,000 vibrations per second (while Euler had given a value of 3000) and the lower limit at 30. Similar measurements have been repeated by countless physicists to date and are very subjective, depending on the intensity and quality of the sound.

In the third part of his treatise, which deals with the propagation of sound, a rational explanation of echo is prominent, as it was a rather unpredictable phenomenon at the time, consistently described as a "wonder of nature", but always explained in a perfunctory manner, likening the reflection of sound to the reflection of light. d'Alembert, followed by a young Lagrange in the Turin *Miscellanea*, had observed that there is no catacoustics, the analogue to catoptrics. After Lagrange, Euler gave a theory of the echo in 1765, and more thoroughly in a paper titled *De motu aeris in tubis* (1784). At least in its key points, the theory was mostly based on Chladni's work. The essential idea of the new theory was that an echo is not only produced by the reflection of a sound wave at an obstacle but also when there is an abrupt modification in the wave. For example, in a finite tube open on one end, a sound wave produced inside the tube reaches the open end and then turns around: such an echo cannot be explained by reflection. Such kinds of echoes were studied by Biot in 1809 in a 951-metre-long Paris aqueduct.

Chladni's work and discoveries deservedly won joint "distinguished praise" from the class of mathematical sciences and physics and the class of fine arts of the Institut de France, plaudits that were accompanied by the reflection that "it would be important to direct the attention and emulation of scientists to the physical-mathematical researches that these discoveries can permit."[358]

Because mechanism for the propagation of sound was thought to be the alternating compression and rarefaction of air, and because water was believed to be absolutely incompressible, scientists deduced that sound could not propagate in water. Moreover, this result was generalized to the statement that air is the only medium for the propagation of sound. Nicolò Aggiunti (1600-1635), in handwritten notes preserved in the National Central Library of Florence and partially published by Raffaello Caverni, observed that if one's head is immersed underwater and two rocks are struck together, a loud sound can be heard.[359] Analogous observations were later made by Hauksbee and Nollet, the latter of whom submerged himself in the waters of the Seine in 1743, while in *Lesson of physics* he demonstrated the propagation of sound by placing an alarm (enclosed in a case) inside a large cylinder full of water and no air. Similar experiments were also performed by Franklin in 1762 and Chaldni, as he described in his treatise (where he also demonstrated the propagation of sound in solids); nevertheless, scepticism remained until the experiments conducted in the waters of Lake Geneva in 1837 by Jean-Daniel Colladon (1802-1893) and Jakob Karl Franz Sturm (1803-1855), from which the propagation velocity of sound in water, up until then never measured directly, was deduced (1435 m/s).

Other phenomena related to acoustics were studied in the 18th century (resonances, combined sounds, etc.): all were interpreted as the motion of parts of vibrating bodies and the particles of the medium in which the sound propagates; in other words, all acoustic phenomena are explained as mechanical processes.

OPTICS

7.12 The spread of Newtonian optics

The triumph of corpuscular theory in the 18th century is commonly attributed to Newton's authority, and the harm it did to science is often compared to that caused by deferring to Aristotle's authority for two thousand years: this historical interpretation is perhaps too simplistic. Newton's authority certainly played an important role, but it is doubtful that it was the determining factor in the success of corpuscular theory.

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[358] *Ibid.*, p. 375.

[359] R. Caverni, *Storia del metodo sperimentale in Italia*, Civelli, Florence 1892, Vol. 2, p. 218.

There were no scientific reasons to decide in favour of one theory or the other in the 18th century. Diffraction, today invoked as the *experimentum crucis* in favour of wave theory, had remained a mystery even for Huygens, and light interference was not yet known. The two competing theories both more or less explained the more common phenomena, and both were complicated. Given that simplicity was not an applicable criterion, the common thinking was that one might as well adhere to the corpuscular hypothesis, which presented itself as a theory of optics in the traditional sense, in that it explained, in an immediately intuitive manner, the most elementary of optical phenomena, the propagation of light in a straight line.

Though the vast majority of 18th century physicists supported the corpuscular theory, which, being free from any undulatory element, was also not the original Newtonian theory, there was no shortage of criticisms and reservations, especially on the part of mathematicians. Such dissent, however, remained on the level of individual reactions against the theoretical failings of Newton's framework. Boscovich, for example, preferred to replace the abstruse theory of "accesses" (§ 6.19), in part corpuscular and in part undulatory, with an exclusively corpuscular framework based on his own attractive-repulsive law, sufficient to explain all optical phenomena according to him.

On the opposing side, Euler, Daniel Bernoulli, and Franklin all emphatically rejected corpuscular theory in favour of a wave theory. Euler, after an uncharacteristically vigorous criticism of Newton's theory, attempted to construct a wave theory that could rationally explain the light phenomena known at the time. Huygens was never mentioned, though it is difficult to think that Euler did not know of his work, and the original idea of a undulatory theory was attributed to Descartes, but Euler criticized him for supposing the speed of light to be infinite. The criticism he directed at Newton's theory was rather common at the time (*Nova theoria lucis et colorum*, 1746), and can be summarized as follows: the continuous emission of luminiferous particles over a certain amount of time causes the extinguishing of the Sun; the crossing of beams of light originating from the Sun and the stars, that is, from every direction, modifies their rectilinear trajectory; transparent bodies contain pores arranged in straight lines in all directions; luminiferous particles continuously emitted by the Sun and the stars fill space more than the thin Cartesian matter.

The contradictions of the Newtonian system had to be opposed by a rival framework, wave theory. In this new system, light is a vibration of a very thin medium called *ether* (or *aether*), which can be found everywhere in the universe and permeates all bodies, and whose very low density and very high elasticity explain the significantly greater speed of light compared to sound. For Euler the analogy between light and sound was perfect: "their difference is only one of degree," he said.[360] Consequently, he set out to bring all optical phenomena into the domain of mechanics. Like the pitch of a sound depends on its frequency, so must the difference in colours depend on the different frequencies of the vibration of the ether, with the colours red and violet corresponding to the least and greatest frequencies, respectively. White light is a combination of all the colours. The theory of colours of illuminated bodies was therefore modelled on acoustic resonance. If light, Euler argued, were reflected by an opaque body upon incidence, an observer would not see the opaque body but the source of the incident light, like a mirror. The visibility of opaque bodies must therefore be explained in another manner. To be precise, according to Euler, light incident upon such a body causes the particles on its surface to vibrate in resonance with the incident light wave. It follows that, for a body to appear a certain colour, the light striking it should contain that colour and the particles of the body should be able to vibrate with the corresponding frequency. In other words, the opaque bodies capture the incident light and re-emit it with the frequency with which its surface particles can vibrate. This theory perfectly explains, as we will later see (§ 7.14), the phenomenon of phosphorescence, a phenomenon that had until then remained at the margins of optical theories.

Euler's theory was not very successful: most remained unaware of it; a few, like Priestley, refuted it; and some, like Lalande, resented it. Its only positive effect perhaps was confirming the beliefs of agnostics like d'Alembert: because we know nothing of the nature of light, the correct scientific attitude is to solely describe phenomena.

Aside from Euler, two more unheeded views were those of Jean-Paul Marat (1743-1793), the future revolutionary, and Johann Wolfgang Goethe (1749-1832), the German poet.

Before the French revolution, Marat successfully dedicated himself to simultaneously practicing medicine and conducting physical studies, which despite not entirely devoid of interesting results remained completely outside academic circles. Among other works on optics, in 1780 he published a volume titled *Découvertes sur la lumière constatées par une suite d'expériences nouvelles* (a copy of the work, with annotations and an autograph from the author, can be found at the Bibliothèque national of Paris). Marat's new theory was concerned with the nature of colours and the refraction of light. According to Marat, light is composed of three primary colours: red, blue, and yellow, whose mixture in appropriate dosages gives all other colours and white light. The idea was not new: other physicists,

[360] Euler, *Letters to a German Princess* cit., p. 87.

like Charles-François Du Fay, had reduced the number of primary colours. In addition, the theory stipulated that all bodies attract light, and that its consequent deviation (always increasing from red to blue to yellow) depends on the affinity between the body and light; it is proportional to the surface density of the body and inversely proportional to the square of the distance.

In the immediate vicinity of an opaque body, a ray of light splits into three rays that correspond to the primary colours and strike the body with different angles of incidence but are equally refracted and therefore emerge separately. According to Marat, Newton was mistaken in combining two distinct and successive phenomena: deviation and refraction. To prove that diffraction is not accompanied by scattering, Marat shined the apex of a cone of light produced by a converging lens onto a prism and collected the refracted light on a screen. He observed that it formed a white spot surrounded by an iridescent halo, where the white spot demonstrated, according to Marat, that the light was not scattered. Evidently, the indifference displayed by academics to these experiments was not entirely unjustified.

Goethe's interest in the theory of colours was not the passing caprice of an artist but a passion that occupied him for the large part of his life and to which he dedicated several writings. After subscribing to the Newtonian point of view, he changed his mind and wrote scathing attacks of the scientists, who he accused of only having considered the physical aspect of colours, with the pretext of explaining them only through differences between rays. According to Goethe, "chemical colours" are permanent and intrinsic to bodies, whereas "physical colours" are temporary and arise from different mixtures of light and shadow: these were old ideas that had already been outmoded for some time.

The only one who still paid attention to these ideas was the philosopher Arthur Schopenhauer (1788-1860), for whom colours are only physiological, that is, due to the structure of the retina and its way of reacting to external stimuli: this concept was not without interesting consequences, even for modern science (for example, the connection between intensity and colour), but was so revolutionary for the time (1816) that not even Goethe had the courage to associate himself with it.[361]

[361] Cfr. A. Schopenhauer, *On Vision and Colours* and *Correspondence with Goethe*, 1816.

7.13 Achromatic lenses

The construction of lenses free from colour distortion, or achromatic lenses, as the British astronomer John Bevis (1693-1771) called them using an immediately popular neologism, were the most important invention in the 18th century in the field of instrumental optics. Based on his studies of the human eye, Chester Moor Hall (1703-1771) concluded that lenses could be made achromatic. In 1729 (only two years after Newton's death), after many experimental attempts, he found two types of glass with different enough scattering properties that they could be used to construct an achromatic lens. Around 1733, he succeeded in making achromatic telescopes, which were examined by some scientists of the time: though this is a historically verified fact, the invention, which Hall did not make public, remained unknown in the scientific circles of the time and had no effect on the evolution of technical optics.

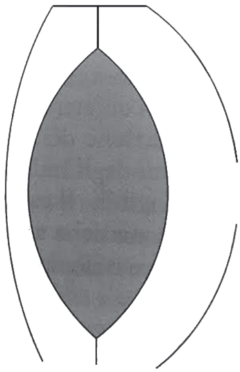

Fig. 7.4

Euler also observed that in the human eye refraction is not accompanied by colouration and attributed the phenomenon to the various substances that light must cross to travel from the cornea to the retina. He therefore thought that appropriate combinations of glass and water lenses could correct colour aberration like they did spherical aberration. In 1747, he set out to calculate the particulars of these lenses starting from the hypothesis that the ratio of the logarithms of the indices of refraction of the mean rays for two substances is the same as the ratio of the logarithms of the indices of refraction of the same substances for less refracted rays. The calculation led him to believe that achromatic lenses could be obtained by conjoining two meniscuses at their concave faces and filling the cavity with water (Fig. 7.4). Six years later, however, he confessed that this approach had not brought satisfactory results. Despite his lack of success on the practical side, Euler made an important contribution to optics by simply reintroducing, in spite of Newtonian hostility, the problem of achromatic lenses. This provoked a reaction from John Dollond (1706-1761), a distinguished British scholar of optics who contested Euler's hypothesis and calculation with an argument resting on Newtonian optics. Euler, in turn, replied to Dollond's criticism. Samuel Klingenstierna (1698-1765), a professor at Uppsala, also joined the debate with a 1755 paper that called Dollond's attention to the fact that the results obtained by Newton on scattering were incorrect. In addition, he confirmed Euler's result: if light is shined through two prisms, one made of glass and the other of water, oriented at an opportune angle with respect to each other, it is possible to eliminate the colouring of the emerging ray without cancelling its deviation and, vice versa, at another angle, it is possible to eliminate the deviation and conserve the colouring of the ray. A summary of Klingenstierna's paper was send to Dollond and induced the British physicist to repeat Newton's experiments, realize their inaccuracy, and begin the first experimental attempts to make achromatic lenses: Dollond related these results in a famous paper published in 1758 in "Philosophical Transactions." Water, however, has an index of refraction so similar to that of glass that, to correct the chromatic aberration, one would have to create a lens so curved that the ensuing spherical aberration would be nearly intolerable. Dollond therefore substituted water with a more suitable transparent medium, in his case flint glass: this idea was his chief contribution to the problem of constructing achromatic lenses. A Dollond achromatic lens was made up of two adjoining lenses, one convex and made of crown glass, the other concave and made of flint glass, a better scatterer.

Achromatic lenses were not widespread in the 18th century, partly because Dolond never fully revealed his manufacturing method, which remained a family secret, partly because of the difficulty of obtaining flint glass in continental Europe, and partly because of the technical difficulty in manufacturing the lenses, which even Dollond directed with an entirely empirical approach.

The most important geometers of the century (Euler, Clairaut, d'Alembert, Boscovich) quickly began to calculate the theoretical aspects of achromatic lenses with the aim of eliminating or at least reducing both the chromatic aberration and the spherical aberration. A new science was thus born, "optical calculus", which was further developed in the following century and is still cultivated by small groups of specialists.

7.14 Phosphorescence

The ability of certain organic substances (fish scales, decomposing meat, etc.) to give off light in the dark was known since antiquity. However, it appears that only around 1604 did Vincenzo Casciarolo discover the same property in an inorganic material, which from then on was called the "Bologna stone", containing barium sulfate. True phosphorus, according to Leibniz's account, was first discovered in 1669 in urine by Brond, a Hamburg alchemist of unknown first name. Phosphorus was commercialized in the first decade of the following century, and all bodies that give off light in the dark were then generically called phosphori.

In the course of the 18th century there were many works on phosphorus published, concerning both the discovery of new phosphori and the study of their properties. Of particular importance was the work published in 1768 by John Canton (1718-1772), in which he described a way to obtain artificial phosphorus by calcifying oyster shells at high temperature with sulphur. The compound (essentially a calcium sulphide), which was called *Canton's phosphorus* for century that followed, shined a bright phosphorescent green. The simple preparation technique made phosphorescent substances, which were rather rare and costly, more widely available.

Canton's paper drew the attention of Beccaria, who then repeated the studies begun forty years earlier by Zanotti, Domenico Maria Galeazzi (1686-1775), Galvani's future father-in-law, and Algarotti to understand if and how of the colour of primary light affects phosphorescent light. Through this approach, the three scientists from Bologna hoped to find a proof of the Newtonian corpuscular theory. After shining prismatic colours onto a piece of Bologna stone, the scientists did not observe any difference in the phosphorescent light obtained, and therefore concluded that the phenomenon was unaffected by the different theories of light. Beccaria thought to have attained the opposite result in 1771 when he experimented on phosphori closed in a box with a stained glass cover, as it appeared to him that the phosphorescent light was the same as the primary light. Beccaria's experiment appeared decisive to Newtonians because, according to them, it showed that the same luminiferous particles that strike and are captured by the phosphorescent body are later re-emitted.

However, no experimenter was able to replicate Beccaria's results, not even Benjamin Wilson (1708-1788), known to the British more as a painter than as a scientist, who, abandoning Beccaria's devices, returned to the techniques of the scientists from Bologna. The many experiments that he conducted by shining light from a spectral prism onto phosphori allowed him to deduce, in opposition to Beccaria, the following general proposition:

"Phosphori do not emit the same light which they receive."[362] This negative proposition – a first approximation to the law published by George Gabriel Stokes (1819-1903) in 1852 – was immediately interpreted by Euler as a patent refutation of Newton's theory and a confirmation of his own (§ 7.12). Indeed, according to Euler, in Wilson's experiment, violet rays (for example) that strike red phosphorus cannot cause its surface particles to vibrate due to their different vibrational period, so their effect reduces to forcing the particles of the body into a state of tension that is free of motion effects. Once the phosphorus is removed from the action of the violet rays, the particles are no longer impeded and can move with their own vibrational period and emit red light.[363] And therefore, the classical explanation of phosphoresce was related to Eulerian concepts. Hegel was right when he said that ideas have hands and feet.

7.15 Photometry

In the Eighteenth century the concept of luminous intensity was made more precise, and scientists began to build devices to aid the human eye in comparing luminous intensities. As early as 1698, Huygens attempted to compare the luminous intensity of the Sun and Sirius; two years later, the French Capuchin monk Jean François-Marie believed (*Nouvelle découverte sur la lumière*, Paris 1700) luminous intensity to be proportional to the number of equally-thick glass sheets needed to completely block the light. Yet the first systematic study was conducted by the French scientists Pierre Bouger, who in 1729 published *Essai d'optique sur la gradation de la lumière.* To compare the intensity of two different light sources, Bouger employed an instrument made up of a vertical screen with two slits covered by white paper, each illuminated by only one of the two sources being examined in such a way that the angles of incidence onto the slits are the same. To use the instrument, an experimenter places her eye behind the screen and the distances of the two sources are adjusted until the two slits appear nearly identically lit to the experimenter: once this has been obtained, the ratio of the luminous intensities of the sources is proportional to the ratio of the inverse squares of their distances to the screen.

The many experimental results Bouger obtained with this device, along with several mathematical calculations, allowed him to study the decrease

[362] B. Wilson, *Additions to a Series of Experiments upon Phosphori and Their Prismatic Colours*, London 1776, p. 20.

[363] L. Euler. *Réflexions sur quelques nouvelles expériences communiquées à l'Académie des sciences par Mr. Wilson*, in "Acta Academiae scientarium petropolitanae pro anno 1777", pt. 1, 1778, pp. 76-77.

in intensity due to reflection, already observed by Guericke and later by Newton. Bouger also shined candlelight onto two mirrors at the same angle of incidence, observing one reflected image and another formed after a second reflection on a third mirror; he then adjusted the candle until the two images appeared to be of the same intensity. With this device and others like it that used sunlight, he studied the reflecting power of different substances and the effects of different angles of incidence. Additionally, Bouger modified François-Marie's law based on his observation that luminous intensity does not decrease with the thickness of the transparent body traversed but rather with its logarithm; he determined the decrease in intensity when light passes through a medium, also observing the selective absorption of various colours in air (research that was repeated later in the century by Musschenbroek, Canton, and Priestley); he measured the ratio of the luminous intensity of Moon and the Sun, and of the Sun at different heights on the horizon. All these studies were collected in his *Traité d'optique*, which appeared posthumously in 1760.

The appearance of *Photometria, sive de mensura et gradibus luminis, colorum et umbrae* by the German mathematician and physicist Johann Heinrich Lambert (1728-1777) marked an important step in the development of photometry. The work was announced in a French treatise on *Les proprietés de la route de la lumière* (La Haye 1759), that is, on geometric optics, which according to Lambert is one of the two subfields into which optics in its entirety can be divided.

The neologism in the title ("photometry"), increasingly employed in the years that followed and still in use to this day, was fitting given the novelty of the concepts and methods contained in the work. Lambert distinguished between splendour, a quantity that pertains to the source, and illumination (*illuminatio*), which pertains to illuminated bodies. In the first pages, the author specified: "To light we attribute the *illuminating force*, that is, the *splendour*. The light that emanates onto objects we will call *illumination*."[364] Theoretical and experimental study started with the second quantity with the proof of four theorems: illumination is proportional to the surface area of the illuminated body, inversely proportional to the square of the distance between the illuminating and illuminated bodies, inversely proportional to the sine of the angle of incidence on the illuminated surface, and directly proportional to the sine of the angle between the rays and the illuminating surface.[365] Keeping in mind that today the angles of incidence are defined as the angles formed by rays with the normal to the surface, the last two

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[364] J. H. Lambert, *Photometria, sive de mensura et gradibus luminis, colorum et umbrae*, Augsburg 1760, p. 19.
[365] *Ibid.*, pp. 35-36.

laws represent the "cosine law" or the "Lambert cosine law", as it is still called today. The second cosine law was qualitatively known by Benedetti, who in the thirtieth chapter of *Divesarum speculationum* observed that the heat received by a surface exposed to sunlight decreases the more the surface is inclined with respect to the light beams; analogous considerations can be found in Day one of Galileo's *Massimi sistemi.*

After illumination, Lambert moved on to the study of splendour; he described absorption in air in detail, confirming Bouger's law on the effect of the medium's thickness in its absorption of light.

In 1740, Bouger abandoned his first type of photometer and replaced it with another, which today can be found among the old relics of physics laboratories. The new device was made up of a screen on which were projected the shadows of two metallic poles formed by the two light sources being compared. In 1795, Benjamin Thomson, count of Rumford (1753-1814), an American who escaped to England and then wandered through half of Europe, revisited Bouger's ideas and very carefully created a more complex instrument. He also saw the need to introduce a "normal light" for the exact comparison of different intensities; he chose the oil lamp built in 1783 by the Swiss Armand Argand (1755-1803), who, aside from the specific heating oil employed, was nothing other than the gas lamp of our great-grandparents with a glass chimney and a circular fuse, the two modifications made by Argand, who revolutionized the illumination system of the time. In 1800, the French clockmaker Guillaume Carcel (1750-1812) added a new clockwork system to keep the height of the fuse constant, obtaining the lamp named after him.

With a somewhat modified Argand lamp and his own photometer, Rumford measured many coefficients of absorption with the chief aim of making public and private illumination more economical.

THERMOMETRY AND CALORIMETRY

7.16 Thermometers

At the end of the 18th century, thermology, which at the century's beginning had been a few scattered and unconnected ideas, had attained scientific dignity: it had led to the discovery and study of numerous thermal phenomena, the construction of instruments, the creation of consistent conventions of measure, and a non-contradictory explanation of phase transitions, which had stumped previous physical theories. Nevertheless, confusion still abounded regarding temperature, the thermometer, and thermometric scales (Fig. 7.5).

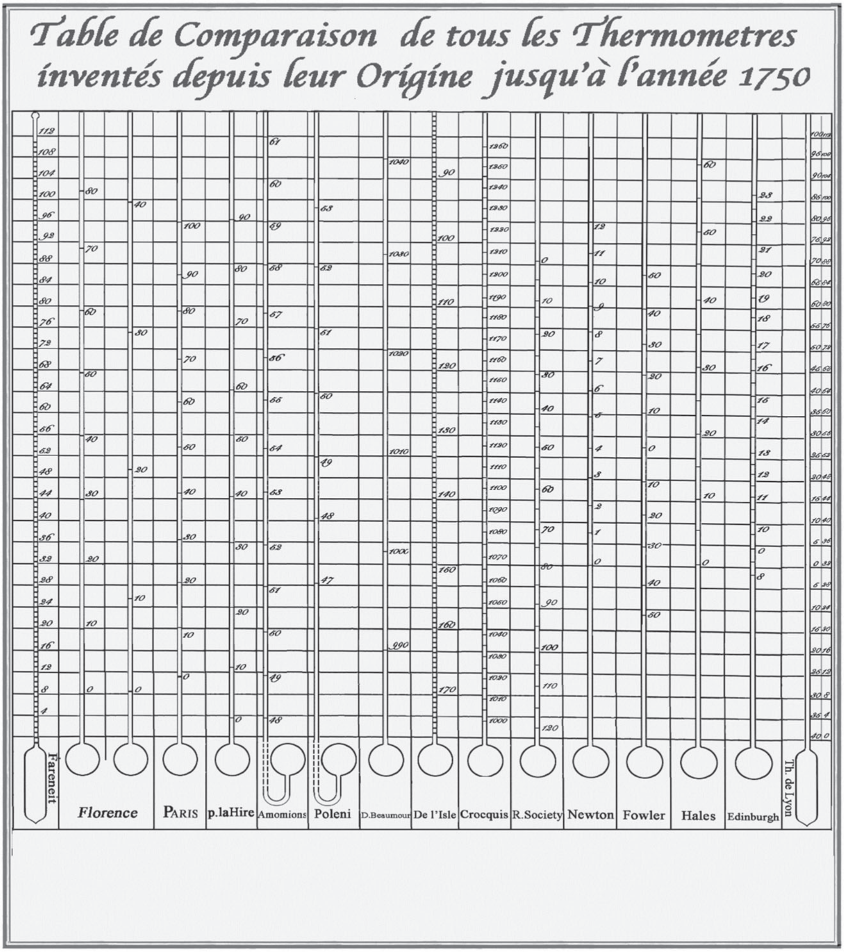

Figure 7.5 - Table comparing thermometric scales, according to De Mairan (*Dissertation sur la glace*, Paris 1749). *Source*: M. Pouillet, *Éléments de physique expérimentale et de météorologie,* Paris 1853.

Out of all physical quantities, the concept of temperature was among the most difficult to consolidate. In consequence, the history of the thermometer was among the most long and tumultuous. It is easy to see why: common experience tends to confuse measures of intensity and quantity, that is, in this specific case, those of temperature and heat exchanged. Once the distinction between the two was made clear, new difficulties arose due to a lack of knowledge of the thermological laws necessary to construct the instrument, the study of which, moreover, required the very measurement of temperatures. It was a vicious cycle that trapped research for the entire century and beyond.

To emphasize the century-long research effort, we think it instructive to recount a few particulars of the long history of the thermometer: an instrument so familiar and simple that one might almost believe that it was born perfect.

Florentine thermometers (§ 5.24), which became famous in all of Europe, had one fundamental flaw: they were not comparable to each other, not only because fixed instructions for their construction did not exist, but also because their limits were too subjective and described by vague expressions, like the temperature of "the coldest winter day in Florence". The need for meaningful thermometric indications was so great that Colbert, the famed minister of Louis XIV, devised a plan to build numerous identical thermometers in Paris and bring them to the ends of the Earth to conduct comparative observations.

This plan was not enacted, however, perhaps because physicists warned Colbert that even thermometers built in this way would not have been in agreement due to the imperfect calibration of their tubes, one of the failings

Renaldi's proposal, though seemingly of comparable importance to Joseph Fraunhofer's discovery (1815) of black lines in the solar spectrum (exact reference points for the measurement of indices of refraction), was not well-known or enacted at the time, as evidenced by the fact that in 1702 Guillaume Amontons (1663-1703) complained that the available thermometric scales were arbitrary, and a 1714 note in "Acta eruditorum" extolled the advantages of thermometers that could give concordant readings and announced that Daniel Gabriel Fahrenheit had been able to build them with a procedure "about which he remains reticent for family reasons [*ob rationes domesticas*]."[366] The brief note only mentions that Fahrenheit replaced spherical bulbs with cylindrical ones, used wine spirits, and divided the scale into 26 equivalent parts: the second part indicated "intense cold" while the 24th "insufferable heat [*aestum*]". Wolff examined two such thermometers for many days and had found their readings to be in accord with a few "slight differences". There were therefore no fixed points in Fahrenheit's scale either, but we shall soon see that the use of fixed points would not have completely resolved the problem of comparable thermometers.

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[366] G. Amontons, *Relatio de novi barometrorum et thermometrorum concordantium*, in "Acta eruditorum", 1715, p. 380.

During the construction of a hot air windmill, Amontons realized that air pressure increases by a constant quantity for the same "degree of heat" and that the increase is greater when the initial pressure is greater. He then applied this property to the construction of a thermometer with one fixed point, that of boiling water, believing himself the first to have realized its constancy. Amontons' thermometer, which is described in the cited note, was made up of a bulb 3,25 inches (about 8.8 cm) in diameter attached to a U-shaped tube with an internal diameter of about 0.1 cm and one side much longer than the other. With an *ad hoc* device and a complicated procedure, mercury was added into the bulb until the room temperature air inside it held up a 27 inches tall column of mercury, or in other terms was at a pressure of about two atmospheres. The bulb was immersed in boiling water and the column of mercury rose to 45 inches: this maximum level was marked by a line, beginning a graduated scale of lines an inch apart going down. Amontons was aware that the volume of air contained varied during use, but he experimentally verified that as long the volume of the tube was small compared to the volume of the bulb, it did not have a detectable effect on the measurement.

In a later 1703 paper, Amontons, having convinced himself of the difficulty in building and using his thermometer, proposed to use it instead as a standard to calibrate other instruments. Furthermore, noting that the rising mercury level is due to an increase in the elasticity of the air due to heating, he observed: "It follows that the extreme cold of this thermometer is that which would reduce air to not sustaining any pressure, a much more considerable degree of cold than that which we call very cold because experiment has told us that if the heat of boiling water allows the elasticity of air to support a pressure of 73 inches of mercury, then the degree of heat that remains in the air when water freezes is still rather large to allow it to support a pressure equal to 51½ inches: this deserves particular attention."[367]

This passage clearly refers to absolute zero, and explains its significance using the same type of argument employed today in physics courses. Using Amontons' experimental data, today we could write

$$73 = 51.5(1 + 100\beta)$$

and deduce

[367] G. Amontons, *Le thermomètre réduit à une mesure fixe et certaine, et le moyen d'y rapporter les observations faites avec des anciens thermomètres,* in « Histoire de l'Académie royal des sciences. Memoires », 1703, pp.52-53.

$$\beta = \frac{1}{239.5}$$

and thus set absolute zero at -239.5 °C. This calculation can be used to evaluate the accuracy of Amontons' experimentation. Seventy-five years later, Lambert revisited the idea of absolute zero, or as he called it, "absolute cold", setting it (as one can calculate today from his data) at -266.6 °C.[368]

Amontons' thermometer was not at all popular: it was unwieldy (having a height slightly above 1.4 m) and difficult to transport because any tilts and oscillations in the apparatus made air escape from its bulb, necessitating a new and painful re-filling. Guided by Amontons himself, an able artisan was able to build a few models, fated to remain on the shelves of collectors, "yet the public took little part in this invention," ensured Abbé Nollet.

The difficulty in building the instrument primarily lay in its filling, which was so difficult that Fahrenheit, a builder of meteorological apparatuses, was unable to put the thermometer together despite his numerous attempts, which nevertheless gave him the idea of using mercury as a thermometric substance. Thus, were born the famous mercury thermometers of the first decades of the century, described ten years after they were put on the market in a series of brief Latin notes in which Fahrenheit made sure to go into as little detail as possible, purportedly *ob rationes domesticas,* but in reality, to limit commercial competition. In any case, it is known that there were two fabrication secrets for Fahrenheit's thermometers: the pre-emptive purification of the mercury and the boiling of the liquid inside the thermometer as the instrument was closed. Through this technique, almost zero trace of air remained inside the thermometer. The scale was organized with the zero corresponding to the temperature of a mixture of ice with ammonia or sodium salts, for which the relative proportions were not revealed, and with 96 corresponding to the temperature of the thermometer when placed in the mouth or under the armpit of a healthy person.[369] This interval was divided into 96 equal parts, and when the thermometer was placed in a mixture of water and ice, the column of mercury reached the part labelled by the number 32:[370] it therefore appears that the melting point of ice was only a verification point. The lesser

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368 J. H. Lambert, *Pyrométrie*, Berlin 1779, p. 29.

369 *Experimenta circa gradum caloris liquorum nonnullorum ebullentium instituta a Daniele Gabr. Fahrenheit*, in "Philosophical Transactions", 33, 1724-25, pp. 1-3.

370 *Experimentia et observationes de congelatione aquae in vacuo factae a D.G. Fahrenheit*, *ibid.*, pp. 78-84.

importance given to the melting point of ice was perhaps due to Faraday's discovery of supercooling, which he described in the same paper.

Using his thermometers, Fahrenheit observed that other liquids beside water had a constant boiling temperature and measured it to be 176 °F (80 °C) for alcohol, 242 °F (77.3 °C) for nitric acid, and 600 °F (175 °C) for mercury. Of much more importance than these data and others like them, which are difficult to compare to modern ones because we do not know the purity of the substances used by Fahrenheit, was the observation that the boiling temperature of water decreases with decreasing external pressure. This discovery led Fahrenheit to propose a new type of barometer essentially made up of long tube thermometer that, when placed inside boiling water, indicates pressure instead of temperature with the level of the mercury inside it,[371] basically the first hypsometer.

Because of their excellent and invariable construction, Fahrenheit's thermometers spread rapidly throughout Britain, the Netherlands, and Germany; but were mostly unknown in France and Italy when René-Antoine de Réaumur read two papers to the Académie des sciences of Paris in 1730-31 on the construction of thermometers and how to make their measurements comparable. He employed large thermometers (the diameter of the cylindrical bulb reached 4.5 inches, or 12.18 cm; the diameter of the tube reached 0.68 cm; and the instrument's total height reached 5 feet, or 1.62 m), facilitating their use, and used rectified alcohol diluted with 20% water by volume as the thermometric solution. Having introduced a certain quantity of water at the "artificial" freezing temperature into the instrument, he marked its level as zero, and with a series of clever operations, he divided the tube into parts whose volume was one-thousandth of the volume of the liquid in the bulb. Réaumur's scale therefore had only one fixed point. To determine the maximum temperature that the instrument could measure, before sealing the end of the instrument Réaumur placed it in boiling water and made sure that the liquid (alcohol) always reached the level marked 80. It is clear that this point depends on too many details of the instrument's construction: the quality of the alcohol and the glass of the thermometer, the ration between the volume of the bulb and the volume of the tube, etc. However, its builders simplified things, using the usual two fixed points and dividing the interval in 80 equal parts. The prohibitive dimensions of Réaumur's thermometer were reduced by Nollet on Réaumur's own advice: the scale of these reduced thermometers was drawn by comparing it with a Réaumur thermometer. Réaumur's thermometer, which spread in large part due to Nollet, who became an authority in experimental physics because of his theatrical lessons, attended by the intelligentsia and distinguished circles

[371] *Barometri novi descriptio a D.G. Fahrenheit, ibid*., pp. 179-89.

of Paris, appeared very functional and became widely used, especially in France and Italy. Thermometric scales proliferated in the eighteenth century, with Lambert writing of 19 different ones. The scale from 100-0, proposed by Anders Cèlsius (1701-1744) and inverted by another astronomer, Martin Strömer (1707-1770) in 1750, took hold only in the 19^{th} century with the introduction of the decimal system.

We will return to the uncertainty regarding “fixed points” in the next section. For now, we limit ourselves to the observation that though towards the end of the century, through homogenization in the fabrication of thermometers, the thermometric measurements obtained were mostly comparable, the situation was still rather confused for scientific problems. Jean-André de Luc (1727-1817), a physicist from Geneva who lived in Britain and Germany for a long time, wrote in 1784: “The invention of the thermometer, nice in and of itself, improved by the greatest physicists, is still very far from providing us the advantages that we could expect from it. All those who have advanced this subject will have noticed that a great number of observations remain useless because of the difference between thermometers and their defects.”[372]

7.17 The study of thermometric liquids

In the course of the 17^{th} century, many thermometric substances were used: alcohol, mercury, flaxseed oil, chamomile oil, olive oil, water, and other liquids. Physicists thus were able to discover a new phenomenon: that the uniform dilation of thermometric substances, which had been tacitly assumed in devising every scale, was an unfounded assumption because thermometers containing different thermometric substances gave entirely conflicting measurements. de Luc built six geometrically identical thermometers calibrated using the usual fixed points and filled with different liquids: mercury, olive oil, chamomile oil, wine spirit, water saturated with sodium chloride, and ordinary water. He immersed all of them in hot water as it was cooling and noticed the nearly constant disagreement in their measurements. When, for example, the thermometer with mercury read 40 °R, the other thermometers read 39.2, 38.6, 38.4, 36.3, and 20.5, respectively. If one supposes that any one of the liquids examined (aside from water) expands uniformly with temperature, the dilation of the others appears to be non-uniform. Only for water can it be affirmed with certainty that its dilation is nonuniform, since near 4 °R (according to de Luc) a reduction of temperature leads to an increase in volume.

[372] J.-A. de Luc, *Recherches sur les modifications de l’atmosphère*, Paris 1784, Vol. 2, p. 54.

How, therefore, was one supposed to measure equal changes in temperature if it was thermometers themselves that should have indicated them? There was no shortage of projects, both theoretically faulty and difficult to execute: for example, heating the water in a container of predetermined dimensions using identical wicks and considering the changes in temperature proportional to the number of wicks lit for the same length of time.

De Luc, desperate to get to the bottom of the issue through such projects, suggested that the matter could at least be put in order by making the thermometric measurements comparable to each other through a common convention: taking a particular thermometer with well-defined geometric characteristics as the standard, and filling it with a substance that, at least in an approximative sense, expands linearly with temperature. de Luc became one of the leading experts on thermometry of the time almost by accident. The main goal of his research had been the measurement of altitude using a barometer: this research had led him to deal with the thermometer because of the well-known influence that temperature has on barometric readings. Furthermore, Charles-Marie de la Condamine and Lalande, amazed and worried because of the unreliable thermometric measurements obtained at different latitudes of the Earth, exhorted him to dedicate himself to an in-depth study of the instrument. This research programme lasted over fifteen years and made several new contributions to physics, many of which occupy almost the entire second volume of the cited *Recherches.*

The building of the standard prototype thermometer planned by de Luc required searching for the most suitable thermometric liquid. Discussing his experiments and those of others, de Luc believed that he could establish the following propositions: 1) the decreases in the volume of liquids which increase in volume when condensing, are not proportional to the decreases in temperature; 2) the expansions of volatile liquids are not proportional to increases in temperature. Because mercury neither increases in volume when it solidifies nor is volatile, the two propositions led de Luc to conclude that, out of all the thermometric liquids, mercury is the closest to the ideal substance. Though his conclusion is a bit stretched, certain parts of his experimentation are excellent, like the study of the anomalous dilation of water at temperatures slightly above its freezing point, a phenomenon that had been already observed at the time and carefully described in 1749 by de Mairan in his *Dissertation sur la glace*, as well as the more regular expansive behaviour of saturated saline solutions, water-alcohol mixtures, and olive oil.

However, de Luc's impassioned battle for the use of mercury highlights the serious errors inherent in the thermometric measures of the time. In

1759, for example, Josue Adam Braun (1712-1768), professor of philosophy and member of the academy of sciences of Saint Petersburg, succeeded in solidifying almost all the mercury in a thermometer by immersing it in a mixture of snow and nitric acid. What temperature did he reach when he observed this effect? Braun claimed it was at 530 degrees on Joseph-Nicolas Delisle's (1688-1768) scale, which de Luc held equal to -202.4 °R (-253 °C) and others held equal to -100 °R (-125 °C): there is a rather large difference between these two temperatures and also when compared to the modern measurement (-38.8 °C); a smaller disagreement is found for the boiling temperature, which Braun measured to be 300.8 °R (376 °C) and modern measurements place at 356.66 °C.

After a long critical examination, de Luc concluded: "Out of all the liquids, mercury is the one which comes closer in measuring equal differences in temperatures with equal differences in its volume."[373] Yet the prudence of this conclusion was abandoned by later writers, who allowed themselves to affirm that mercury expands uniformly with temperature: the 19th century, therefore, would have to re-examine the issue[374].

Once the liquid was chosen, the "fixed points" had to be defined in an exact way, though they were anything but fixed, primarily for three reasons: salts were dissolved in ordinary water at unknown concentrations; the "artificial" freezing suggested by Réaumur was used, in which water was frozen using refrigerating mixtures to glean the exact moment in which it froze, but sometimes supercooling would confuse the experimenter; and atmospheric pressure was rarely taken into account. de Luc partially avoided these problems by prescribing the use of ice for freezing and fixing the normal pressure of mercury at 27 Parisian inches, along with providing a correcting rule in the case of different external pressure.

7.18 Dilatometers and pyrometers

The change in volume of bodies due to a change in their temperature had been observed by the academics of the Cimento (§ 5.24). In the comments and additions that accompanied his Latin translation of the *Saggi* (1731), Pieter van Musschenbroek, a Dutch physicist and one of the leading experimenters of the first half of the century, proposed a quantitative study of the phenomenon through a simple instrument that he called the "pyrometer" and that in fact was a dilatometer: a metallic rod fixed on one end rests at its other end on the arm of an angular level, which moves a pin

[373] *Ibid.,* pp. 181-82.

[374] Chapter 2 in M. Gliozzi, *A History of Physics over the Last Two Centuries.* Cambridge Scholars Publishing, in press 2022.

attached to a spring in front of a marked quadrant. When in use, the rod is heated by a series of small alcohol lamps arranged in a straight line. This apparatus spread rapidly throughout European physics laboratories, initiating or accelerating research on dilatometry, a practice undertaken primarily by clockmakers.

Even before Musschenbroek, in a paper that appeared in the "Philosophical Transactions" of London in 1726, George Graham (1675-1751), a British mechanic, proposed to compensate the variations in length of a pendulum by attaching not a sphere or metallic lens to the rod but rather a metallic cylinder almost completely filled with mercury. The apparatus was planned such that the lengthening of the rod would be compensated by the variations in the level of mercury in the cylinder, leaving the centre of oscillation of the system fixed. A dozen years later, taking advantage of the first quantitative measurement of the linear expansion of metals, Julien Le Roy (1717-1785), clockmaker for the King in Paris, replaced Graham's compensation mechanism with the well-known system of differently dilating bars. By the second half of the century, these techniques reduced the disparity between a clock during the coldest and hottest days of the year from 20 seconds to 2 seconds.

Towards the end of the century, knowledge of the thermal expansion of solids found a new application in metallic thermometers. The first to have this idea is thought to have been Jacques-Alexandre Charles (1746-1823), a scientist very skilled at planning and executing spectacular experiments, who created a thermometer using a two-sided iron-brass sheet whose deformations were transmitted through a lever to a moveable pin in front of a marked scale. Many of these thermometers were built by the clockmaker Antide Janvier (1751-1835), who placed them in his famous pendulums. In reality, though, the two-metal sheet had already been introduced in 1765 for the compensation of pocket-watches by John Harrison (1693-1776).

In any case, Charles' metallic thermometer, which too closely resembled Musschenbroek's pyrometer, was of little to no use, and thus had limited distribution. Perhaps its only contribution was that it inspired the creation of Abraham-Louis Fréguet's much more useful metallic thermometer in 1817, in which the needle is hung from a long three-layer helix.

That which we would properly call a pyrometer today was first described in 1782 by Josiah Wedgwood (1730-1795): it consisted of cubic blocks of clay placed in the downhill grooves of a porcelain plate, which was placed in an oven. As the temperature of the oven was increased, the clay blocks became deformed and slid down the grooves, which had a thermometric scale engraved at their ends.

More difficult was the comparison of thermal expansions of liquids. Borelli, in chapter 105 of *De vi percussionis* (1777), described an experiment that is still repeated today in physics classes: a large bulb with a long and thin neck contains a liquid that lies at a level marked on the neck; the bulb is then immersed in hot water and one first observes a lowering in the level of the liquid and later a rise past its original level (vice versa, if the bulb is immersed in freezing water, the level rises immediately). Borelli explained the effect by supposing that first the capacity of the contained increases, followed by an increase in the volume of the liquid that exceeds the increase in the container's volume. Thus, the idea of apparent and absolute dilation of a liquid was tacitly introduced.

Isaac Vossius (1618-1689), however, in chapter 11 of the treatise *De motu marium et ventorum* (1663), held that, "when approaching heat", liquids first decrease in volume and then increase in volume by a greater amount. In 1695, Amontons, still unaware of Borelli's experiment, conducted an almost identical experiment and provided the same explanation.[375] In 1700, Etienne-François Geoffroy (1672-1731) upheld Vossius' explanation at the Académie des sciences of Paris. In 1705, though, still at the Académie, Amontons reasserted his conviction based on flimsy theoretical considerations and questionable experimental evidence. He first acutely observed that the hypothesis that the container dilates is plausible if one shows different apparent deviations for different liquids in the same container, thus Borelli's experiment, which in the meantime had come to his knowledge, "proves nothing" (much like Amontons' own 1695 experiment). Then, he proposed a new experimental demonstration: a tube open at both ends is placed through the cork of a bottle, and water is added until its level reaches a few inches above the cork. If the bottle is held in a person's hands, the water level in the tube immediately drops by a few lines, but if the bottle is entirely filled with water, then the level drops by more than ten lines. The experiment does not appear very convincing, however, because Amontons does not account for the increase in the pressure of the air inside the bottle due to heating.

The disagreement went on for a good part of the century. To avoid getting into the thick of things, Nollet and many of his contemporaries only dealt with apparent dilation, with the precaution of always comparing dilations between the fixed points, successively filling the same thermometer (or a different, but identical one) with different liquids. For the entire century, the problem remained intertwined with the temperature

[375] G. Amontons, *Remarques et expériences physiques* [sic] *sur la construction d'une nouvelle clepsidre. Sur les baromètres, termomètres et higromètres,* Paris 1695, pp. 52.55.

correction for the barometer, which some did not think necessary (de La Hire, Du Fay) while others did (de Luc, Antonio Lorgna).[376] A truly important step forward was taken in the first decades of the 19th century by Dulong and Petit with the measurement of the absolute dilation of mercury: we will discuss this later, along with advances in the study of gas expansion.

7.19 The nature of heat and its measurement

For the entire first half of the eighteenth century, physics concerned itself with the construction and improvement of thermometers, convinced that they measured "degrees of heat", an expression that has unfortunately somewhat survived and made its way into our ordinary lexicon. The majority of physicists combined both thermal sensations and heat into a single vague concept. Even if a physicist (like Klingenstierna) did notice the distinction between temperature and heat, he did not consider it his duty to deal with heat (in the modern sense), which had been traditionally left for chemists to study. On the other hand, chemists dealt with the nature of heat, not its measurement, which was confused with the measurement of temperature. There were essentially two theories on the matter, though subject to numerous variations, which had been handed down from classical antiquity and revived during the Renaissance: kinetic theory, advocated by both Roger Bacon and Kepler; and the elemental theory, which associated heat to the element of "fire".

Kinetic theory, which held that heat is a way of being or an accidental property (in the Aristotelian sense) of matter, was fairly popular in the first half of the eighteenth century, so much so that in 1738, Euler won the first prize in a competition organized by the Académie des sciences of Paris with the thesis that "heat consists of a certain motion of the small particles in bodies.[377] That same year, his colleague Daniel I Bernoulli made the century-old intuition precise in a classic work.[378] According to Bernoulli, the particles of "elastic fluids", or as we would call them, gases, rapidly move in all directions. If a certain number of such particles is found inside an empty vertical cylinder closed at the top by a moveable diaphragm, the impulses transmitted to the diaphragm from the particles that strike it compensate its weight: the diaphragm rises if its weight decreases and falls

[376] A detailed review of this chapter of history can be found in Knowles Middleton's *The History of the Barometer*, cit., pp. 176-78.
[377] *Recueil des pièces qui ont remporté les prix de l'Académie royale des sciences*, Paris 1752, Vol. 4, p. 130.
[378] D. Bernoulli, *Hydrodynamica, sive de viribus motionibus fluidorum commentarii*, Argentorati 1738, pp. 200-03.

if it increases. In the second cases, however, the elastic force of the enclosed gas increases, both because the number of particles per volume occupied becomes greater, and because each particle strikes the diaphragm more frequently. Bernoulli showed that, assuming these hypotheses, the spaces occupied by the elastic fluid are inversely proportional to the elastic force of the gas: this is Boyle's law. Furthermore, according to Bernoulli, increased temperature increases the velocity of the particles and the expansive force of the gas is proportional to the square of the increase in velocity, since with increasing temperature the number of collisions increase as well as their individual intensity. Bernoulli tested his theory using Amontons' thermometer (§ 7.16), which he described in minute detail without mentioning its inventor, an old family habit.

Bernoulli's theory did not specifically deal with the issue of the nature of heat: it was simply a kinetic theory of gases in which heat played a part, with a mechanism that was still unclear, as an accelerator of gas molecules. It was Lomonosov who, in the midst of a dispute with his colleague at the academy of Saint Petersburg, Georg Richmann, distinguished the two issues, which he addressed in two consecutive papers.

According to Lomonosov, for each solid, liquid, or gaseous body, heat consists of the internal motion of matter, that is, the motion of its smallest particle components. However, out of the three types of motion that a particle can undergo – translational, oscillatory, and rotational – which is the one that strictly generates heat? Lomonosov ruled out the first to kinds of motion for reasons that may appear insufficient to the modern reader, but seemed reasonable at his time (how could the quivering of particles in a compact, solid body go unnoticed?), and was therefore led to affirm that heat consists in the rotational motion of the smallest constituent particles of a body. The production of heat through friction (a phenomenon that he critically invoked to support his mechanical theory), the propagation of heat, and phase transitions are then mechanically explained as the transmission of rotational motion between particles in contact at their surfaces. This theory also predicts a lower limit of "heat", that is, of temperature, but not an upper limit, "because no velocity can be chosen so large that a larger one cannot be imagined. This can also legitimately apply to calorific motion; so like motion there cannot exist a highest and last degree of heat. On the other hand, motion can be reduced until a body is at rest and there can be no further reduction in its motion. Therefore, the highest and last degree of cold that consists in the absence of rotational motion of the particles can exist."[379]

[379] M. V. Lomonosov, *Meditationes de caloris et frigoris causa*, in "Novi commentarii Academiae scientiarum petropolitanae", I, 1750, p. 220.

Lomonosov's theory, like any other analogous theoretical approach at the time, had a glaring shortcoming, which explains both its tepid welcome and its rapid fall into oblivion: it was not quantitative, aside from still not clearly distinguishing between quantity and intensity of heat or temperature, which were both indicated (by an explicit declaration of the scientist) by the term *calor*.

In a later paper, Lomonosov attempted to explain the elastic force of air and all the other exhalations (of which some are inopportunely called gases according to him) without resorting to a peculiar fluid, "as was the custom of the century". Lomonosov constructed his theory by detailing the nature and form of the smallest particles of air. In his view, these are corporeal, extended, highly solid, and cannot be deformed: they are, therefore, genuine *atoms*, rounded spheres with a spiked surface. The atoms of air are not typically directly in contact with each other, but two nearby atoms can come into contact through Newtonian attraction, wedge their "teeth" into each other, and repel because of their rotational motion, repeating this process with other nearby atoms "such that, continually repelled by these frequent and reciprocal collisions, they tend to spread out."[380] Heat magnifies the rotational motion of atoms and therefore increases the reciprocal repulsion when they come in contact, that is, the elastic force of air. From his theory, one can also deduce that the atmosphere cannot extend to infinity and that its density must decrease as the distance from the Earth increases.

Lomonosov's theoretical framework thus explained all the phenomena observed by experimenters save one: Boyle's law, which was instead explained by Bernoulli's theory. Richmann brought this to his attention after the reading of his manuscript at the meeting of the academy of sciences of Saint Petersburg. Disturbed, Lomonosov promised that he would tie up the loose ends. Indeed, he resolved the issue in the winter of 1749, detailing his work in a *Supplementum ad meditationes de vi aeris elastica*.[381] His demonstration was essentially based on the observation that the frequency of collisions between atoms is inversely proportional to interatomic distance. This consideration even allowed Lomonosov to explain the deviation from Boyle's law in the behaviour of air at pressures above four atmospheres that had been experimentally observed by Musschenbroek.

In conclusion, Lomonsov, without a doubt inspired by Bernoulli's own theory, of which he was well aware, fundamentally accepted Bernoullian elastic collisions as a secondary phenomenon resulting from Newtonian attraction and the rotational motion of atoms, a more elaborate theory that was more difficult to understand, but had the advantage over Bernoulli's

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[380] M. V. Lomonosov, *Tentamen theoriae de vi aeris elastics*, *ibid.*, p. 236.
[381] *Ibid.*, pp. 305-12.

theory that it pointed to heat as the mechanism that permits us to perceive atomic motion.

Both Bernoulli's theory and Lomonosov's were quickly forgotten. It is said that Bernoulli's theory appeared unfounded because when it was applied by Bernoulli to find an altimetric formula, it led him to a result that was rather far from the experimental value. However, we do not think that it was this setback that discredited mechanical theories of heat and kinetic theories of gases in the 18th century (Bernoulli's theory was later extended in a later paper by him and his father which was given an award in 1746 by the Académie of Paris). Rather, it was a deeper reason: the theories were premature for their time. This is an accepted historical phenomenon: when a theory is too far ahead of its time, it is easily forgotten.

For the entirety of the 18th century, the mechanical theory coexisted with the fluid theory, which gradually acquired more support as the century went on: halfway through the century, Nollet, exaggerating, affirmed in his physics lessons that "kinetic theory has almost no followers left." Galileo had previously come close to the fluid theory, as we discussed (§ 4.4), when he hypothesized "atoms of fire" or "ignicles" that penetrate bodies, in particular fluids, and cause their expansion.[382] The fluid theory was based on a deeply intuitive hypothesis replete with simple analogical connections that in the first half of the century went hand in hand with phlogiston theory, developed and disseminated by Georg Ernst Stahl (1660-1734) starting in 1700. Phlogiston was conceived of as a special fluid present in combustible objects and metals that, during the act of combustion or calcination (that is, oxidation), escapes from the body containing it in the form of heat. Heat reduction, on the other hand, occurs when the phlogiston that had been removed from a body is returned to it. In short, the process of oxidation was described almost completely opposite to the explanation provided by modern chemistry. To the few who had experimentally observed that calcinated metal weighs more than the original sample (scales were rarely used in experiments at the time), proponents of phlogiston replied that this observation was completely logical, as phlogiston has negative weight and thus as it escapes a body during calcination, the body gains weight. In reality, there were a few (like Beccaria in Italy) who attributed the increase in weight to the combination of the metal with other bodies, like air, with which it comes in contact. The majority, however, subscribed to the

[382] In *Risposta alle opposizioni del S. Lodovico delle Colombe* (in Galilei, *Le opere* cit. Vol. 4, p. 451) by Castelli, which Galileo helped edit, he proposes the experiment of the glass ball with "a long and rather thin neck" half-filled with water and placed over a fire, where the water level rises "as atoms of fire multiply in the water" (*Ibid.*, p. 654).

convenient hypothesis of negative-weight phlogiston, which made the theory complicated and even, from a modern perspective, strange and contradictory. Nevertheless, many 18th century scientists and philosophers considered it simple and consistent: Euler, Cavendish, Priestley, Laplace, Volta, and Kant all fully accepted it.

Phlogiston is not heat, but when it escapes from bodies it produces heat. The theory was therefore not identical to the elemental theory of heat. It was only related to it, and not closely enough to share the same fate. Lavoisier brought an end to phlogiston theory but strengthened the elemental theory of heat, including *caloric* (and *lumen*) among the elements.

In this scientific environment, shortly before the century reached its halfway point, the first calorimetric experiments began. Their first traces can be found in the treatise on chemistry by the Dutch scientist Hermann Boerhaave (1668-1738), in which he teaches that when mixed, two equal masses of water at different temperatures acquire the same temperature, the average of their original ones, indicating, according to him, that the heat of the colder water "disappears" in mixing.[383] Boerhaave's formula was challenged by a relatively unknown German physicist and longtime resident of Saint Petersburg, Georg Wolfgang Krafft (1701-1754), who set out to experimentally determine the temperatures of water mixtures at different temperatures. He believed that he could summarize his experimental results in the following formula: if *a* and *b* are the two masses of water, and *m* and *n* are their respective temperatures, assuming $m > n$, the temperature of the mixture is given by[384]

$$\frac{11am + 8bn}{11a + 8b}.$$

Krafft's formula did not appear acceptable to Georg Wilhelm Richmann (1711-1753), and Estonian physicist working in Saint Petersburg, based on his fluidistic conception of heat: "I reflected on the heat of a fluid under certain climactic conditions," wrote Richmann, "distributed equally throughout the entire fluid mass, and I realized that if the same quantity were distributed in a mass twice, three times, or four times as large, the degree of heat generated should be one half, one third, one fourth, etc. of

[383] H. Boerhaave, *Elementa chemiae*, Lugduni Batavorum 1732, Vol. I, exper. XX.
[384] Reported by G.W. Richmann, *Formulae pro gradu excessus caloris supra gradum caloris mixti ex nive et sale amoniaco post miscelam duarum massarum aquearum diverso gradu calidarum confirmatio per experimenta*, in "Novi commentarii Academiae scientiarum imperialis petropolitanae", I, 1750, p. 171.

the initial degree, and in general inversely proportional to the mass throughout which it is distributed."[385]

This idea led him to show that if a and b are two masses of the same fluid at respective temperatures m and n, the temperature of their mixture is

$$\frac{am + bn}{a + b},$$

a formula that can also be generalized to multiple masses of the same liquid.

One can see that if two similar containers of cross-sectional areas a and b containing the same liquid, at respective levels m and n, are connected on the same horizontal plane, the level of the liquid in the two containers becomes the value given by Richmann's formula. In short, this hydraulic analogy led to the first calorimetric formula, which can still be used today as a first approximation. Using water, Richmann observed that experimental results were closer to the theoretical predictions calculated using his formula than the ones calculated with Krafft's.

On the contrary, the experimental results were so close to the theoretical predictions that Richmann was led to believe his general formula valid for any mixture, even one of equal parts boiling water and ice mixed with ammonia salts: that the final temperature in this case was significantly lower than the theoretical one he attributed to heating of the thermometer and the sides of the vase, as well as a loss of heat during the experiment. The scientist made no mention of either a heat of fusion or a difference in the specific heat of bodies, clearly indicating that he had not yet grasped such concepts.

7.20 Phase transitions

Richmann's important experiments caused less sensation that the equally important ones conducted in 1757 by Joseph Black (1728-1799) on fusion and evaporation. The British scientist set out to extend Richmann's formula to substances besides water. Like Richmann, he supposed the existence of a specific fluid responsible for variations in temperature that can pass from one body to another without changing in quantity. When a cold body is immersed in hot water, it heats up because some of the heat in

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[385] G. W. Richmann, *De quantitate caloris, quae post miscelam fluidorum, certo gradu calidorum, oriri debet, cogitationes, ibid.*, pp. 151-52. In the lengthy title, the *quantitate caloris* does not refer to our modern quantity of heat bur rather to our temperature. Likewise, in the cited passage, which we have tried to translate with maximum faithfulness to its original intent, one notes some confusion between the two concepts, chiefly due to linguistic difficulties.

the water moves into the body: from this fundamental idea, Black was led to formulate rules of measurement for heat and the first measurement technique, the method of mixtures.

Until Black, it was believed that one only had to bring a solid to its melting temperature for the attractive forces between its molecules to weaken enough to transform the body into a liquid state. Black brought an end to this theory with a pivotal experiment: he combined a mass of ice at 32 °F with an equal mass of water at gradually increasing temperature until he obtained the melting of the entire mass of ice, and the temperature of the combined mixture remained at 32 °F. In this way, he found that the necessary temperature of the added hot water was 172 °F (approximately 58 °C), or rather, as we would say today, that the heat of fusion of ice is about 76 cal/g °C. This experimental result disproved the fundamental tenet of the elemental theory of heat: the constancy of the quantity of heat in thermal phenomena. In the experiment, the heat imparted by the hot water to the ice seemed to disappear, as it could not be found in the form of a temperature increase of the ice and was therefore not measurable using a thermometer. How could this effect be explained by the theory? How could the ledger be balanced? Black did not hesitate: he corrected the imbalance by inventing "hidden heat" or, to use a Latin expression, "latent heat".[386] During melting, some of the heat becomes fixed in the molecules of the body and is no longer measurable by a thermometer, which can only detect "free heat". The existence of latent heat in water seemed evident to De Luc based on the inverse effect that he was able to obtain: having obtained supercooled water at 14 °F (-10 °C), he placed a small piece of ice in it and the water immediately heated back up to 32 °F and froze.[387] How could one say that latent heat was invented if it could be seen reappearing in the reverse process? It therefore seemed obvious that water is a deeply intertwined union, a chemical combination of ice and heat.

Owing to the idea of latent heat, other phenomena were soon given similar explanations. Georg Lichtenberg attempted to explain the increase in fluidity (measured by the number of droplets obtained from the same quantity of liquid) with increasing temperature through an increase in latent

[386] J.-A. De Luc, *Idées sur la métrologie*, Londres 1786, Vol. I, p. 178. The calorimetric writings of Black were published posthumously in *Lectures upon the Elements of Chemistry*, Edinburgh 1809, 2 volumes. His ideas, however, spread throughout Europe through various channels, the most important of which were De Luc and James Watt, assistant and friend of Black. In this case too, De Luc assures the reader that he learned of Black's experiments from Watt, who had also repeated them for him.

[387] De Luc, *Idées sur la métrologie* cit., p. 176.

heat, that is, stipulating that because some heat is used to increase fluidity, it becomes undetectable by the thermometer.[388] Laplace and Lavoisier, based on the fact that one effect of heat is expansion, advanced the hypothesis that in thermal dilation part of the heat is absorbed and becomes undetectable to the thermometer, and thus that it is likely that specific heat increases with temperature. Perhaps an instinctive analogy pushed Laplace and Lavoisier to the following hypothesis: like a change in the level of a liquid inside a container with elastic walls changes its cross-section and thus its capacity, a change in temperature causes a change in the heat capacity of a body. On the other hand, perhaps the two scientists simply intuited that thermal dilation must occur at the expense of something, which would bring their idea much closer to a modern viewpoint.

When heat is not in a chemical combination, that is, it is not latent, it is "free" and exerts influence on a thermometer. It follows, to use De Luc's words, that "every time that the total space occupied by a certain quantity of free fire suddenly increases or decreases, which without a doubt often occurs without us becoming aware of it, there are necessarily changes in the temperature of the affected substances." For example, a piece of iron heated until it glows slightly red and then hammered on an anvil becomes incandescent: this is one of the phenomena invoked by De Luc to support the fluid theory, which represented the free heat contained inside bodies like orange juice inside oranges.

Let us move on to evaporation. Of particular success was the theory put out in 1751 by Charles Le Roy (1726-1779): evaporation, according to Le Roy, is a solution of water in air. Much like other solutions, in a closed environment evaporation too reaches saturation, and the saturation limit increases with increasing temperature. The theory was confirmed by the transformations that occur when humid air is enclosed in a sealed flask. What happens when the flask is refrigerated? Its surfaces fog up. What happens when it is heated? The mistiness decreases until it disappears and the air inside the flask becomes transparent again, apparently dry. This theory held for a long time, though some observed that evaporation can also occur in a sealed vacuum. Supporters of the theory replied that such vacuum evaporation was of a different nature than evaporation in air.

The theory began to weaken when Black showed that the production of vapour requires heat. Placing a mass of water over a controlled fire, Black heated it until boiling and measured how much weight had been lost after a certain amount of time. Conversely, he observed that a mass of water heats up if a certain quantity of vapour is condensed in it. Having thus demonstrated the large latent heat of vapour, Black left the task of measuring it to Watt,

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[388] *Ibid.*, pp. 183-209.

who devised the following quantitative experiment (which he also conducted in the presence of De Luc): after having condensed a certain amount of water vapour at boiling temperature inside a certain quantity of water, he deduced the quantity of heat lost by the vapour from the increase in the temperature of the water. From these experiments it resulted that the latent heat of a mass of vapour at boiling temperature increases the temperature of an equal mass of water by 943 °F (506.1 °C); in other words, the heat of vaporisation of boiling water is about 506 cal/g.

Black and Watt's experiments were invoked by De Luc to modify Le Roy's theory: evaporation is not a solution of water in air but a solution of water in heat.[389] Lavoisier attempted to reconcile the two theories: according to him, evaporation is a solution of a liquid partially in air and partially in *caloric* (a term he used to indicate the fluid of heat), as evidenced by the fact that refrigeration always accompanies evaporation. However, the evaporation of a liquid at its boiling point is a phenomenon of completely different nature, in the sense that the part of the liquid dissolved in air is almost negligible compared to the part of the liquid dissolved in *caloric*. Therefore, Lavoisier proposed to call this latter phenomenon vaporisation and the former evaporation,[390] terms that have now become nearly synonymous, though the first still connotes a certain intensity. Another vestigial effect of the theory is the use of phrases like, "dissolving in heat". Yet the salient physical distinction between the two phenomena, according to Lavoisier, is that in evaporation the quantity of vapour produced is proportional to the evaporating surface, while in vaporisation it is proportional to the quantity of *caloric* provided. In short, Lavoisier took a step back from the Black's science.

7.21 The measurement of heat and specific heat

Richmann's experiments were repeated in 1772 by Johann Karl Wilcke (1732-1796), who confirmed the mixture formula and introduced a unit of measure for heat, more or less defined as it is today, though it remained without a specific name until 1852, when Pierre Favre (1813-1880) and Jean Silbermann (1806-1865) gave it the name *calorie*.

The idea of "thermal capacity" (or calorific capacity) arose around Wilcke's time, and scientists used the expression in two completely different senses, a fact that can possibly disorient the modern reader. Some

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[389] *Ibid.*, p. 83.

[390] A. Lavoisier, *Traité élémentaire de chimie présenté dans un ordre nouveau, et d'après les découvertes modernes*, Paris 1789, pt. 3, ch. 5, sec. 3. This work was published in numerous later editions and translations.

employed the term to mean the total quantity of heat "contained" in a body, which Lavoisier and Laplace in vain tried to measure, as we shall see (§ 7.22); others, the majority, used it in the current sense, referring to the heat necessary to heat or cool a body by one degree (for a given temperature scale). It was easy to pass from this idea to the idea of specific heat, namely thermal capacity per unit mass of a body. Wilcke himself began its study in a paper published in 1781 by the academy of sciences of Stockholm (and written in Dutch). In the paper, Wilcke described a novel calorimeter based on the discovery of latent heat of fusion. He bored a hole in a block of ice and placed the hot body to be examined inside it, closing it with more ice and waiting (sometimes more than ten hours) for the temperature of the body to reach 32 °F. Then, he gathered the water produced in the cavity with a sponge and based on its weight he deduced the amount of heat lost by the body. But the unavoidable amount water that remained attached to the ice, the length of the experiment, and the consequent loss of heat to the external environment made the measurement highly uncertain, so Wilcke abandoned the fusion method and returned to the well-known method of mixtures, with allowed him to measure many specific heats and introduce the concept of "equivalent in water".

The method of mixtures was used by countless physicists in the following century. In particular, it was this method that Dulong and Petit resorted to (1819) to determine the specific heat of a great number of solids, arriving at their famous law that the product of specific heat and atomic weight is constant, invaluable to chemistry and the source of a century of headaches for theoretical physics ([391]). The method of ice melting that Wilcke abandoned was picked up by Lavoisier and Laplace in a famous paper that we will analyse more fully in the next section. The French scientists, however, claimed that they had come up with the idea independently of Wilcke's attempts, as they read his work after completing their research and reading their paper to the Académie des sciences of Paris.[392] The "machine", which Lavoisier later called *calorimeter* in his treatise on chemistry, was made up of three concentric containers: the internal metallic one held the heated body; the intermediate one held the ice that was to melt; and the external one contained water or ice with the aim of ensuring a constant

[391] § 6.12 in: M. Gliozzi, *A History of Physics: over the Last Two Centuries.* Cambridge Scholars Publishing, in press 2022.

[392] A. Lavoisier and P.-S. Laplace, *Mémoire sur la chaleur*, in "Histoire de l'Académie royale des sciences. Mémoires.", 1780, p. 373 in note. Although it was inserted into the Académie's volume for 1780, the paper was read on 18 June 1783 and the volume was effectively published in 1783. The paper later appeared in numerous editions and translations.

temperature of 0 °R in the central one. The scientists insisted on essentiality of the external shell and attributed Wilcke's failures to its absence. Based on the quantity of melted ice, taking into account the equivalent of the internal container in water, Lavoisier and Laplace measured the specific heats of many bodies, both solid and liquid, and discovered that the specific heat of a body is not constant but rather varies with temperature. Indeed, they affirmed, in agreement with the experimental results obtained and their hypothesis of a latent heat of expansion, that it always increases with temperature, which, as is now known, is not always true. Lastly, they measured heats of mixing and respiration.

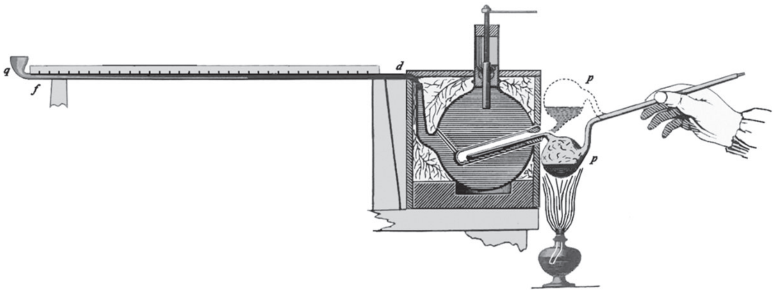

Figure 7.6 - Favre and Silbermann's calorimeter (1852), a precursor to Bunsen's calorimeter. The ball *a*, filled with 8-10 kg of mercury, is connected to a calibrated tube *df.* Based on the displacement of the mercury in the tube, one can deduce the amount of heat given off by the body being examined, which is placed inside the ball through the metallic tube *b*. *Source*: M. Pouillet, *Éléments de physique expérimentale et de météorologie*, Paris 1853.

The biggest problem with Lavoisier and Laplace's calorimeter, as was later discovered, is that part of the ice melted into water cannot be collected because it remains attached to the ice. Many attempts were made to improve the instrument with a few positive results. The problem, however, was only eliminated completely in 1870, when Robert Wilhelm von Bunsen (1811-1899), refining an apparatus devised in 1852 by Favre and Silbermann (Fig 7.6), proposed his famous calorimeter in which the amount of melted ice is deuced from the reduction in volume.

7.22 Mathematicization of the fluid theory

Between 1750 and 1781, physics succeeded in clearly distinguishing between the concept of heat and that of temperature; the heat of fusion and vaporisation was discovered and measured; the concept of heat capacity was developed; and, above all, consistent measurement conventions were fixed and two new measuring techniques were introduced that are still used today. The ideas introduced were so far from the usual structures that the most important thermologists of the time – Black, De Luc, Laplace, and Lavoisier – complained of the lack of a specialized language to avoid misunderstandings. In particular, Lavoisier ended the preface to his treatise on chemistry with a quote from Abbé de Condillac: “Scientists think better when they improve their language.” With this phrase, Lavoisier referred mostly to chemical nomenclature, but many subfields of thermology were still considered part of a standard chemistry course.

The work of organizing the copious experimental results collected in the previous thirty years was undertaken by the aforementioned paper by Lavoisier and Laplace, which integrated it with new experiments and brilliant theoretical developments. The new experimental work had been conducted as a collaboration between the two scientists in the winter of 1782-83, but it was Lavoisier who wrote the paper, which exhibits his systematic style. The paper is divided into four sections or “articles”: the first essentially describes the ice calorimeter, the second details the results of numerous experiments, the third discusses their consequences, and lastly, in the fourth section there is a demonstration that respiration is “a very slow combustion, exactly like that of coal.”

The discussion begins with the observation that whatever be the cause that produces the sensation of heat, it can increase and decrease and therefore is amenable to calculation. After mentioning thermometers and phase transitions, the two scientists continue: “Physicists do not agree on the nature of heat. Many of them consider it like a fluid dispersed in all of nature that more or less interpenetrates bodies in proportion to their temperature and their particular ability to hold it; it can combine with bodies and, in this state of combination, cease to act on thermometers and pass from one body to another; only the free state permits it to attain equilibrium in bodies: in this state it constitutes what we call *free heat*. Other physicists hold that heat is the result of invisible movements of the molecules of matter. It is known that even the densest bodies contain a great number of pores or small holes whose volume can greatly exceed that of the matter containing it; these empty spaces give the smallest undetectable particles of matter the freedom to oscillate in all directions, and it is natural to think that the particles are in a continuous state of excitement, which, if it grows past a certain limit, can destroy and decompose bodies: this internal motion constitutes heat, in the view of those physicists to which we are referring… In the hypothesis examined here, heat is the living force that results from the undetectable movements of the molecules of a body; it is the sum of the products of the mass of each molecule and the square of its velocity."[393]

Having noted that the mechanical hypothesis can correctly interpret both the experimental principle of thermal equilibrium and the fact that luminous rays have an imperceptible impulse (proportional to ordinary velocity) but produce significant heat (proportional to the square of the velocity), the scientists concluded by avoiding a ruling in favour of one of the two theories, affirming that "perhaps both are true!" In our view, this agnostic conclusion can be attributed to the fact that the two scientists could not agree on this issue: Lavoisier was a supporter of the fluid theory while Laplace, on the other hand, subscribed to the mechanical theory (but later changed his mind).

The two scientists were nevertheless able to find principles compatible with both theories, like the following: the amount of free heat remains constant in simple mixtures, that is, those that do not host chemical reactions. If, instead, chemical reactions do occur, the principle can become erroneous for both theories, though both then remain compatible with the following new principle: all the changes in heat, both real and apparent, experienced by a system of bodies that changes phase are repeated in reverse when the system returns to its original state. Other postulates and definitions followed, serving to provide a rational framework for the theory of specific heats and to permit discussion of the two methods for measuring heat.

In the second part of the paper, after the heat of fusion of water is determined to be 75 cal/g °C (using modern notation for simplicity), there is a table of the specific heats of many substances (wrought iron, crystal, mercury, lime, etc., with eleven substances in total) accompanied by a careful description of the experiments conducted to obtain the values. The experiments to measure the heat of combination, combustion, and animal heat then follow.

The third part is of a theoretical nature and contains a few "reflections on the theory of heat": here the (plural) "theories" of heat have given way to the (singular) "theory", naturally the fluid theory. The section begins with a problem that was highly discussed by physicists at the time: is it possible to measure the "total quantity" of heat possessed by a body, namely that which at the time was known as the "absolute heat"? The two scientists believed that they could attempt the calculation because, in their view, the

[393] *Ibid.*, p. 357.

heat produced in exothermal chemical reactions comes at the expense of the total heat possessed by the reagents. Furthermore, they assumed the postulate (already advanced by others and in contradiction with the supposed latent heat of expansion: § 7.21) that the ratio of two specific heats is equal to the ratio of the respective total quantities of heat. Such a postulate was obviously fluidistically inspired. Indeed, if we refer to the quantity of liquid necessary to raise the level in a container by one notch as the "specific liquid capacity", then it follows that the ratio of the amounts of liquid in two similar containers (where the liquid levels are the same) is equal to the ratio of their specific liquid capacities. Once the scientists had built a mathematical theory based on these and other hypotheses, they applied it to measure the absolute heat of water at 0 °R, having already measured the heat produced from the combination of water with quicklime (calcium oxide), sulphuric acid, etc. Using this method, they found five results in complete disagreement with each other: water at 0 °R supposedly had an absolute heat ranging from thousands of positive heat units to thousands of negative heat units (despite the latter not having any physical meaning)! The concept of absolute heat thus remained a "dead letter", as De Luc wrote. Yet Lavoisier and Laplace were more optimistic than their Swiss colleague and hoped that more careful future experiments could bring success to their theory.

The phenomenon of supercooled water was the subject of a careful study by scientists, which we hold useful to relate in detail to give a concrete example of the first thermological calculations carried out.

Let a unit mass of water be at c °R below zero (freezing) and let, for any arbitrary reason, the molecules of $1/n^{\text{th}}$ of this mass be arranged as ice, that is, let $1/n^{\text{th}}$ of the mass of water freeze: it will produce $60/n$ units (per degree Réaumur) of heat, which will be distributed throughout the water and the ice (supposing no exchanges of heat with the external environment). If q is the ratio between the specific heats of water and of ice, it follows that in the entire mass there will be an increase in temperature equal to

$$\frac{-60}{n+q-1}$$

Thus, the temperature of the mixture will become

$$c-\frac{60}{n+q-1}$$

Because n is arbitrary, there can be infinitely many possible equilibrium states, but the expression above is limited by the fact that the temperature of the mass can never surpass the zero of the thermometer, because at this temperature the ice begins to melt. In other words, while the experiment shows that water can remain liquid at temperatures below 0 °R, the same experiment tells us that (keeping the pressure constant) ice cannot exist at temperatures above 0 °R. Supposing, then, that we reach this limiting condition, one has

$$c - \frac{60}{n+q-1} = 0,$$

from which one can find

$$\frac{1}{n} = \frac{c}{60 + c - cq},$$

an expression that indicates the greatest amount of water that can be converted into ice when the water is at a temperature of *c* °R below zero. If the entire mass can be transformed into ice then $1/n = 1$, and therefore one has

$$c = \frac{60}{q}.$$

Richard Kirwin gave a value of 0.9 for *q*, thus resulting in *c* = 66(2/3) °R = 82(5/6) °C: the lowest temperature (below zero) that a mass of water can have and still completely freeze without any exchange of heat with the external environment. In practice, this temperature is much closer to zero for inevitable losses of heat to the environment.

Regardless of the specific mathematical formulation, modern science accepted the fundamental concept on which the above theory was based: the transition from supercooling to solidification is a phase transition at increasing temperature that occurs at the expense of the heat of fusion provided by the solidification process. Logically following from this theory is a consequence that went unnoticed by scientists of the time and was only experimentally discovered much later: if the temperature of water is less than *c* degrees below zero, the heat of fusion is not enough to bring the mass to the freezing temperature. We believe that it was this effect that the unknowing experimenters of the 18th century sometimes saw when they tried to determine the zero using "artificial freezing" of water, as suggested by Réaumur (§ 7.17).

Lavoisier and Laplace's paper was followed by Lavoisier's famed treatise on chemistry, which spread the theory and nomenclature of "caloric"[394] and established the name "calorimeter" for the associated instrument of measure.[395]

From the previous pages it is clear that the fluid theory of heat fulfilled all of the requirements of a sensible physical theory because of its predictive capability and its ability to calculate experimentally defined physical quantities. It is no surprise, then, that for over fifty years it appeared consistent and harmonious, preferable to the mechanical theory, which had remained only qualitative. Furthermore, one can see that for a science that is still undeveloped, an intuitive hypothesis with simple analogical connections is a more convenient, if not more powerful, instrument than a remote mathematical conception.

7.23 Note on the introduction of the steam engine

Although the history of engineering physics lies outside the scope of this book, we must say a few words on the invention of the steam engine, which directly influenced the study of physics itself.[396]

A few scientists of the 16th century, like Cardano and Porta, had worked with the expansive force of water vapour, and Porta had even devised a machine to lift a column of water using the vacuum produced by the condensation of water vapour. In 1629, Giovanni Branca (1571-1640) engineered a strategy to transform Heron's aeropile (§ 1.9) into a genuine vapour-based turbine, but his plans did not result in any construction (not even of a model).

Denis Papin (1647-1714) was a student of Huygens and his collaborator in 1682, when they built a machine with which a piston inside a cylindrical tube was raised by the combustion of gunpowder placed at the bottom of the cylinder. In 1690, he had the idea of replacing the gunpowder with some water that would then be vaporised by heating. Whether or not he was in fact able to obtain the practical results that are attributed to him using this machine or another, it is certain that in the course of these studies he discovered that the boiling temperature of water increases if the pressure is increased, and applied this discovery to obtain water at temperatures above

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[394] The term "caloric" had been proposed by the reformers of the chemical terminology: *Méthode de nomenclature chimique, proposée par MM. De Morveau, Lavoisier, Berthollet et de Fourcroy*, Paris 1777; Lavoisier, *Traité élémentaire de chimie* cit., pt. 1, ch. 1.

[395] Lavoisier, *Traité élémentaire de chimie* cit., pt. 3, ch. 3.

[396] For more information the reader can consult a work on the history of engineering, for example (in Italian) U. Forti, *Tecnica e progresso umano*, 2 volumes, Fabbri, Milan 1963.

80 °R, heating it in a closed pot. To avoid the possible explosion of the pot, he invented the *safety valve*, a self-regulation mechanism that has been more useful to our era of automation than it was to Papin's contemporaries.

The increasing use of coal in Britain created a production issue that in the previous centuries had been solved with primitive means: the extraction of filtered water in coal mines. The efforts of many British inventors turned to devising a more efficient extraction method. Edward Somerset (1601-1667) devised a heat engine to lift water that was later rediscovered by Thomas Savery (1650-1715) in 1698; in 1705, Leibniz sent a drawing of it to Papin. That same year, it is said that the blacksmith Thomas Newcomen (1679-1730) patented a heat engine related to Papin's work and in particular the idea of using a piston. In Newcomen's engine, the vapour, produced by a steam boiler, lifts the piston; then, when the admission valve is closed, the vapour condenses as the cylinder is cooled with water, and the piston, weighed down by the atmospheric pressure, falls. The alternating motion of the piston is then transmitted to the shaft of a pump through a lever. The very rudimentary engine worked for decades, though with an enormous loss of heat, mainly due to the cooling of the cylinder at every pump through a jet of water.

James Watt (1736-1819), manufacturer of mathematical and mechanical instruments at the University of Glasgow and thus in continual contact with Black, was commissioned by the University ton repair a model of Newcomen's engine in 1763. Watt first turned to the enormous waste of heat by the engine because of the cooling of the cylinder. Setting out to reduce its waste, he realized (1765) that the expulsion of vapour from the cylinder could also be obtained by opening a connecting tube between the cylinder and an empty container at the right time – the vapour would be sucked right in. Thus, was born the *refrigerant*, the third component of the heat engine, which in this way truly became a *steam engine*, whereas the previous models would be more properly called *atmospheric engines* because their operation was based on the use of atmospheric pressure.

Encouraged by his first great success, Watt continued to make other brilliant improvements to his engine, taking advantage of collaborations with many other ingenious engineers. He created the *double acting steam engine* (that is, an engine in which vapour acts on both sides of the piston) and introduced the centrifugal generator, the slide valve, the steam jacket around the cylinder, and the pressure indicator. These are the essential elements of a modern heat engine, and Watt and his collaborators should be more rightfully called the inventors and not just the improvers of the steam engine.

By the beginning of the 19th century, Britain already had five thousand functioning steam engines, France had several hundred, and Germany had a few dozen. The spread of the steam engine, a leading cause of the new era of industrialization, turned physicists' attention first to the study of water vapour and gases in general, and later to broader considerations on its operation[397].

ELECTRICITY AND MAGNETISM

7.24 Phosphoric light; conductors and insulators; vitreous and resinous electricity

An experiment that was only definitively proven to be of electric nature in the second half of the 19th century gave new impulse to the study of electricity. In 1676, the French astronomer Jean Picard (1620-1682) noticed as he was transporting a barometer at night that occasionally a flash of light would appear in the part of the tube not containing mercury. The phenomenon, which not everyone was able to reproduce, was described in a book that fell into the possession of Johann I Bernoulli. Struck by its originality, he began a careful study of the phenomenon, succeeding in building a small apparatus in 1700 that spread rapidly throughout Europe and provoked the astonishment of both learned and unlearned people. The apparatus consisted of a glass tube emptied of air and partially filled with mercury, which miraculously radiated as if it were "full of fire" when it was shaken in the dark. Bernoulli likened the phenomenon to the one exhibited by phosphorus, and the light emitted from such tubes was called "phosphoric light" because it was believed to come from the agitated mercury in vacuum: at the end of the century, for the wonder generated by the recent discovery of phosphorus, every luminescent effect was attributed to phosphoric emanation, and everything became phosphorus.

With a phosphoric light experiment performed on 5 December 1703 in front of the Royal Society of London, presided over by Newton, Francis Hauksbee (1660-1713) made his entrance into the history of physics, a man whose life is almost completely shrouded in mystery. A salaried demonstrator for the Royal Society until his death, which occurred between the end of May and the beginning of June 1713, he became a member in 1705, while from 1704 to 1713 he published a series of papers in "Philosophical Transactions" concerning various problems of experimental physics, in

[397] Chapter 2 in: M. Gliozzi, *A History of Physics over the Last two Centuries.* Cambridge Scholars Publishing, in press 2022.

particular on the vacuum pump, capillary tubes, phosphoric light, and electrical phenomena. A shrewd manufacturer of physical apparatuses and experimenter, Hauksbee perhaps owed his theoretical preparation to Newton, like Newton owed the planning and execution of many experiments that he described to support his theories to Hauksbee.[398]

His 1703 experiments, according to the unpublished transcript of the Royal Society meetings, demonstrated a phenomenon occurring in mercurial phosphorus, as Hauksbee called it, that was later described in a 1705 paper: a strong phosphoric light is obtained when a mass of mercury is moved inside the empty vacuum pump by a puff of air. The light is produced when the mercury drips down the walls of the tube, and the effect looks like "fire rain", or better, "fire snow". The effect seemed linked to the slipping or rubbing of the mercury on the glass, so Hauksbee wondered if it could also be obtained by rubbing other substances in vacuum. With an ingenious device, Hauksbee was able to observe luminous phenomena by rubbing amber and wool, glass and wool, glass and glass, etc. He obtained most marked effect when he used a hollow glass sphere emptied of air and set it in a rapid spinning motion, over which he lay his bare, open hands: the light produced was purple and so bright that it allowed one to read lowercase letters and sometimes lit up the entire room.

Perhaps it was Newton who, having already experimented on the electrification of rubbed glass (§ 6.22), brought the electrical phenomena that accompany the rubbing of glass to Hauksbee's attention. In any case, with no explicit connection to the previous experiments, Hauksbee began a study of the "extraordinary electricity" displayed by his spinning glass globe, which Newton made famous by describing it in an added note to the eight questions of the second edition (1717) of *Opticks*. From the glass glove, Hauksbee moved onto a hollow glass tube and then a solid cylinder, noting the attractions, the "wind", the electric spark, and the negative influence of humidity on the success of electrical experiments. Hauksbee's most important contribution remains the introduction of the glass rod still used today, as it made electrical experiments easily accessible to everyone at little cost and much entertainment value.

Stephen Gray (1666-1736), another British scientist, took advantage of this starting in 1720, perhaps to occupy the combat of his last years of retirement. Gray's critical discovery, which he made in 1729 and was announced in one of his seven published articles from 1720 to 1736 in

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[398] In 1709 a first collection of Hauksbee's papers was published in London, followed by the complete collection of his works: F. Hauksbee, *Physico-Mechanical Experiments on Various Subjects ... To Which is Added a Supplement, Containing New Experiments not in the Former Edition*, London 1719.

"Philosophical Transactions", was that the electrical property can be passed from certain bodies to certain others (Fig 7.7).

Figure 7.7 - Gray's experiment, from a 1754 incision; the person on the right brings an electrified glass tube close to the woman suspended by silk ropes and the man on the left is struck by a spark.

He had demonstrated this using an empty (and probably humid) glass tube sealed with stoppers: when the tube was rubbed, the stoppers attracted a feather. Furthermore, a small ivory sphere fixed to a fir branch attached at one side to a stopper acquired electric properties every time Gray rubbed the glass tube. The experiment, however, failed when the scientist tried to lengthen the path by hanging a horizontal rope between two nails fixed on a beam. On the suggestion of a friend, he then hung the rope using silk strings, which he believed would not disperse the electric property because of their thinness. The experiment was a success, and the rope was then lengthened bit by bit until its weight broke the silk strings. When these were replaced by stronger bronze strings, the experiment once again failed. Gray immediately concluded that the silk strings did not disperse electricity not because of their thinness but because of their nature: thus, the idea of a conductor and a nonconductor entered physics, to use the terminology introduced by Desaguliers in 1739.

Yet was electric virtue only transmitted through solid bodies? To answer this question, Gray conducted the first experiment on electromagnetic induction in 1730: a conductor, hanging from a linen cord, attracted a bronze sheet if a piece of pre-rubbed glass was brought close to it without touching.

What's more, he added in 1734 after reading Du Fay's paper, which we will soon discuss, if a pointed iron rod suspended by silk strings is approached by an electrified glass rod, one can observe two sparks at the pointed ends, therefore, as Gray prophetically concluded, electrical phenomena are connected to thermal and luminous ones, which one day may perhaps be made evident. It was not this experiment and these considerations that attracted the attention of his contemporaries but rather another simply spectacular demonstration performed by Gray in 1730: a boy suspended from the ceiling on a swing became electrified when a rod of electrified glass was brought close to his feet.

Gray's work attracted the attention of Charles-François Cisternary Du Fay (1698-1739), a short-lived French scientist with multiple interests, ranging from archaeology to botany (he was the director of the botanical gardens of Paris and picked the then unknown Georges-Louis Buffon as his successor), from geometry to anatomy: a moving eulogy to him was written by Fontenelle. In 1733, Du Fay presented four papers on electricity to the Académie des sciences of Paris. In the first, he recounted a brief history of electricity; in the second, he detailed the great number of experiments that brought him to conclude that all bodies, except metals and fluids, can be electrified by rubbing, though an exception, may one day be found, as indeed occurred in 1778 due to the work of John Ingenhousz (1730-1799), a physicist of Dutch origins that however lived his whole life in England.

In repeating Gray's experiments, Du Fay introduced the use of the insulating support (the term "insulator" and its derivatives were also coined by him), which is still used to this day. The device allowed him to easily establish that all bodies are electrified by external influence, and that those that are not easily electrified by rubbing are less suitable for transmitting the electric property: thus, the distinction between conductors and insulators was established, for many years held to be rather unambiguous, forgetting that *natura non facit saltus* (nature does not jump), as Leibniz warned.

Much more important was the content of the last paper published by Du Fay in 1733. Having observed (like Cabeo and others had before) the phenomenon of electric repulsion, he initially supposed that the repulsion was only apparent and due, therefore, to the attraction of other bodies near the repelled fragment. However, he was forced to abandon this hypothesis after repeating Guericke's experiment (in the form suggested by Hauksbee) many times: a gold flake that came into contact with a tube of electrified glass was repelled, continued to follow the movement of the tube as an experimenter brought it across the room; after a few minutes the flake returned to the tube, barely touched it, and was newly repelled. Furthermore, when Du Fay touched the flake with his finger, he noticed an oscillatory between his finger and the tube. The glass tube, he wrote, is a source of electricity for the gold flake, but when the tube and the flake are both electrified, they repel.

This new theory was very original, but less revolutionary than it would have been a decade later. Newtonianism and its dogma of "universal" attraction had only just poked its head into continental Europe (§ 6.11), and Cartesian vortices offered a reasonable mechanical representation of Du Fay's phenomenon. However, a new and unexpected phenomenon risked to disprove Du Fay's theory: after a gold flake was repelled upwards by an electrified glass tube, a piece of electrified copal attracted the flake instead of repelling it, as the theory would have predicted, but a piece of electrified rock crystal repelled it. After this experiment, Du Fay, overcoming great difficulties, moved on to the classic experiments: a thin wooden stick with a hinge like a magnetic needle was attached at one end to a piece of electrified copal. Another piece of copal, amber, or wax, electrified by rubbing, repelled the first when it was brought close, whereas electrified crystal or glass attracted it. "We therefore have," concluded the scientist, "two well-documented types of electricity, and I cannot avoid giving them different names, to avoid a confusion of terms or the quandary of always having to define the one to which I refer; I shall therefore call one *vitreous electricity* and the other *resinous electricity*."[399] The rule to recognize the type of electricity acquired by a body that has been rubbed, identical to the one found in physics texts today, immediately follows. With Du Fay, electrical phenomena began to organize themselves into a coherent theory equipped with its own laws, sixteen of which the French scientist stated in a 1734 paper.

7.25 The electrostatic machine and the Leiden jar

Hauksbee's rotating globe, which had been forgotten in favour of the simpler glass tube or stick, was made relevant again by Christian August Hausen (1693-1743), who added a crank to it; soon after, in 1744, Georg Matthias Bose (1710-1761) equipped it with a "conductor", a metal cylinder that acted as a collector of electricity. The device was significantly improved by Johann Heinrich Winkler (1703-1770), a professor of Latin literature at the University of Leipzig who replaced the globe with a glass tube rotated by a pedal system, with the tube no longer electrified by hand but by rubbing against a leather cushion covered in horse hair; the cushion was connected to the ground by conducting columns. Thus, around 1743,

[399] Ch.F. Du Fay, *De l'attraction et répulsion des corps éléctriques*, in "Mémoires de l'Académie royale des sciences", 1733, p. 469.

the electrostatic machine consisted of a rotating glass globe or tube pressing against a cushion; its vitreous electricity was collected by a long and wide metallic cylinder called the *chain* or (*first*) *conductor*, which hung horizontally above the glass globe from silk strings. The base of this cylinder was held so close to the globe that it could almost directly touch it with a metallic brush or a hanging chain, and the accumulation of electricity thus occurred a bit by contact and a bit by discharge from the metal edge. It should be noted that until past halfway through the century, electrification by contact, induction, and discharge were conflated into a single effect, called "electrification by communication". The conducting column connected to the rubbing cushion, if insulated, supplied the negative electricity.

The replacement of the globe or cylinder with a more practical glass disk occurred much later, between 1755 and 1766; Martin Planta (1727-1802), Ingenhousz, and Jesse Ramsden (1735-1800) all claimed to have invented it first. Today the machine is named after Ramsden, a manufacturer of scientific apparatuses in London who built and popularized it (Fig 7.8).

Interest in these new phenomena, which had been limited to a few scientists in the years leading up to 1740, erupted that year into a public curiosity that continued to grow with the increasingly remarkable effects obtained using electrostatic machines and with the new electric phenomena that were discovered in parallel: flame conductivity, electric hum, the "lighting" of ether through a spark from a glass tube, electric shock, etc. Shows involving electricity were held in town squares and streets everywhere, organized by scientists and magicians that had found another way to earn money. This buzz attracted more scientists to the new field, despite the condescending smiles of prime people asking the usual question: what is it good for? Physicists were not the only ones to turn to these new researches, doctors did too: the first attempts to apply electricity to medicine occurred in Halle, Venice, Turin, and Bologna. In Halle, for example, Christian Gottlieb Kratzenstein (1723-1795), after testing the real or presumed physiological effects of electric discharge (muscle contraction, increase in heart rate, acceleration of the circulation of blood), proposed to use it as a curative technique.

Others believed that potions of electrified water were beneficial to health. It was perhaps in the midst of preparing such a potion in 1745 that the cleric Ewald Jürgen von Kleist inserted a nail into the neck of a bottle full of water and touched it to the conductor of a working electrical machine. Then, after interrupting the contact, he touched the nail with his other hand: it sent a violent concussion through him that made his arm and shoulder ache.

That same year, Musschenbroek, a professional physicist, independently conducted the same experiment and felt his "whole body shocked as if by lightning". News of this effect, immediately communicated from Musschenbroek to Réaumur, spread rapidly and the experimental device came to be known as the "Leiden jar", a name given to it by Nollet, who perhaps chose this neutral denomination because other experimenters in Leiden had contested Musschenbroek's claim of priority in various letters sent to Nollet in the days that followed.

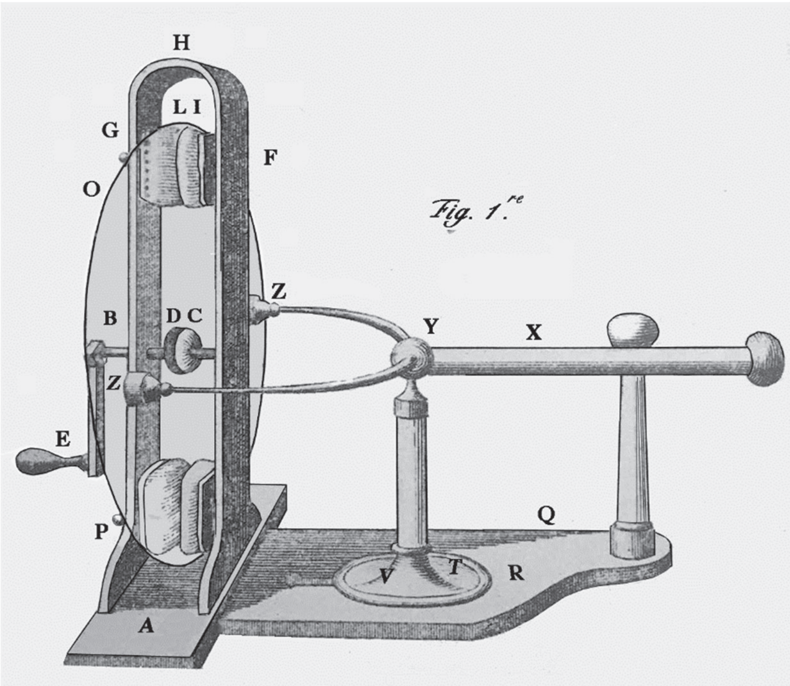

Figure 7.8 - Ramsden machine, late 18th century.
Source: M. Guyot, *Nouvelles récréations physiques et mathématiques*, Paris 1800.

The unusual and striking effects immediately roused great interest and thrill. Nollet began the demonstration of "shocking" a chain of monks that held each other's hands at the Paris monastery. He continued to experiment on birds, employing a modest but useful apparatus, the *surge protector*, which is still used today. After having killed a few small birds with electric discharge, Nollet, ever theatrical, cautioned that this new mysterious force that "peut s'animer et s'irriter" (can come to life and become angry) had to be treated with care. We refrain, however, from any retroactive condescension.

In 1746, the water in the bottle was replaced by a metallic sheet covering on the internal and external faces: at the time it was believed that the only advantage of this substitution was that it made the bottle portable, that is, independent of the machine. The parallel plate capacitor was also built (the term, however, was introduced much later by Volta: § 7.32) and, to amplify its effects, Johann Winkler in German and Benjamin Franklin in America connected it in parallel to Leiden jars, obtaining powerful "batteries", to use the term introduced by Franklin.

Nollet's aforementioned experiment at the Paris monastery set off a series of attempts to determine the transmission velocity of the electric fluid. That same year, 1746, Louis-Guillaume Le Monnier (1717-1799) attempted the first measurement: the experimental circuit was made up of two men at diametrically opposite positions around a circular pond, connected by a chain along half the circumference of the pond. The discharge of a Leiden jar, held by one of the men, travelled through the water of the pond, to the other man, and then through the chain, finally ending up again at the first man. The experiment consisted of establishing whether the shocks felt by the two men were simultaneous or in succession. The results were contradictory: sometimes the velocity seemed infinite to Le Monnier and other times he calculated it to be 6517 m/s.

Several of Le Monnier's illustrious colleagues (William Watson, Cavendish, Bevis) continued his attempts: the results of these experiments, which lasted a little more than a year between 1747 and 1748, were communicated by Watson in the 1748 edition of "Philosophical Transactions". To the British scientists, it appeared that the propagation of the electric fluid, both in the waters of the Thames and through a nearby mountain, was "nearly instantaneous".

7.26 Benjamin Franklin

Franklin (Fig. 7.9) was led to such research almost by chance. He was forty years old when he began them and in less than three years he brought science bounds ahead. One of the first details that struck him was, to use his own words, "the wonderful effect of pointed bodies, both, drawing off [attracting] and throwing off [repelling] the electrical fire."[400] The observation was not new, as we know (§ 7.25), but what was new was the systematic nature of the experiments with which he established the "power

[400] Benjamin Franklin, *Letter to Peter Collinson*, 11 July 1947.

of points", namely, as Franklin understood it, that pointed objects are equally suitable for attracting and expelling electric fluid.

Figure 7.9 - Benjamin Franklin. Engraving by M. Chamberlin and E. Fischer. *Source*: Museo Nazionale della Scienza e della Tecnologia Leonardo da Vinci, Milan.

Yet how could this be logically explained? Franklin attempted it, but he himself felt that his explanation was inadequate. And then what? Did physics perhaps have to understand the ultimate essence of phenomena?

The American, equipped he with the straightforward and pragmatical attitude that accompanied all of his scientific research, answered: "Nor is it of much importance to us to know the manner in which nature executes her laws; it is enough if we know the laws themselves. It is of real use to know that China left in the air unsupported will fall and break; but how it comes to fall, and why it breaks, are matters of speculation. It is a pleasure indeed to know them, but we can preserve our China without it."[401]

The qualitative similarity between an electric spark and lighting had been noticed since by the first experiments, but the use of the Leiden jar showed other similarities: killing animals, melting metals, producing a phosphoric odour. Franklin noticed these but also noted that there was at least one phenomenon that, for the moment, did not allow one to equate sparks to lighting: electric fluid is attracted by points, while it was not known if the same thing applies to lighting – "An experiment should bedone," he wrote in his notebook. On 29 July 1750, in a letter to Peter Collinson (1694-1768) – the friend and scientist who had sent him a glass tube with instructions for its use as a gift in 1756, thus encouraging him to conduct electrical research, and to whom Franklin therefore reported the results of his studies in letters – he detailed the experiment that he was about to perform: Franklin would erect a long, pointed iron rod at the top of a high tower (like the Philadelphia bell tower) and observe if it attracted sparks when storm clouds passed over it.

Collinson, who was a member of the Royal Society, attempted to have Franklin's letters published in its "Philosophical Transactions", but they were rejected, with the journal labelling the letters unworthy of publication and the project of harvesting sparks from clouds delusory. Some historians see this rejection as a reflection of the political relations between Great Britain and its insubordinate colony, permeated by the spirit of liberty. Perhaps this did play a role. Yet perhaps a greater factor was the conservative mentality that in every era in the history of science has counterbalanced and slowed the spirit of adventure, that can and at times does divert research, but without which true scientific progress would be nearly impossible.

After the Royal Society's rejection, Collinson published Franklin's letters on his own. They were so successful that they were almost immediately translated (on Buffon's request) into French, and aroused the admiration of scientists across continental Europe and the admiration of the approval of the French court. "The publication offended the Abbé Nollet," wrote Franklin in his autobiography, "preceptor in Natural Philosophy to the royal family, and an able experimenter, who had form'd and publish'd a theory of electricity, which then had the general vogue. He could not at

[401] *Ibid*, p.59

first believe that such a work came from America, and said it must have been fabricated by his enemies at Paris, to decry his system."[402]

Nollet's suspicion highlights how little the American's name was known in Europe. The son of a British colonist, Franklin was born in Boston on 17 January 1706. Growing up, he worked in his father's soap and tallow shop, later becoming an apprentice to his brother, who was a printer, a job that gave him his first education and a nondenominational moral development. In 1723, he moved to Philadelphia; and later went to Britain to improve his typographic art; he returned to Philadelphia and dedicated himself to work as a printer and civil endeavours: he founded papers and cultural circles, a library, which in 1743 became the American Philosophical Society, and in 1751 built an academy for the education of the youth, the beginning of the University of Pennsylvania. Among these various activities, he did not overlook the study of science, which had attracted his attention and then captivated him during his stay in Britain. The applicational aspect of science, his favourite, led him in 1744 to invent the type of stove that is now named after him. The conflict between Britain and the American colonies brought Franklin intro political activity and revealed his unusual talent for diplomacy, evident from his missions to Britain (1757-76) and France (1776-85). Upon his return to Philadelphia, he continued his political and civil life, and died on 17 April 1790. His fame, already great at his death, continued to increase in the 19th century: Franklin became a symbol of the fledgling American democracy as a polymath and persevering man of action.

Let us return, however, to his scientific works. Encouraged by the French king, Buffon, Thomas Dalibard, and De Luc conducted the experimented devised by Franklin: on 10 May 1752, as storm clouds passed over a garden in Marly-la-Ville, a dragoon giving watch observed sparks between a metal rod that had been erected and the sky. News of the discovery spread rapidly throughout Europe and made Franklin a household name. The experiment was repeated soon after, with the same result, in Bologna by Giuseppe Veratti and Tommaso Marino: Franklin, pleased with the news from Europe, repeated it himself by flying a kite with a pointed iron wire attached to its apex and anchored at its base to the ground. He later repeated the experiment more safely and conveniently by setting up a pointed rod on top of his house, and after many observations concluded that storm clouds are most commonly negatively electrically charged, but can also be of positive charge. In France that same year, Le Monnier discovered a new effect: the

[402] B. Franklin, *Autobiography*. The autobiography, which terminates unfinished at 1757, was published posthumously for the first time in 1818. It was reprinted in many editions and translated into countless languages.

electrification of the atmosphere even with calm skies. In Saint Petersburg, the poor Richmann, who earlier we saw occupied with successful calorimetric studies (§ 7.19), died of electrocution while experimenting on lighting, with Priestley remarking that such a demise was both "glorious" and "enviable". Undaunted, Lomonosov continued Richmann's studies and reported his results in *Oratio de meteoris vi electric ortis* (Saint Petersburg). The Russian scientist and poet also concluded an ode on the usefulness of glass with a celebration of Franklin's work[403]:

Here with great bounds
Franklin advances in his physical career,
Making his way through the clouds.
And at the tempest's darkest frontier
Erecting a rod, he kidnaps from the irate heavens
Lighting the destroyer with hand insincere.

Hail, immortal Franklin! Your divine
Example doth Europe emulate
And one less path to darken doth Death find.
You have now crossed eternity's gate
O peer to Newton, Wisdom acclaims your name
And time, obligation, and unjust fate
Are powerless to injure your lofty fame.

The experimental confirmation of atmospheric electricity upheld his project of designing a lightning rod and, despite the opposition of several physicists and civil authorities, Winkler build a prototype as early as 1752; in 1769 the grand duke of Tuscany decreed that all storage houses containing gunpowder in the duchy to be protected by lightning rods; in 1770 Girolamo Maria Fonda proposed the construction of lightning rods in the form later also named after Melsens, as he rightfully thought that the protective action of the pointed tips would be increased by increasing their number.

7.27 Electrical theories

From 1745 to 1750 there was a series of proposed electrical theories that all had a characteristic the existence of a *sui generis* fluid, to whom the imagination of the scientists ascribed "qualities so extravagant," wrote

[403] M. V. Lomonosov, *Ode on the usefulness of glass*, trans. Italian by G. De Coureil Provenzale, in *Poemetti italiani,* Turin 1797, Vol. 10, p. 160.

Euler, whose own theory was no less bizarre, "that the mind rejects them with contempt."[404]

Out of all the candidates, the theories of Nollet and William Watson (1707-1787) were rather well-known. The philosophical underpinning of Nollet's theory was the impossibility of action at a distance. He therefore imagined the existence of a fluid, perhaps the same that makes up fire and light, permeating the entire universe and inside bodies. The rubbing of a body excites this fluid, which flows out into a handful of diverging flows. This leakage of *effluent matter* brings out similar *affluent matter* from nearby bodies, which, in equal quantity, enters with reduced velocity but through more pores. Electric phenomena are then the mechanical effects of the two fluxes of electric matter and occur in the electrified body.

For Watson too every electrified body is the motor for the flux of *electric fluid* or *ether*, like animal's heart is the motor that drives the circulation of its blood. Yet while for Nollet the electrified body always contains the same quantity of fluid, for Watson every electrified body has an excess of electric fluid in a state of elastic tension, which causes the fluid to slosh towards nearby bodies, such that a flux of ether arises that is then responsible for attractions and repulsions. Watson abandoned this as soon as he studied Franklin's, while Nollet, stubborn and alone, clung to his own theory.

The theory formulated by Franklin in 1747 quickly converted not only Watson, but the majority of electrical physicists. The theory was based on the following experiment: if a person stands on an insulating stool and rubs a glass tube with a dry, bare hand, then another person standing on the floor will produce sparks by bringing her finger close to the glass tube rubbed by the first person. The experiment can be modified in several ways and, according to Franklin, is fully explained by admitting the existence of a single electric fluid contained in all bodies. Every process of electrification consists in the loss of part of a body's fluid and its subsequent injection into another body: the consequent lack or excess of electric fluid in each other the two bodies is manifested in the peculiar electric phenomena observed. Therefore, a body is electrically charged because the quantity of electric fluid it contains is either greater or less than the amount contained in its natural state: in the first case, Franklin called the body *positively* electrified, in the second, *negatively* electrified, two terms that have remained in use up to this day.

The American scientist attributed three key properties to the electric fluid: extreme thinness; mutual repulsion among its parts; and the strong attraction between ordinary and electrical matter. If a body is positively charged, the excess of electric fluid is found on its surface and forms an

[404] L. Euler, *Letter to a German princess* cit., p. 529.

electric atmosphere. This expression, already in use prior to Franklin's works, was employed almost until halfway through the 19th century and not always in a figurative sense, like a more or less plastic metaphorical expression, but in a strictly physical sense: scientists spoke of the "thickness" of the electric atmosphere in a geometric sense and sometimes claimed to have measured it. Nevertheless, it was through the idea of the density of the electric fluid took hold in the 18th century, which was rightfully then considered a more fundamental cause than the "power of points".

However, assuming the existence of electric atmospheres, how could the forces between negatively charged objects, which therefore have no electric atmosphere, be explained? In addition, is it action at a distance or mediated action? Cautiously, Franklin kept silent on such matters. Attraction and repulsion are experimental facts: for a pragmatic mindset such as his it is almost enough to simply accept them and ask no questions.

The theoretical model that he had developed was enough Franklin to build the parallel plate capacitor named after him and to recognize that Leiden jar's ability to electrically shock lies in the glass of the jar and not the two conductors or *armatures*, as he called it. He was led to this discovery by an experiment born out of his practical sense. He emptied the water of a charged jar into another jar, which did not become charged itself; the first jar, on the other hand, was electrically charged even when filled with new water. After this experiment, he moved on to the famous experiment of the decomposable parallel plate capacitor.

The inability of Franklin's theory to explain actions between negative particles encouraged a revival of Du Fay's dualistic theory, which alternated in popularity in its competition with the unitary theory.

The occasion was offered by certain curious phenomena that in 1759 attracted the attention of the British scientist Robert Symmer (1707-1763), like six year earlier they had aroused the interest of Beccaria. Symmer observed a characteristic crackling and flash of sparks every time that he pulled off the first of two (overlaid) socks, much like those observed by a person taking off a sweater. What caused the opposite electrification for the two socks? Setting out to study the question, Symmer answered that electrical phenomena are caused by two distinct entities, both active and positive, that act in contrast, so to speak. Every body contains both fluids, but in its natural, that is, uncharged state, the quantities contained are equal, and thus there are no external effects. The body appears positively or negatively charged depending on whether it has an excess of one fluid or the other. In reality, the experimental evidence that Symmer had for the theory was neither much nor convincing. The persuasive piece of evidence was without a doubt the shape of the holes in paper traversed by an electric discharge: according to Symmer, the shapes qualitatively revealed the presence of two different agents that passed through the paper in opposite directions.

Before abandoning the topic of electric theories, it is worthwhile to mention the theory of Johann Albrecht Euler (1734-1800), outlined by his father Leonhard (who perhaps collaborated in its development) in his *Letters to a German Princess*. The theory was explained in two papers with almost identical titles: the first (*Disquisitio de causa physica electricitatis*, 1754) won a prize from the academy of sciences of Saint Petersburg; the second, in French, was published in 1757 in the proceedings of the academy of sciences of Berlin. Euler accepted the idea of a single fluid but, to avoid adding to the list of unusual fluids, he identified it as the luminiferous ether: all bodies are imbued with it up to their smallest pores. The pores, moreover, were thought of as small elastic bladders that are connected to the outside through differently sized holes. When two objects are rubbed, the bladders of one compress more than those of the other and ether escapes from them, entering the pores of the other body. This causes a variation in the elasticity of ether, which tends to return to equilibrium, thus giving rise to electrical phenomena. With the artifice of open and closed pores, of ether that sprays out from some and is injected into others, Euler explained the electrical phenomena known at the time with difficulty, creating new makeshift hypotheses as he went along.

Like Newton's theory, which saw electrical phenomena as a particular case of the general attraction and repulsion of objects, Euler's theory had almost no influence on the scientific environment of the 18th century. Yet it is for this very reason that were discussed it here, to highlight the experimental seriousness of those first scientists, who did not allow themselves to be seduced by mental sleights of hand, even when proposed by famous men. A science in its infancy must verify facts through experiment; a theory is useful to connect facts, but it must itself be suggested by facts and not exist *a priori.*

7.28 Giambattista Beccaria

The importance attributed to the Leiden jar, which was hailed as a great scientific conquest when it was first developed, was certainly exaggerated. However, this exaggeration was providential, turning a few pioneers into a legion of physicists convinced that the study of electrical phenomena was worthy of a philosopher. The psychological effect was even greater when Franklin demonstrated the electric nature of lightning: electrical phenomena were no longer reserved for idle curiosity, they were powerful means for investigating the secrets of nature.

The enthusiasm that pervaded physicists was fostered by Franklin's letters published by Collinson. Seventeen years from their publication, Joseph Priestley (1733-1804), more famous as a historian of electricity, wrote: "Nothing was ever written on the subject of electricity, which was more generally read and admired in all parts of Europe, than those letters. Electricians everywhere employed themselves in repeating his experiments, or exhibiting them for money. All the world, in a manner, and even kings themselves, flocked to see them, and all returned full of admiration for the inventor of them."[405]

This research fervour spread everywhere and also struck the Royal Society, which, three years after having considered Franklin's letters unworthy of publication, awarded him the Copley medal and in 1756 elected him a member.

Among the numerous writings that followed the publication of Franklin's letters, a book published in Torino in 1753 deserves mention, as it was without a doubt one of the most well-organized and complete works on electricity at the time. It was titled *Dell'elettricismo artificiale e naturale libri due* and its author was the monk Francesco (of religious name Giambattista) Beccaria (Fig. 7.10),406 born in Mondovì on 3 October 1716. Beccaria moved to Rome in 1732, where he became a member of clergy at the Scuole Pie. After having taken on various teaching assignments in the peripheral schools of his order, he was recalled to the main school in Rome. Gaining fame as a mathematician, Beccaria acquired the respect of Father François Jacquier (1711-1788), who introduced him to scientific circles. In 1748, he was asked to teach physics at the University of Turin, an institution steeped in Cartesianism. In contrast, his teaching took a decidedly experimental and Newtonian approach. Among the numerous assignments he was given, including those of a practical nature, was the measurement of the length of a degree of the meridian. He completed this mission, along with his student Domenico Canonica (1739-1790), in fourteen years, from 1760 to 1774, during which he obviously had to slow down his original research into electricity, without however abandoning it completely, as we will later see. In 1770, he directed the erection of a lightning rod to protect the Duomo in Milan. His work on the meridian degree brought criticism and a bitter feud with Cassini. He died in Turin on 27 May 1781.

[405] J. Priestley, *The History and Present State of Electricity with Original Experiments*, London 1767, p. 154.
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Figure 7.10 - Giambattista Beccaria. Portrait taken from *Dell'elettricismo* (Macerata 1793).

The first book of the cited work deals with *artificial electricism*, that is, electrostatics, while the second book deals with *natural electricism*, that is, atmospheric electricity. The style of the work is reminiscent of geometry texts; the collection of known and novel experiments is detailed in a systematic way, tinged by the influence of Franklin's theory, with a clear orientation towards quantitative research and a mathematical treatment. Beccaria was an avowed Franklinian, though "with a broader view of phenomena" that brought him to introduce the concept of relativity of electrification, the first step towards the idea of an electrical state or potential.

Whereas Franklin had accepted electrical repulsion as an experimental fact, Beccaria rejected it: according to him the repulsion was only apparent, caused by the interplay between unbalanced attractive forces, in particular the attraction between all electrified bodies and the ambient air. Beccaria's efforts towards a quantitative electrology are exemplified less by his mathematical writing style and more by his experimental works. For example, he described his liquid manometer, today known as a "Kinnsersley thermometer", as follows: two connected tubes contain a coloured liquid; in one of these, which is closed at its top, a spark is produced above the level of the liquid using two iron tubes. At the same time as the spark, one can observe a rise in the level of the liquid in the other tube. Beccaria in vain tried multiple times to transform the manometer into a device for measuring the "quantities in electric phenomena". "I am attempting certain experiments," he wrote in this regard, "that give me hope that I will eventually determine the quantity involved in electric phenomena more exactly; but those who are aware the difficulties involved in measuring other, simpler phenomena that have been well-known for centuries will be persuaded by the difficulty of measuring electric ones, which, aside from the fact that we are just now coming to know of them, are certainly multifaceted and exhibit rapid and monumental changes in behaviour."[407]

In reality, the first difficulty was unfortunately an erroneous interpretation of the electric phenomenon, which he attributed to a mechanical effect: the breaking of the "electric vapour" at the moment that discharge occurs. The effect was re-examined by Ebenezer Kinnersley (1712-1778), a talented American whom Franklin had encouraged to present his electrical experiments at public conferences (with compensation) that Franklin held from town to town with great success. In 1761, in a letter to Franklin, Kinnersley attributed the phenomenon discovered by Beccaria to the heating of air caused by the spark. He supported this explanation by showing that the spark from an electric discharge can heat the conductors it strikes to the point that they glow red. Informed of his friend's discovery, Franklin carefully observed the effect of a lightning bolt that had struck a house, finding that the house's floor had burned down. He thus debunked the myth that had been repeated for centuries in philosophy books and that he himself had believed, that lighting melts metals without heating them: "cold fusion", as it was called.

Until Beccaria, physicists separated bodies into two classes: conductors, which all equally conduct electricity, and insulators, which all equally insulate. Beccaria's great contribution was that he demonstrated that such a marked distinction does not hold water: he thus introduced the idea of

[407] G. Beccaria, *Dell'elettricismo artificiale e naturale libri due*, Turin 1753, p. 120.

"electrical resistance" and began its first studies, demonstrating that water is less conductive than solid metals and mercury. In 1772, he also established the following notable proposition: "Metals that conduct more than any other body still carry some resistance, which is proportional to the length of the path that the spark must traverse in them."[408] Beccaria's experiments were repeated that same year, 1753, by Canton, who corroborated the variable resistance of bodies.

Another British physicist, Cavendish, whom we will discuss more thoroughly in § 7.32, produced so many spectacular successes in this field that, considering the modest equipment at his disposal, his scientific exploits appear almost magical. In a paper that appeared in 1776, the second and final one that he published on electrology, Cavendish clearly understood the concept of electrical resistance and almost anticipated Kirchhoff's laws, observing that if the two ends of a Leiden jar are connected to multiple conductors of varying resistance, current passes in all the conductors, but does so in greater proportion through the conductors of least resistance.[409] Noting that "metals conduct surprisingly better than the human body," he added that he measured iron wires to be four hundred million times more conductive than rainwater or distilled water, and furthermore, "Seawater, or a solution of one part salt in 30 of water, conducts 100 times and a saturated solution of sea salt about 720 times better than rain water."[410]

If these results are compared with the resistances of solutions given by Friedrich Kohlrausch and those of metals given by Augustus Matthiessen, one can see that at 11 °C, the ratio of the specific resistances of a saturated solution of sea salt and cast iron is exactly the one predicted by Cavendish. We also add that his unpublished papers recount that in 1781, after numerous experiments that lasted eight years, Cavendish was able to establish that electrical resistance is independent of current:[411] essentially the crux of Ohm's law. To obtain these results, Cavendish conducted two successive experiments on two circuits attached to the ends of a Leiden jar; one circuit was made up of his body, while the other was made up of one of the two conductors being examined: he judged two conductors to be of equal resistance when he felt the same electric shock in both experiments.

After this perhaps not useless digression, we return to Beccaria, who in the second part of the cited 1752 work described highly ingenious experiments

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[408] G. Beccaria, *Elettricismo artificiale*, Turin 1772, p. 134.
[409] H. Cavendish, *An Account of Some Attempts to Imitate the Effects of the Torpedo by Electricity*, in "Philosophical Transactions", 1776, pt. 1, pp. 196-225, later in Id., *The Electrical Researches*, edited by J. C. Maxwell, Cambridge 1879, p. 195.
[410] H. Cavendish, *The Electrical Researches* cit., p. 195.
[411] *Ibid.*, p. 333.

on atmospheric electricity, which allowed him conclude that the electrification of clouds can be both positive and negative.

Beccaria's biggest contribution to the study of electricity can be found in the *Lettere al Beccari*, published in Bologna in 1758 and considered a masterpiece by his contemporaries. After repeating Franklin's 1751 experiments, in which one obtains the magnetization of an iron wire or the inversion of polarity in a magnet by discharging a battery through the wire or magnet, Beccaria advanced the hypothesis that there is an intimate link between the "circulation" of the electric fluid and magnetism. He wondered whether it was the electric fluid that "with some universal, imperceptible, perpetual, and periodic circulation … produced and conserved all magnetic directions,"[412] a brilliant thought that earned admiration from Priestley, who wrote, "This is a truly great thought; and, if just, will introduce greater simplicity into our conceptions of the laws of nature."[413]

Nollet had tried to replace the Leiden jar with bottles full of pitch or wax without ever obtaining an electric discharge. Franklin too believed that, because of its internal structure, glass was necessary to produce the effect. This belief was widespread for years among experimenters, until in 1756 a young physicist, then unknown, observed that the only role of the glass in the bottle or Franklin's capacitor is to impede the passage of electricity from one end to the other. Therefore, "even without glass, with resinous bodies an electric discharge is possible: a conclusion that all the other authors that up until now have written on electricity agree to deny."[414]

Yet even before this seminal paper by Aepinus was published, which we will discuss in the next section, according to his first biographer Francesco Antonio Eandi (1735-1799), Beccaria had taught at the University of Turin that glass could be replaced with another insulator as early as 1754, substituting it with sheets of wax, sulphur, pitch, or rosin. This is confirmed by a manuscript detailing Beccaria's university lessons, of uncertain date but published after 1758, that is kept at the National University Library of Turin. In a brief chapter of the manuscript titled *De re electrica institutio unica*, Beccaria declares to have discovered that resinous and sulphurous bodies can perform the same function as glass in a Leiden jar.

It is also interesting to note that the first experiments on what Faraday later called specific inducting power were conducted by Beccaria. Indeed, in the fifth *Lettera al Beccari*, after having described a Franklin capacitor

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412 G. Beccaria, *Opere*, Macerata 1793, Vol. 2, t. 2, p. 139.

413 Priestley, *The History and Present State of Electricity* cit., p. 331.

414 F. U. Th. Aepinus, *Mémoire concernant quelques nouvelles expériences électriques remarquables*, in "Histoire de l'Académie royale des sciences et belles lettres", 1757, p. 118.

with a marble insulator, Beccaria added: "I have also proved that a similar layer of only well-polished peat activates it, to a lesser extent, but it activates it all the same. Furthermore, I have experimented on a layer of equal parts pitch and resin, and have found that it does not work as well as wax, but is better than sulphur or pitch alone. However, I confess that I have not conducted enough experiments to be sure of the correctness of the comparison."[415]

We will have other occasions to mention this very active Piedmontese scientist, who was a teacher to Lagrange, encouraged Volta in his early years, and marked the renewal of experimental research in Italy after a century of torpor.

7.29 Franz Ulrich Theodor Aepinus

After the observation cited in the previous section, Aepinus continued by saying that every insulating body, including air, placed between the two ends of a capacitor produces the same effect as glass. This conclusion, reached meditating on Franklin's theory, was well worth an experimental check. Aepinus achieved this using two parallel metallic plates placed a distance of 1.5 inches apart, one attached to the conductor of a generator and the other connected to the ground by a chain: the apparatus produced quite an electric shock, similar to that obtained with glass, though only provided the plates were large enough. Indeed, the larger the surface areas of the plates were, the larger was the electrical effect. Typically, Aepinus used plates of 7.5 square feet each.

The above description was a concluding digression in a paper whose principal aim was to announce a new electric discovery, later called *pyroelectricity*, which had so greatly amazed the young scientist that he began his paper with a hymn to nature, the inexhaustible treasure of marvellous facts.

Despite being naturally found all over Europe, *tourmaline*, a mineral that forms a rhombohedral crystal, was "brought" to Europe in 1717 by the chemist Louis Lemery (1677-1743), who described it as a rare stone originating from Ceylon (modern day Sri Lanka) and also called *trip.* As jewellers soon discovered, tourmaline has a curious property: when the stone is heated on embers, it oscillates between attracting and repelling the surrounding ashes. Because of this property, in 1747 Linnaeus called it *lapis electricus*, without, however, having confirmed the electrical nature of the effect. Aepinus too suspected it as soon as Johann Lehmann (1719-1767)

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[415] Beccaria, *Opere* cit., Vol. 2, t. 2, p. 54.

spoke of the crystal to him and showed him a sample. A series of expertly conducted experiments immediately confirmed the suspicion that the unusual phenomenon was of electrical nature, but certainly distinct from the common effect of electrification by rubbing. Tourmaline, on the other hand, becomes electrified through heating, and always has one side positively charged and the other negatively charged.

After the publication of his paper, Aepinus was the centre of a heated controversy involving, among others, Benjamin Wilson, Musschenbroek, and Johann Wilcke, a student of Aepinus, who questioned Aepinus' experimental results, in particular because these experiments, which are still quite delicate today when electroscopic powders (unavailable at the time) are not used, were not repeated with the same care with which Aepinus had first performed them. The controversy was quashed by Canton, who in 1759 read an intricate paper to the Royal Society in which he not only confirmed Aepinus' results, but added that electrification can also be obtained through cooling. The following year, in 1760, he discovered that this property of tourmaline is also found in Brazilian topaz, and later Wilson found it in other gems as well. Furthermore, in 1762, Canton demonstrated that the charges provoked by heating tourmaline are equal and opposite in sign. He reached this conclusion by immersing tourmaline in a metallic recipient containing boiling water that he connected to his electrometer (§ 7.31), which detected no charge.

In his 1803 treatise on physics (and the following editions up until 1821), the mineralogist René Haüy (1743-1822) gave pyroelectricity the description that it more or less has today, aside from some theoretical questions that are still not entirely resolved today. In addition, the important discovery that pyroelectric crystals can also be electrified with pressure (1817) is also due to Haüy, who observed that a two-faced piece of spar became positively charged when pressed between two fingers. This phenomenon, later called *piezoelectricity* and studied for the entire 19th century with numerous technological applications, was used by Haüy to build a highly sensitive electroscope.

Let us return to Franz Ulrich Theodor Aepinus, born in Rostock on 13 December 1724 and died in Dorpart on 10 August 1802. After studying mathematics and medicine in his birth town, he was named a member of the academy of sciences of Berlin in 1755 and the academy of sciences of Saint Petersburg in 1757. He became a professor of physics in the latter city, obtaining the protection and support of Catherine II and her successor Paul, to whom he became a counsellor in 1797.

Aepinus was above all a mathematician. He began his physics research almost by chance, as he himself recounts, his aspiration was to describe electrical phenomena using mathematical language. However, in an irony of fate, it was the mathematical part of his work that was completely sterile, while his experimental discoveries caused quite a stir among his contemporaries and played an important role in the further development of science. The apparatus described in the previous section, called the "air plate", was considered by Priestly as one of the greatest electrical discoveries since Franklin's time. Even greater, however, was the discovery announced in 1759 in his seminal work, published as a supplement in "Novi commentarii" of the academy of Saint Petersburg. Two sheets of mirror glass are rubbed against each other and then separated. A small cork pendulum is brought close to one of them and is first attracted and then immediately repelled. If the other sheet is brought further or closer to the first, the pendulum's oscillations respectively increase or decrease. The experiment always produces the same results no matter the nature of the sheets, as long as at least one of the two is made out of an insulating material. With this experiment, Aepinus demonstrated that metals too can be electrified by rubbing but, in reality, his original interpretation was different. According to him, when the insulating sheet is rubbed by the metallic one it becomes electrified and then cedes some of its electricity to the metallic sheet. This explanation was accepted by physicists until Ingenhousz, in a nice paper published in 1778 in "Philosophical Transactions", showed that even metals (provided they are isolated from other metals) can be electrically charged by rubbing, thus the fundamental difference between conductors and non-conductors is not their triboelectricity but simply their conductivity.

In the second chapter of his *Tentamen*, Aepinus describes an important experiment: a metallic rod is placed on top of two insulating supports, and two metallic fragments rest on the ends of the rod, each tied to a silk string. When an electrified glass tube is brought near the rod, the metallic fragments, lifted by the silk strings, become electrified, with the one closest to the glass tube becoming negatively charged and the other positively charged. An identical effect is obtained with a shaft of electrified sulphur, with the only difference that the sign of the fragments' charge is switched.[416]

In a paper that appeared that same year, 1759, but was written after the *Tentamen*, the explanation of the experiment is repeated with a modification

[416] F.U.Th. Aepinus, *Tentamen theoriae electricitatis et magnetismi. Accedunt dissertationes duae, quarum prior, phaenomenon quoddam electricum, atera magneticum, explicat*, Saint Petersburg, undated (from the inscription October 1759), pp. 64-66.

that has essentially remained to this day: the electrical state of the fragment further from the sulphur or glass shaft is detected using a small pendulum.[417]

Even more original than the experiment itself was the interpretation Aepinus gave of it, a genuine turning point in the theory of electrostatic influence, which, having always been considered a form of electrification by communication, had misled all previous electric theories. According to Aepinus, the explanation of the phenomenon was simple: an electrified body A placed near a discharged conductor B either repels or attracts (depending on whether it is positively or negatively charged) the electric fluid inside B, so B appears electrified and of the same charge as A in the part further from A, and of the opposite charge in the part closer. Therefore, because of the interplay of electric forces, the same conductor can at the same time be both positively and negatively charged: something that to Nollet, who had observed the phenomenon in 1752, seemed "plus qu'incroyable".

In the last chapter of *Tentamen*, Aepinus reasserts his aversion to the idea of electric and magnetic atmospheres, though admitting that at very short distances from an electrified body, the ambient air, which is an imperfect insulator, can be somewhat electrified itself. However, the thin insulating layer around bodies has little to no influence on the electric phenomena, as is demonstrated by observing that the phenomena occur in exactly the same way if the layer of air around electrified bodies is removed using a slight wind produced from a fan.[418]

7.30 The electrophorus

The mechanisms for electrostatic influence described by Aepinus (§ 7.29) could also have very well been used, against the scientist's own intentions, as legitimate evidence in favour of the dual fluid theory, proposed that same year by Symmer (§ 7.27). A dispute therefore emerged between the supporters of the two theories involving, among others, Giovanni Francesco Cigna (1734-1790), a former student of Beccaria and a somewhat famous physicist at the time, partly because, along with Lagrange and Giuseppe Angelo Saluzzo, he founded a private society in Turin that in 1783 was became the current academy of sciences of Turin. Among Cigna's experiments, the following was particularly noteworthy: when an electrified silk ribbon and a finger are brought close to an insulated sheet of lead, a

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[417] F.U.Th. Aepinus, *Descriptio ac explicatio novorum quorundam experi-mentorum electricorum*, in "Novi commentarii Academiae scientiarum petropolitanae", 7, 1758-59, pp. 277-302.
[418] Aepinus, *Tentamen theoriae electricitatis et magnetismi* cit., pp. 357-58.

spark is produced between the sheet and the finger, while the silk ribbon is attracted to the sheet. When the ribbon is then detached from the sheet, it has the same charge as before and the sheet is oppositely charged. The sheet can then produce a new spark, and the procedure can be repeated as many times as one wishes. Cigna, for example, succeeded in charging a Leiden jar by using a single series of sparks from the detachment and adhesion of the ribbon and the sheet. Based these experiments (which he later cited as evidence of his priority in the invention of the electrophorus), Cigna concluded that electrical phenomena could be explained both with Franklin's theory and Symmer's theory.

Cigna's experiments pulled Beccaria back to electric studies, as he opposed the conclusion of his former student and (already for some time) current enemy, first in a letter to Franklin dated 1767, then in a booklet published in February of 1769 titled *De atmosphaera electrica*, and lastly in another booklet published a few months later: *Experimenta, ataque observationes, quibus electricitas vindex late costituitur, atque explicatur*. According to Beccaria, when to nonconducting sheets or one nonconducting and one conducting sheet that are oppositely charged come into contact, they annihilate their opposing electricities, but each of them regains its electricity when they are once again separated: he called this property *vindictive electricity*.

A young Volta argued against this theory in his first published work, titled *De vi electrica ignis electrici ac phaenomenis independentibus* (1769), written in the form of a letter directed at Beccaria. This is an important paper to understand the subsequent evolution of Volta's thought; in retrospect, one could say that it contained the seed of all of his later works. In it, Volta observed that the attractive forces between matter and the electric fluid depend on the relative positions of their constituent particles. Rubbing, striking, and electric atmospheres all alter the collection of forces, thus they alter the equilibrium of electrical forces and the natural saturation of the body. From this structural, microscopic, and undoubtedly machinistic theory, Volta draws, among other conclusions, the persistence of charges on insulating laters, thus explaining the effects observed by Beccaria as instances of electrical influence. Indeed, such phenomena were studied with great care and a new mentality.[419]

The fruits of the new formulation of electric problems did not take long to ripen. On 10 June 1775, Volta could write to Priestley: "I present you a body that, electrified only once for a short time, and not even strongly, never loses its electricity, obstinately conserving its active signed force despite

[419] The first Italian translation of Volta's interesting paper can be found in A. Volta, *Opere scelte*, edited by M. Gliozzi, Utet, Turin 1967, pp. 45-100.

innumerable touches."[420] Volta then described the apparatus, which he baptized a "perpetual electrophorus", a device famous enough that we consider its description superfluous.

The success obtained by the new apparatus was nearly limitless, and electrophoruses were built everywhere: some were small, permitting disassembly, and contained in cases that were easy to transport; others were enormous, to the extent that their plates (which could reach two metres in diameter) had to be lifted by pulleys. With its success came also its detractors, who, as often happens in these cases, could only contest the novelty of the invention, claiming that the meat of the apparatus was found in an experiment described by Aepinus in *Tentamen*: when an insulated metallic cup was filled with melted sulphur and allowed to cool, Aepinus observed that both the cup and the sulphur were oppositely charged each time they were separated.[421] However, this criticism quickly died down, perhaps less because of Volta's honourable reply and more because of the obvious question that could have been asked of any critic: if the invention had already existed for sixteen years, why had you not realized it?

As Volta himself admitted, it was not Aepinus, whose work was still unknown to Volta when he built the electrophorus, but Cigna who gave him the idea for the invention. However, there is a sizeable gap between Cigna's experiment that we described above and the electrophorus.

The electrophorus was the prototype of a new type of electrostatic machine that is more efficient than the rubbing-based one, the induction generator. This type of generator began as Giuseppe Belli's (1791-1860) "duplicator" in 1831 (though built in 1828), followed by more practical machines built in 1865 by August Topler (1836-1912) and the following year by Wilhelm Holtz (1836-1913) with his *Whismhurt*. The long history of the inductive generator continues well into the 20th century with its numerous improvements and applications.

Out of all the research generated by the electrophorus, we note the work of Georg Christoph Lichtenberg (1742-1799) on the figures that are named after him and represent the first studies of semiconductors; the use of electroscopic powders (also in the course of Lichtenberg's studies); the construction of Abraham Bennet's "multiplier" (different from Belli's duplicator), which, following the criticisms levelled by Tiberio Cavallo, was improved by William Nicholson (1753-1815) in 1788. Nicholson built an instrument that, as Volta wrote, "by means of the rotation of a handle produces the two electric states, without friction or communication with the ground." The apparatus, which was a sensitive detector of small charges

[420] A. Volta, *Le opere*, national edition, Hoepli, Milan 1918-20, Vol. 3, p. 95.
[421] Aepinus, *Tentamen* cit., pp. 66-67.

than an induction generator, was very useful in Volta's studies on contact electricity (§ 7.36).

7.31 The electroscope

The small fragments attracted to rubbed amber can be thought of as the first electroscopes, replaced by Gilbert with the versorium (§ 3.21), by Guericke with a feather, and by Hauksbee with strings hanging from a semicircular metallic sheet. The simple electric pendulum appeared much later, after 1740: Johann Winkler describes it in his 1746 work. A little later, in an anonymous work that is typically attributed to Eusebio Sguario, the pendulum becomes a ball-based electroscope made up of two small cork sphere that almost touch, hanging from two hemp strings.[422] Nollet projected the strings onto a graduated scale and, that same year, 1753, Canton thought it opportune to protect the electroscope from possible air currents when preparing himself to study atmospheric electricity, and therefore enclosed it in a small box with glass walls and a sliding cover. He measured the sign of the electrification of clouds based on the extent of the separation of the two spheres when they were brought near a piece of electrified amber (or wax). The use of this more convenient instrument allowed the British scientist to discover that wax can be both positively and negatively electrified, and that when polished glass is rubbed with a woollen garment, it acquires resinous electricity. Therefore, the maxim passed down by Du Fay that a body can only have has a single type of electricity crumbled, paving the way for Symmer's dualistic theory, which differed from the Frenchman's only in that respect.

Canton's electroscope remained unchanged for almost twenty-five years, until in 1777 Cavallo set out to improve it: he hung the two pieces of cork by two thin sliver strings folded into a ring and threaded through two nearby holes in an ivory plate; he hung the plate containing the strings from a metallic cap inside a glass cylinder, itself on top of a wooden base; and he glued two strips of tin foil to the inside of the glass walls that were connected to the base, which the pendulums touched, enabling discharge when they were strongly electrified. A further improvement was made in 1783 by De Saussure, who shortened the pendulums and hung them directly from the metal cap, eliminating the ivory sheet, replaced cork with elder wood, and changed the glass container into a bell shape. Lastly, he made the base metallic instead of wooden, with the aim of completely discharging the

[422] E. Sguario, *Dell'elettricismo o sia delle forze elettriche de'corpi svelate dalla fisica sperimentale con un'ampia dichiarazione della luce elettrica. Sua natura e maravigliose proprietà.*, Venice 1746, p. 193.

apparatus when one places a hand on its base and a hand on the cap. On the lower border of the bell, he glued a graduated scale for measurements.

The improvements made by Cavallo and De Saussure pushed Volta to repeat his studies of electric meteorology, which after the first decades of observation had seemed an exhausted field that attracted physicists' attention less and less, both because of the poor sensitivity and inconvenience of the instruments and because of the impossibility of quantitative assessments. Volta thus recommenced his studies on electric meteorology with two clear goals: increasing the sensitivity of the instruments and making their observations comparable to each other. In his mind and work, therefore, electrometry became a preliminary introduction for electric meteorology.

The nine *Lettere sulla meteorologia elettrica*[423], written between 1787 and 1789 and addressed to Lichtenberg, lay out the principal results of these new studies. Volta improved the electroscopes then in use with a modification "that appears a trifle and yet is of the greatest importance and consist in changing the force and matter of the pendulums, eliminating the balls of elder wood or something else, and replacing the thin metallic strings with two bare straws about five inches long, which are hung by very thin rings and nearly touch each other along their entire length." In the letters, Volta identified many advantages of the new instrument, chief of which is the uniformity of scale; he explained how to construct electrometers with straws of different but comparable sensitivities; he found Henley's quadrant electrometer scale (which we will soon discuss) to be uniform between 15° and 35°; and he insisted on the usefulness of having four measurement instruments at ones disposal (two straw electrometers and two quadrant electrometers) whose measurements form pre-determined ratios.

We hold our discussion of the coupling of the electroscope and the capacitor for now (§ 7.32). Here we note that in 1786, Volta lit a flame at the end of a pole attached to an electroscope, as Bennet had independently done a few months earlier, transforming the apparatus into an electrometer particularly suited to meteorological observations because, as Volta observed, the flame acquires the same potential (as we would say) as the ambient air. The flame electroscope spread rapidly, and its use in meteorological observatories peaked again in the second half of the 19th century after William Thomson (later Lord Kelvin) suggested its use, explaining that the flame condenser, like his own water-dropping condenser, quickly attains the potential of the ambient air.[424]

[423] Volta, *Le opere* cit., Vol. 5, pp. 29 et seq.

[424] R*eport of the Twentieth Meeting of the British Association for the Advancement of Science*, 1859 (*Transactions of the sections*, p. 27).

Volta's greatest achievement in the field of electrometry, however, remains his absolute measurement of the "fundamental degree of tension". After the scales of first one, then many different electrometers are made comparable to each other, a further, more difficult problem remains: making all electrometers comparable, no matter who uses them or where they are used. Research on an international system of measure (which, facilitated by the French Revolution, led to the decimal metric system) was not specific to electrology, occurring in all science and, on a broader scale, society of the time: a common system was an urgent necessity for commercial relations and became vital for scientific progress as well. An active debate among scientists continued for over a century, with two fundamental tendencies coming to the forefront. The first suggested to choose, through an international agreement, a prototype sample for every physical quantity; the second, worried that the first solution could not guarantee that the sample would be conserved indefinitely, suggested another solution: finding a "universal" or "natural" measure, that is, one founded on immutable physical laws, and thus, to quote the first president of the Royal society Tovert Moray, "such that it can be made exactly equal in all locations, without first having to compare notes."[425]

De Luc proposed the first solution for the unit of tension (or voltage, as it is called today in homage to Volta) and Volta accepted: a sample electrometer that, by international accord, all physicists could use as a reference, had to be created. After some time, however, Volta realized that there was a better solution, namely that of defining an international unit of tension using measurements of geometric and mechanical quantities that are easily accessible to experiment. After having experimented with balances and plates at length, in the end Volta declared his preference for a device exploiting the attraction between a conducting plane and a movable plate hanging from the beam of a balance. With this setup, the movable plate is charged, while the underlying plane lies on the ground. Volta experimentally found that the attraction between the plate and the plane is inversely proportional to the area of the plate and the square of its tension. He therefore proposed to defined the fundamental "degree of tension" as the tension that in a 5 inches diameter plate placed 2 inches above a conducting plane produces an attraction of 12 grains (6.3732 g).[426] Giovanni Polvani repeated Volta's experiment and found that the "fundamental degree" is equal to 44.5 absolute electrostatic units, equal to 13,350 Volts.[427]

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[425] Letter from Moray to Huygens dated 23 December 1661, in Huygens, *Œuvres complètes* cit., Vol. 3, p. 427.
[426] Volta, *Le opere* cit., Vol. 5, pp. 79 et seq.
[427] G. Polvani, *Alessandro Volta*, Domus Galileana, Pisa 1942, p. 145.

While Volta was concerned with his straw electrometer, British physicist and clergyman Abraham Bennet (1750-1799) was building the gold-leaf electrometer that he described in a 1787 paper in "Philosophical Transactions"; at the same time and independently, Anton Maria Vassalli built a similar instrument (1761-1825; after 1799 he added his uncle Eandi's last name to his own). Volta received news of Bennet's electroscope in September of 1787, read its description in 1788 and immediately recognized a "great advantage in such an instrument", which despite not having the "advantage of being comparable" like his own straw electroscope, was very sensitive. "For this reason, instead of electrometer, it should be called an electroscope." As evidenced by this statement, we believe that the systematic distinction between electrometer and electroscope began with Volta.

To conclude our history of the electroscope, which was truly a *deus ex machina* of 18th century electrology, we add that in 1801, William Pepys (1775-1856) added two thin metal rods next to the gold leaves to increase the sensitivity of the instrument. The walls of the containers surrounding the "gold" leaves were made of glass until the theory of potentials realized that they should be metallic and grounded. This improvement also sheds light on the ancientness of many of the glass electroscopes still in use in schools.

7.32 Measures of capacitance: Cavendish and Volta

If one considers the overall direction of electrical studies in the thirty years after Franklin's work, a new mentality is evident. Electrical phenomena began to lose their initial appearance as separate curious facts and became a unified framework that theories attempted to explain using a minimal number of principles. Physics went from qualitative to quantitative and started to distinguish and define quantities, connect them with mathematical relations, and measure them with instruments that were pushed to be ever more sensitive and precise.

This research direction can be detected as early as Beccaria's first works (§ 7.28), becomes explicit in Aepinus' *Tentamen*, and attains full maturity with Cavendish and Volta. Aepinus based his mathematical treatment of electricity on the following principles; every body has a given amount of electricity in its natural state; the particles of the electric fluid repel each other and are attracted by ordinary matter; electrical phenomena manifest themselves when a body has more or less electric fluid than in its natural state. Analogous principles hold for magnetism, since one of the two cornerstones of the Aepinian approach is equality between electric theory and magnetic theory, as the title of his work itself indicates. Aepinus then moved on to an analytic treatment supposing the forces between electric charges to be proportional to the charges themselves, but independent of their distance and distribution inside the conductors. That the forces indeed do depend on distance is a trivial observation and was also known to Aepinus. However, because he did not know the law governing this variation, he chose to ignore it, though it appeared to him that the force should obey an inverse square law for the economy and harmony of nature. Without delving into other details, it is easy to see why the analytical part of Aepinus' theory is worthless today. Nevertheless, at its time it served the important function of directing further study.

Cavendish too, having read Aepinus after writing his first electrology paper in 1771, admitted Aepinus' hypotheses and added another one of great importance: the attraction between two charges is inversely proportional to an unspecified power of their distance. This hypothesis implies that electrical action extends to infinite distances, while both the theoretical ideas of the time, in particular that of "atmospheres", and everyday experience held that the opposite was true: electricity can only be manifested in the space immediately surrounding the electrified body, though with some variability within this space. Cavendish derived the mathematical consequences of his new hypothesis, eliminated those that appeared absurd to him, and concluded that electrical forces must act with an intensity inversely proportional to a power of the distance, where such power is less than three.

In the second part of the paper, Cavendish tackled the problem from a completely new point of view. He studied how the electric charge is distributed in a charged conducting disk or sphere, intuiting that this distribution depends on the laws of attraction and repulsion in the electric fluid. From the results and the hypotheses admitted in the first part and using interesting mathematical manipulations, Cavendish reached the conclusion that if the force between electric charges obeys an inverse square law with distance, "almost all" electric charge must be confined strictly to the surface of the conductor: an indirect path towards the discovery of the law had been created.

At the time, experiments on the distribution of electricity in conductors had already been conducted, and Cavendish was certainly aware of them. As early as 1753, Beccaria, following Gray, had observed that a solid cube and a hollow one with the same charge attract equally; Franklin had designed the experiment (still done today in some schools) of the chain that, when less extended, appears more electrified; in 1755, the same scientist announced another unique experiment: when a small electric pendulum held by an insulating support is placed inside a charged silver vase, the pendulum is neither attracted by the sides nor becomes charged when it touches the bottom. Neither Franklin nor Priestley could explain the phenomenon, but the suggested that perhaps the experiment could be explained by assuming that electric actions follow an inverse square of distance law. Because of this suspicion, physicists and especially historians from Britain sometimes call the law of electric attraction and repulsion "Priestley's Law".

Beccaria explained Franklin's effect (in the meantime, Benjamin Franklin had added the observation that Volta later independently made as well, that a charged pendulum loses its charge when it comes into contact with the bottom of the vase) in the cited *De atmosphaera eletrica* (1769) and repeated his explanation in *Elettricismo artificiale*. After building the *electric well* and the *saggiatore*, an electroscope made up of two small pieces of paper hanging from a wax rod, Beccaria observed that,, even if the well was significantly charged, the electroscope gave no reading when it touched the inner sides of and the bottom of the well, so he rightly concluded (as Faraday later did from analogous experiments[428]) that "every electricity [charge] is reduced to [remaining on] the free surface of bodies, without spreading at all into their interior."[429]

Let us leave this topic for now (we will return to it in the next section), and return to our analysis of the second part of Cavendish's paper, in which the scientist stumbled upon the idea of potential, defined as a quantity that takes equal values at all points of one or multiple connected conductors. "If several bodies," Cavendish wrote, "are insulated, and connected together by conducting substances, and one of these bodies is positively or negatively electrified, all the other bodies must be electrified in the same degree."[430] Cavendish did not give a name to this quantity in the paper, but in his unpublished *Thoughts on Electricity*, he called it "compression": "Let any number of bodies which conduct electricity with perfect freedom be connected together by substances which also conduct electricity. It is plain that the electric field must be equally compressed in all those bodies, for if it was not, the electric fluid would move from those bodies in which it was more compressed to those in which it was less compressed till the compression became equal in all."[431] He then continued, writing that compression should not be confused with the quantity of electricity

[428] §3.18 in: M. Gliozzi, *A History of Physics over the Last Two Centuries.* Cambridge Scholars Publishing, in press 2022.
[429] Beccaria, *Elettricismo artificiale* cit. p. 193.
[430] H. Cavendish, *An Attempt to Explain Some of the Principal Phaenomena of Electricity by Means of an Elastic Fluid*, in "Philosophical Transactions", 1771, pp. 584-677; later in Id., *The Electrical Researches* cit., p. 45, which we follow.
[431] *Ibid.*, p. 95.

(charge), as two bodies can have equal compressions but different amounts of electric fluid.

With the concept of potential, Cavendish also comes across that of capacitance. Here we note that the term "capacity" had been used by the Cavendish and Volta's predecessors in its ordinary meaning: the total amount of electricity that a conductor can contain, as today one might speak of the capacity of a container that stores liquid. The concept expressed by Cavendish in the following passage is quite different: "The shock produced by the Leyden vial seems owing only to the great quantity of redundant fluid collected on its positive side, and the great deficiency on its negative side; so that if a conductor was prepared of so great a size, as to be able to receive as much additional fluid by the same degree of electrification as the positive side of a Leyden vial, and was positively electrified in the same degree as the vial, I do not doubt but what as great a shock would be produced by making a communication between this conductor and the ground, as between the two surfaces of the Leyden vial, supposing both communications to be made by canals of the same length and same kind."[432]

The lack of proper terms to refer to potential and capacitance (both words were introduced by Volta) is certainly a weakness of Cavendish's electrostatics, which probably appears rather hard to understand to the modern reader, among other reasons because the "charge" of which Cavendish spoke is often actually capacitance, making it is easy to confuse the two. However, the two concepts were clear to the scientist, as evidenced by both his unpublished manuscripts and his second and last paper on electricity, published in "Philosophical Transactions" in 1776, in which Cavendish likens the electric discharge of a torpedo to that of a large, weakly-charged battery made of Leiden jars. The two quantities, which Volta later called tension and capacitance, played a critical role in Cavendish's approach, and he even devised an original way to measure the capacitance of a battery: the repeated contact method, which Wilhelm Weber and Rudolph Kohlrausch also employed in the 19th century in their classic works on the ratio between electromagnetic units. After charging a Leiden jar and noticing the subsequent divergence *a* of a connected electrometer, Cavendish divided the charge in two equal parts by connecting the first jar to a second identical one and measured the new divergence *b* of by the same electrometer. Having done this, he charged a series of jars until the electrometer diverged by *a*; he then repeatedly touched the battery with a small plate that partially discharged it at every contact. He observed that between 11 and 12 contacts, 11¼ to be precise, were needed for the charge

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[432] *Ibid.*, p. 56.

of the battery to halve, that is, for the divergence of the electrometer to become *b*.

Now, if the charge on the plate and the charge in the battery are in an x : 1 ratio, then the charge of the battery is reduced by a fraction of $1/(1 + x)$ with every contact. After 11¼ touches, it will be reduced by a factor of

$$1 : (1+x)^{(11+\frac{1}{4})}$$

thus, one can write

$$(1+x)^{(11+1/4)} = 2$$

from which x can be calculated.

Using this method, Cavendish found that his battery of 49 Leiden jars had a capacitance of 321,000 "inches of electricity", that is, using his terminology, the capacitance of his battery was that of a sphere 321,000 inches in diameter.[433]

To better understand Cavendish's terminology, one must resort to his unpublished papers. After having established that the capacitance of a conductor depends on nearby conductors, Cavendish gave this definition of the unit of capacitance: "The quantity of electricity on a globe one inch in diameter, placed a great distance from any other body, shall be called a globular inch."[434] Today, the radius is preferred in defining of the unit of measure of capacitance, so *n* inches of electricity in Cavendish's terminology correspond to $n/2$ inches of capacitance in modern terminology.

The unpublished manuscripts, which for imperfection, modesty, or misanthropy were not sent to printers by Cavendish,[435] include numerous theoretical and experimental works on capacitance: Cavendish measured the capacitance of cylinders of various dimensions; he determined the ratio between the capacitance of square and a sphere of equal side length and radius, respectively; he found the capacitances of a square and rectangle of the same area to be equal (in contrast to Volta, who, in agreement with the modern mathematical theory, found the capacitance of the former to be

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433 Cavendish, *An Account of Some Attempts to Imitate the Effects of the Torpedo by Electricity* cit., pp. 202 et seq., 299.
434 *Ibid.*, p. 347.
435 They were first published in 1839, followed by the cited collection edited by Maxwell, reprinted as H. Cavendish, *The Scientific Papers*, The University Press, Cambridge 1921.

greater than that of the latter); and he gave a formula for the capacitance of a cylindrical capacitor.

In addition to the aforementioned method of repeated contacts, capacitance comparisons were also done by comparing the capacitances of two bodies *B* and *b* with that of a third, which Cavendish called the "test plate", made up of two rectangular metallic surfaces sliding on top of each other. First, two Leiden bottles are charged by the same electrostatic generator. Then, the positive side of one is connected to *B* and the negative side of the other is connected to the test plate, whose surface is adjusted such that when it is placed in contact with *B*, the system as a whole becomes uncharged. The same procedure is repeated using *b*, and if the requisite surface area of the test plate is the same as before, then the bodies *B* and *b* have the same capacitance. In most cases, obviously, the surface areas are not the same: if they are in a ratio of t^2/T^2, then Cavendish concluded that their capacitances are in a t/T ratio, and thus that the capacitance of the plate is proportional to the square root of its surface area.[436]

Beccaria's experiments on specific inductive power (§ 7.28) were perhaps known to Cavendish because they were referenced in Priestley's *History*, with which he was certainly familiar. However, the works that were unfortunately buried in the British scientist's unpublished manuscripts greatly surpassed those of Beccaria. Observing that there is an appreciable difference in the charge on two identical plates depending on the type of insulator placed between them (flint glass, crown glass, shellac, resin, beeswax), Cavendish set up a series of experimental measurements of capacitance, comparing circular air capacitors with circular capacitors having other insulating substances between their plates. In this way, he obtained results that could even be called surprising given the extremely modest instrumentation Cavendish had at his disposal. For instance, he found that the specific inductive capacity of vulcanite is 2.21-2.76 (using air as a reference), while modern data gives 2.7-2.9; for paraffin his values were 1.81-2.47, compared to the modern 2-2.3, and so on for other materials.[437]

It is truly a great misfortune that these first-class pages of sciences remained unpublished, and therefore sterile, until 1839, when the first unpublished writings started to appear in print. A shy, solitary thinker, Cavendish was born in Nice on 10 October 1731, enrolled in Cambridge in 1749, and left in 1753 without obtaining any degree. He then moved to London and lived a frugal and reclusive life without ever leaving the city, dedicating all of his time to science, and making important contributions in

[436] Cavendish, *The Electrical Researches* cit. pp. 115-20.
[437] *Ibid.*, pp. 181-88.

the field of mechanics (§ 7.8), electrology, and chemistry, a discipline in which he is remembered for his discovery of hydrogen in 1766 and his successful synthesis of water in 1781. He died in Clapham on 24 February 1810.

The ideas and associated measures of potential and capacitance spread throughout 18th century science because of Volta, whose work is, in our view, linked to Cavendish's earlier insights.

A new concept arose from the complicated theory outlined in Volta's first paper (§ 7.30): the natural saturation of a body, that is, its electric state or potential, as we call it. The concept was still vague, and underwent a conspicuous metamorphosis with the influence of Cavendish's thought to become more precise. Indeed, in a letter dated 16 July 1773 that was perhaps addressed to the abbot Carlo Amoretti (1741-1816) and today is kept at the British Museum in London, Volta asked to consult the editions of "Philosophical Transactions" starting from 1770 to bring himself up to date on what had been published regarding electric atmospheres. Though there is no direct evidence, it is likely that Volta obtained the volumes of "Transactions" and read Cavendish's 1771 paper, which Beccaria too had admired and considered superior to the "still very worthy" work of Aepinus. Furthermore, Volta had certainly read Cavendish's 1776 paper because he cited it both in a letter to Madame Lenoir de Nanteuil and in his own 1802 paper on the equivalence of electric fluid and galvanic fluid. Reading these works and reflecting on his own ideas, the *naturalis saturias* of 1769 evolved into *tension*, which he defined practically in a famous 1782 paper: "With the term tension, I denote … the effort that each point of the electrified body makes to discard its electricity and communicate it to other bodies; to which tendency the electrical phenomena of attraction, repulsion, and especially the degree of elevation of an electrometer correspond,"[438] and, moreover, "the language of the electrometer is out of all the most meaningful, and I dare to say that only it can explain all the phenomena detailed in this work, as well as infinitely many analogous ones."[439] The precise definition of potential or tension (both terms are still in use today) and the recognition that its proper instrument of measure is the electrometer were the two key original contributions of Volta's electrology, which marked a leap in quality in 17th century electrology.

Following in Cavendish's footsteps, Volta introduced a new quantity, electric capacitance, which he defined so precisely that both the term and core of the definition remain unchanged to this day: "the greater the fluid

[438] Volta, *Le opere* cit., Vol. 3, p. 286.
[439] *Ibid.*

necessary to make the electrometer elevate to a given tension, the greater the capacitance of the conductor."[440]

In the paper *Osservazioni sulla capacità de' conduttori elettrici*, published in 1778 as a letter to De Saussure (1740-1799), Volta experimentally demonstrated that for equal lateral surface areas, longer cylindrical capacitors have greater capacitance. Armed with this result (made possible by Beccaria's experiments with the electric well), Volta put Cavendish's theoretical project into practice: he built a 96 feet long (about 31 metres) "solitary" (isolated) conductor with the same capacitance as a Leiden jar and charged to the same tension, and received from it the same electrical shock, leading him to conclude that the electric shock effect depends only on the capacity and tension of the conductor.[441]

Volta had two main methods of comparing capacitances, both less refined than Cavendish's. The first consisted of supposing the capacitances of two bodies to be proportional to the number of turns of an electric generator needed to bring them to equal tension. This method, considered *very fallacious* by Cavendish,[442] gave excellent results when used by Volta. The second method consisted of supposing the capacitances of two bodies to be equal if, when brought to the same tension, they gave the same electric shock.[443] Alongside these two principal techniques, Volta also had other subsidiary methods. For example, he would sometimes charge a conductor and a Leiden jar to equal and opposite tensions and then connect them: if the two bodies had the same capacitance, at the end of the process they would both be uncharged. Other times, he would charge a conductor and then let it discharge onto a second uncharged conductor, if the tension of the first conductor was halved, then the two bodies had the same capacitance.

After solitary conductors, Volta moved on the study of "combined" conductors, that is, conductors in each other's presence, the fundamental problem of electrostatics. For this new study, he was inspired, as one can glean from his unpublished manuscripts, by Aepinus' two plate experiment, but for unknown reasons, when he published his results in the 1782 edition of "Philosophical Transactions", he chose to derive his studies from the electrophorus instead. The key experiment is one of Volta's greatest: a metallic plate with a "rather thin coating of resin" is overlaid with another metallic plate with an insulating handle; when this plate is connected to a

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[440] *Ibid.*, p. 291.
[441] *Ibid.*, p. 199 et seq.
[442] Cavendish, *The Electrical Researches* cit., p. 204.
[443] For further assessments and details on Volta's measurements of capacitance, see M. Gliozzi, *Consonanze e dissonanze tra l'elettrostatica di Cavendish e quella di Volta*, "Physis", 11, 1969, pp. 231-48.

source of electricity for some time and then disconnected, the experimenter lifts it by its insulating handle: the tension of the plate increases significantly. The "condenser of electricity", as Volta called it, "to use a simple and plain term," was thus invented.

In the second part of this paper, whose title Antinori later shortened to *Del condensatore*,[444] Volta declared that "tension must be inversely proportional to capacitance,"[445] and experimentally demonstrated this statement, verifying that if metallic rod charged to 60° of an electrometer (using equally spaced degrees, added Volta) is touched by another rod of similar thickness but six times the length, then its tension falls to 10°. More explicitly, in *Lezioni compendiose sull'elettricità*, published after 1784, Volta wrote: "The quantity [of fluid is] proportional to the product of the intensity and the capacitance."[446] Translated into our modern mathematical languages, this proposition implies

$$Q = CV,$$

a fundamental relation of modern electrostatics.

Using ingenious techniques, Volta was able to measure the ratio between the capacitance of a capacitor and that of a single one of its plates: with his most frequently used 10 inches (around 27 cm in diameter) capacitor, he obtained a value of 120, but Volta observed that this ratio varies greatly depending on many circumstances, like the nature and physical conditions of the dielectric between the plates and the "celerity of the operation." In listing factors that affect the ratio, however, he missed the diameter of the capacitor. After this experimental investigation, Volta naturally wondered whether the tension differences he measured could be increased by connecting the capacitor plate directly to the electrometer to form a single apparatus, as opposed to two separate ones. Employing the relation $Q = CV$ and thinking, as he preferred, about concrete numerical examples, he shoed that the combination of the two instruments, electroscope and capacitor, into a single one is not only more convenient but also advantageous in other ways. Therefore, starting in 1783, he screwed a metal plate to the top of an electrometer, and after a great deal of trial and error, settled on using his hand inside a taffeta bag as the second conducting plate. The now familiar device with two small plates separated by an insulating sheet and screwed into an electroscope, sometimes called the Volta *electroscope-capacitor*, appeared much later: Volta described it in a letter

[444] The original title was *Del modo di rendere sensibile la più debole elettricità sia naturale sia artificiale*, in Volta, *Le opere* cit., Vol. 3, pp. 269 et seq.
[445] *Ibid.*, p. 286.
[446] *Ibid.*, Vol. 4, p. 419.

to Lorenzo Mascheroni (1750-1800) dated 23 March 1799, just a few months before the invention of the Voltaic pile.[447]

7.33 Charles-Augustin Coulomb

It is clear from what we have related in the previous section that in the decade following 1770, the law of electrostatic attraction and repulsion was in the air. Nevertheless, direct experimental evidence beyond a reasonable doubt was lacking.

The law governing magnetostatic actions was in an analogous situation, with science at the brink of its discovery too. As early as 1641, Father Athanasius Kircher used a hanging scale to measure the attractive force of a magnet: he hung and balanced a magnet to one side of the scale, attached the magnet to a fixed iron bar, and gradually added weight to the other plate of the scale until the magnet detached from the bar.[448] This method evolved throughout the century until it was used to measure the attractive force of a magnet hanging from a scale on another magnet or piece of iron placed at various distances below the first. The reverend Michell, whom we have already mentioned (§ 7.8), published a treatise on magnets in which he maintained that magnetic attraction and repulsion are equal and both vary in proportion to the inverse square of the distance between the two magnetic poles.

Michell assured that this law of his had been experimentally derived. Other experimenters, however, were not able to verify this using Kircher's techniques. The reason for this was discovered by Giovanni Antonio Dalla Bella (1730-1823), an Italian physicist living in Lisbon who, having conducted a series of experiments in 1781 using Kircher's device, described and explained them in two papers read to the academy of sciences of Lisbon in 1782, but unfortunately only published in 1797, in the academy's first collection of memoirs. To determine the pole of a magnet, Dalla Bella placed a needle on its horizontal surface and looked for the direction in which it aligned itself. In this way, he realized that the magnetic force emanates from a point in the interior of the magnet and not on its surface. Therefore, concluded Dalla Bella, the distance between magnetic poles should not be measured from surface to surface, but from the internal points from which the magnetic force can be taken to emanate, much like to calculate of the Newtonian attraction on a body we measure its distance

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[447] Volta, *Epistolario*, cit., Vol. 3, p. 438.

[448] A. Kircher, *Magnes sive de arte magnetica opus tripartitum*, Coloniae Agrippinae 1643, p. 159. The first edition appeared in Rome in 1641, as did a third in 1651. That three editions appeared in ten years is a testament to the popularity of the work.

from the centre of the Earth. Aside from experimental difficulties, this idea explains the failures of previous attempts. Dalla Bella, believing to have confirmed that Newton's law of attraction holds for magnets too, devised experiments to calculate (and indeed did calculate) the distance from the surface of each magnet to the point from which the emanating magnetic force could be taken to originate.

These were the limits reached by science in 1784, when Coulomb (Fig. 7.11) began his research in electrostatics almost by chance, or more exactly, carried by his success in the mechanical study of the elasticity of tension, as we related in § 7.8.

Fig. 7.11. Charles Augustin Coulomb

Charles-Augustin Coulomb (or De Coulomb) was born in a family of magistrates in Angoulême on 14 July 1736. Completing his studies in Paris, he joined the French army as an officer in the engineering corps. He was sent to Martinique to make important fortifications to a military base and returned to France after several years in poor health. He entered the scientific world with a theoretical-experimental paper published in 1776 on the applications of maximum and minimum rules to some problems of architectural statics. The paper, which studied the statics of arches and domes in particular, was greatly appreciated by French physicists and engineers. A few years later, his famous paper on simple machines (§ 7.8) earned him (unanimous) admission to the Académie des science of Paris in 1784, while at the same time, after overcoming serious disagreements with his supervisors, he was named *intendant des eaux et fontaines* (steward of waters and fountains) of France. He was later appointed to various other public offices, though he resigned from all of his appointments when the French Revolution struck (1789) and retired to a private estate. He returned to public activities in 1802 when he was named the inspector of public instruction. Coulomb died in Paris on 23 August 1806.

Coulomb's scientific approach, characterized across his research by an effective intertwinement of experimental investigation and mathematical calculation, is particularly evident in his seven classic papers on electricity and magnetism, which he read to the Académie between 1785 and 1789. Though his arguments can sometimes appear unusual and his mathematical reasoning unrigorous, the laws he formulated from experiments have been thoroughly confirmed.

In his first paper, reusing his laws on the elastic torsional force of strings (§ 7.8), Coulomb described the "electric balance" and detailed the first measurements he conducted with it. The balance consists of a horizontal needle, which is made of either wax-covered silk or straw, hanging from a vertical silver string inside a glass case, where the string is at one micrometre of torsion. A cork ball is fixed to one end of the needle, and a vertical paper disk balances it on the other, in addition to damping the oscillations. At rest, the moveable ball touches another ball attached from an insulating rod that itself hangs from the top of the enclosing glass cylinder. Around this cylinder, at the height of the needle, a graduated scale from 0° to 360° is marked.

Coulomb began by measuring the repulsive force between identical charges when the distance between them is varied. He reported the results of three measurements for which the distances between the two charged balls were in a 36:18:17/2 ratio, finding the corresponding ratio of forces to be 36:144:575.5, approximately an inverse square relation. In reality, he immediately observed that the experimental data deviated somewhat from the theoretical law. Coulomb explained the reasons for this disagreement, which, in addition to some mathematical simplifications made in his calculations, included electrical leakage during the experiment, a subject that he also treated in a later paper. Coulomb felt that he could conclude:

"The repulsive force of two small globes electrified with the same type of electricity is inversely proportional to the square of the distance between the centres of the globes."[449]

The measurement of the attractive force, on the other hand, turned out to be more difficult because when the moving sphere approaches the oppositely charged fixed one, the attractive force increases with the inverse square of the distance, while the torsional force increases less rapidly, in proportion to the angle of torsion. More precisely, if the elastic reaction force of the twisted string is set equal to the attractive force (which is assumed to follow an inverse square law), then it follows that equilibrium can only be obtained if the product of the electric masses in question does not exceed a certain limit. In practice, moreover, due to the oscillations of the needle, the equilibrium position not particularly stable, and rarely can one impede the moving sphere from colliding with the fixed one. Coulomb, however, claimed that he had achieved this equilibrium for various distances and that he had consequently verified the inverse square law, but abstained from providing his experimental data.

The French physicist preferred to describe another method, less simple and less direct, but also needing less precautions to carry out: the oscillation method that him and others had already used in the study of magnetism (§ 7.34). Specifically, he took an insulating rod with a small charged metallic plate vertically attached to one end and made it oscillate in front of an isolated metallic globe carrying the opposite charge, with the oscillation such that the horizontal diameter of the globe passed through the centre of the plate when it was in equilibrium. Using this setup, if T is the rod's period of oscillation and φ is the force between opposite charges, then by the law of oscillatory motion one has,

$$T = \frac{k}{\sqrt{\phi}}$$

If φ is assumed to be inversely proportional to the square of the distance d between the charges, which are assumed to be concentrated in the respective centres of globe and the plate, it follows that $T = Cd$, where C is a constant. This is a cursory argument that is only valid if the charge on the globe is much greater than that on the plate (a small disk of gold foil), but Coulomb did not mention this caveat at all. According to him, one simply has to verify if the period of oscillation of the rod (or the time it takes to

449 Ch.-A. Coulomb, *Première mémoire sur l'électricité et le magnétisme*, in "Histoire de l'Académie royale des sciences … avec les mémoires de mathématique et de physique. Mémoires", 1785, p. 572.

complete a certain number of oscillations) is proportional to the distance between the charges. In a series of experiments, the scientist obtained that when the distances were in a 3:6:8 ratio, the times to complete 15 oscillations were in a ratio of 20:41:53.33. The difference between theory and experiment, especially accounting for electric leakage in the experiment, was so small that Coulomb considered the inverse square law confirmed for attractive forces too. Clearly the method of oscillations can be used for repulsive forces too, but these are more precisely calculated using the torsional balance.

The above methods cannot be immediately extended to magnetic actions because of the conceptual and experimental difficulty pointed out by Dalla Bella: localizing the magnetic masses. Coulomb overcame this issue with two innovative methods. The first consisted of finding the equilibrium position of a magnetic compass needle that was also subject to the action of a second needle placed perpendicular to the first and in the same horizontal plane. The second method entailed comparing the periods of oscillation of a magnetic needle subject to only the Earth's magnetic field and a magnetic needle also subject to the action of a magnetized vertical wire along its magnetic meridian.

Once the location of the poles had been found, the oscillation method then also be used to study magnetic actions. Indeed, if a compass needle completes n oscillations when it is only subject to the action of the Earth and m oscillations when it is also subject to the action of a second needle placed vertically along the magnetic meridian of the first, then the action of the fixed needle on the moving needle is proportional to $m^2 - n^2$. The experimental data obtained allowed Coulomb to conclude that the law governing magnetic actions is an inverse square law. The torsional balance, used with the requisite precautions owing to the different experimental conditions, also confirmed the law. In this case, the experimental results and the theoretical predictions were even closer than those for electric charges, as there was no charge dispersion, despite the fact that the calculations were always performed assuming that there were only two North poles were present in the system (Coulomb gave up on experiments with different poles because of the aforementioned instabilities). However, because different poles were relatively distant from each other and their action was rather inclined in the horizontal plane containing the moving needle, their effect on the moving needle was negligible.

Once the fundamental law of electric and magnetic actions was established, Coulomb continued in his experimental and theoretical research. His attention first turned to electric leakage, which had so greatly disrupted his measurements in his previous studies. According to Coulomb, electric leakage occurs through insulating supports, which are never perfectly insulating, and the convection of air, whose particles come in contact with the conductor, take part of its charge, and then are repelled. Careful experimental studies led him to conclude that the electricity (charge) of a conductor in air exponentially decays with time. When this leakage was reduced to a minimum through the use of supports containing silk threads dipped in hot wax, the torsional balance permitted him to quantitatively study the leakage by convection, since in these conditions the decrease in force between two charges at a constant distance can only be attributed to the dispersion of the charges. The experiments led Coulomb to conclude that, for equal times, "the loss of electricity is always equal to the electric density."[450] If we translate Coulomb's proposition into mathematical language and indicate density with δ and time with t, we have

$$-d\,\delta = m\,dt$$

from which one can find that

$$\delta = De^{-mt}$$

where D is the initial density and m is a coefficient of proportionality. Therefore, Coulomb's proposition means that the electric density in a perfectly isolated conductor in air exponentially decays with time. We add, however, that for Coulomb the "density" was simply what we call charge.

Coulomb assumed, as Aepinus had done before him, the postulate that the force between two electric charges is proportional to their product. The attempts of some later scholars to demonstrate this postulate were all illusory; they serve only to confuse the ideas of inexperienced students, though they aimed to clarify, because they deduced Coulomb's postulate from other, much less intuitive ones. Having formulated the postulate, Coulomb studied the distribution of electricity on conductors, introducing the use of a "test plate", a gold foil disk connected to a thin insulating rod, which can be placed on the various parts of a conductor's surface to probe its electric density. After charging a cylinder with multiple holes, Coulomb observed, using a very sensitive electroscope composed of a thin needle hanging from a silk string inside a glass cylinder, that the "test plate" became charged when placed on the cylinder but remained uncharged when it was placed inside any of the holes. Coulomb concluded that electric charge was held at the surface of a conductor. The fact that he considered this a new discovery when it had already been discovered by Beccaria

[450] *Ibid.*, p. 618.

fifteen years earlier (§ 7.32) using a more reliable apparatus demonstrates Coulomb's lack of information regarding his field, a result of the fact that his electric studies began almost by chance.

In any case, Coulomb realized that this property is a consequence of the repulsion between electric charges, and gave a first unrigorous demonstration of this fact that was not substantially different from that of Cavendish (§ 7.32). Curiously, the two scientists also devised the same experimental test of the property: if an isolated charged conductor is enveloped by two metallic coverings with insulating handles that are then separated from the charged body, the two coverings become charged and the original conductor becomes uncharged. This experiment was described by Coulomb in his sixth paper (1788), while Cavendish had described it in a paper that was written around 1722 and remained unpublished, *Experimental Determination of the Law of Electric Force*.[451] In Cavendish' experiment, the conductor was an isolated sphere (while in Coulomb's it had an arbitrary shape) and the two coverings were hollow conducting hemispheres that attached at a hinge. Cavendish established that the charge of the sphere, which he assumed was not detectable due to poor instrument sensitivity, had to be less than 1/50th of the charge acquired by the hemispheres. From this, he deduced that the exponent of distance in the inverse proportionality law could fall between

$$2+\frac{1}{50} \text{ and } 2-\frac{1}{50}$$

thus, there was no reason to doubt that the law was an inverse square law.

In formulating his theory of the test plate – a rather unsuccessful theory in which the scientist forgot that, due to a property that he himself had experimentally discovered, the contact area between the test plate and the charged conductor has no charge – Coulomb assumed that the electric force at an external point infinitely close to the surface is proportional to the surface density in the vicinity of the point: this is known as Coulomb's theorem, though he never stated it as such.
With the help of the test plate and a sophisticated experimental technique, Coulomb showed that electric charge is uniformly distributed on an isolated conducting sphere; he studied the charge distribution on multiple conducting spheres in contact and then a cylinder; and he rigorously demonstrated using his balance that the amount of charge induced on a conductor by a charged body is equal and opposite to the inducing charge.

The good agreement between theoretical predictions and experimental results, the discovery of a long sought law, and the framing of these new

[451] Cavendish, *The Electrical Researches* cit., pp. 104-13.

forces using Newtonian language gave scientists a sense of confidence and, from a mathematical point of view, placed the electro and magnetostatic theories in first place in the ranking of scientific theories, alongside the theory of gravity.

Nevertheless, there was no lack of criticisms and reservations regarding Coulomb's theory. The critics included Martinus van Marum and, more authoritatively, Volta, who used his electrometric balance (§ 7.31) to "disprove" the inverse square law, "which Monsieur Coulomb…has recently claimed to have derived with experiments and demonstrations."[452] According to Volta, the law did not hold for repulsions, which he always considered only apparent, "and neither for the attraction does it generally take place … but only in the case that the plate is one side touched by a charged ball in its current position, and on the other the plate that it rests on is constantly in communication with the ground."[453]

Volta remained firm in this conviction for the rest of his life. According to Jean-Baptiste Biot, even the most thorough discussions with Laplace and Coulomb himself, which perhaps occurred in 1801, on the occasion of Volta's presentation of the battery to the Institut de France in Paris, could dissuade him from his opposition to Coulomb's law.[454]

Volta's criticism was repeated in the 19th century by other experimenters. In 1834, William Snow Harris (1791-1867), a well-known British electrologist held in high esteem by Faraday, set out to experimentally verify Coulomb's law using an electrometric balance similar to the one used by Volta. Harris too found that attraction does not fall with the inverse square of the distance, aside from a few special cases.[455]

In a later 1836 paper, Harris, using a modified Coulomb balance, found that electric repulsion follows an even more complicated law: if the distance between two charged bodies is great compared to their linear dimensions, then the force between them is still governed by the inverse square law, but at smaller distances the repulsion is always less than what Coulomb's law predicts, and even transforms into attraction if one of the charges is much greater than the other.[456]

A few years later, in 1838, Faraday also expressed reservations about Coulomb's law based on his studies of specific inductance capacity.

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452 Volta, *Le opere* cit., Vol. 5, p. 78.

453 *Ibid.*, p. 79. The experimental conditions described by Volta obviously refer to his electrometric balance apparatus.

454 J.-B. Biot, *Volta Alexandre*, in *Biographie universelle*, Paris 1827, Vol. 49, p. 460.

455 W. S. Harris, *On Some Elementary Laws of Electricity*, in "Philosophical Transactions", 1834, pt. 1, pp. 213-45.

456 *Ibid.*, 1836, pt. 1, pp. 417-52.

Faraday's hesitation arose from the fact that he did not accept the idea of action at a distance, and thus attributed the propagation of electric force to the (not necessarily linear) action between the molecules of the ambient medium.[457]

A significant number of physicists, following the ideas of Harris and Faraday, began to distrust Coulomb's law. A deeper reexamination of the issue was needed. This task was taken on by a very young scientist, William Thomson (1824-1907, from 1892 known as Lord Kelvin), who, after a careful critical examination, observed in an 1845 note that Coulomb's law is valid for point charges that are distant from each other and from other conductors, and not for the complex configurations studied by Volta and Harris. On the contrary, the experimental results obtained by these experimenters using the electrometric balance confirmed Coulomb's law because Thomson demonstrated that if F is the force of attraction between two metal plates, v is their potential difference, A is the area in contact between them, and a their distance, Coulomb's law gives

$$F = \frac{v^2 A}{8\pi a^2}$$

a formula that exactly corresponds to the experimental laws found by Volta (§ 7.31). The anomalies observed by Harris were also interpreted by Thomson as mathematical consequences of Coulomb's law.[458]

The perfect agreement between previous experimental results and his theoretical predictions gave Thomson the idea of using Volta's experimental apparatus to carry out an absolute measurement of potential difference.[459] Thus was born the absolute electrometer, a true marriage of Volta's thought and Thomson's work despite the fact that Thomson did not cite any of Volta's specific studies. The absolute electrometer was unveiled in 1755 at the annual meeting of the British Association for the Advancement of Science, but was only effectively used to measure the electromotive force of a voltaic battery in 1860.[460]

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457 M. Faraday, *Experimental Researches in Electricity*, London 1851, §§ 1295 et seq.

458 W. Thomson, *Note sur les lois élémentaires de l'électricité statique*, in "Journal des mathématiques pures et appliquées", 10, 1845, pp. 209-21.

459 W. Thomson, *On the Transient Electric Currents*, in "Philosophical Magazine and Journal of Science", 4th series, 5, 1853, p. 405.

460 W. Thomson, *Measurement of Electrostatic Force Produced by a Battery*, *ibid.*, 20, 1860, p. 233.

7.34 Magnetism

Magnetic phenomena do not exhibit the variety and dynamism of electric phenomena, so it is no surprise that the scientific literature on magnetism in the 17th century is not as wide-ranging as that on electricity.

There were three main goals orienting magnetic research in the century: discovering the law governing magnetic attraction and repulsion, which Coulomb finally found, as we discussed in the previous section; increasing the magnetic intensity of artificial magnets; and studying the phenomena related to Earth's magnetism, an issue that had been long scrutinized by 17th century physicists, who inherited from their forefathers the little knowledge described in § 3.18.

Following Musschenbroek, the discovery of the daily variation of the Earth's magnetic declination, the angle between true North and the magnetic North pole, is generally attributed to the Jesuit priest Guy Tachard (1651-1712), who is thought to have made it in 1682 during his voyage in Siam and described it in pages 223-25 of his *Voyage de Siam des pères jésuites* (1689). Father Tachard supposedly measured the declination on four consecutive days, finding respective declinations of 16', 31', 35', and 38', and after a long time measured it again for three consecutive days, finding 28', 33', and 21'. Musschenbroek concluded that these were the first observation of the daily variation in declination. In reality, it appears that these observations do not add much to what was already known at the time: the declination measured, at a certain point, changes with time.

The daily variation in magnetic declination, tied to the apparent motion of the sun, was carefully studied by the clockmaker and engineer Graham, whom we already mentioned earlier (§ 7.18). In a paper published in the 1725 edition of "Philosophical Transactions", he reported that he had experimented with three magnetic prism-like needles attached at one end to a vertical point and placed inside an enclosure to protect them as much as possible from external perturbations. With this setup, he found that all three needles pointed in the same direction, but not only did they indicate different directions on different days, but often even at different hours of the same day, with variations of up to half a degree. The daily variations were unaffected by heat, cold, humidity, or dryness; the only pattern was that the greatest variations could be observed between the hours of 12:00 and 16:00 and 18:00 and 19:00.

Around this time period, the idea of measuring the intensity of the Earth's magnetism began to spread throughout scientific circles. Naturally, it was not quite clear yet how to do this. For example, a rather misguided idea was expressed by Musschenbroek in a 1725 letter to Desaguliers that was later published in "Philosophical Transactions": after attempting to find the law of magnetic attraction (§ 7.18) using a scale in several experiments, Musschenbroek concluded that in addition to the varying magnetic declination and inclination, the variation of a magnet's force in time should also be investigated.

More refined experimental methods are found in the work of William Whiston (1667-1752), Newton's successor at Cambridge (1703). His book, titled *Treatise on the Dipping Needle*, was not very popular, but its contents was brought to continental Europe by Musschenbroek's *Physicae experimentales* (1729). Whiston compared the period of oscillation of a magnetic needle placed along the magnetic meridian (vertical motion of a horizontal needle in the plane of the Earth's surface) to the period of a pendulum of half its length in order to determine the ratio between the force of gravity and the magnetic force. More precisely, if T and t are the respective periods of the magnetic needle and the pendulum and f_g and f_m are the force of gravity and the magnetic force, respectively, then one has

$$T^2 : t^2 = f_g : f_m$$

from which one can find an expression for f_m that substantially agrees with the modern $F = kn^2$ (where F is the intensity of the Earth's magnetic force, n is the frequency of oscillations, and k is a constant). However, neither Whiston nor Musschenbroek knew how to define f_m, nor did they know that k depends on the geometric and magnetic conditions of the needle, as Graham had realized in 1726 while experimenting on a compass needle oscillating about the magnetic meridian. Experimenting in the same way, Musschenbroek found that the ratio between the gravitational and magnetic forces was 295:1, while Whiston had found a value of 600:1. Instead of seeking the cause of such different values, Musschenbroek limited himself to recording them and expressing his surprise that Earth's magnetic property acts less intensely on longer magnetic needles.

If instead of making the needle oscillate about the magnetic meridian it is made to oscillate about another vertical plane, the motive force changes as the cosine of the angle formed between the plane of oscillation and the magnetic meridian, as Whiston acutely observed.

It was not difficult to extend the oscillation method from magnetic inclination (up and down movement of a compass needle) to magnetic declination (side to side movement of a compass needle) to find "the intensity of the directive virtue [magnetic force]". It was Musschenbroek who took this step, concluding based on eight experiments that the ratio of squared oscillatory periods of two needles of different length is equal to the ratio between the product of their lengths and the product of their masses: a rather nontrivial proposition, and considering that these products are proportional to the magnetic moments involved given that it is reasonable to assume that Musschenbroek magnetized both needles in the same way, not an incorrect proposition either. From his experiments, it resulted that the "directive virtue", that is, the horizontal component of the Earth's magnetic force, is less than the force in the direction of the magnetic needle, which is the maximum force.[461]

Another experimental procedure, introduced by Whiston, was applied by Thomas Le Seur and François Jacquier to check the statement contained in book III (proposition VI, theorem VI, corollary 5) of Newton's *Principia*: magnetic force decreases not in proportion to the inverse squares of the distance, but to the inverse cubes. The annotators of Newton's work maintained, on the basis of what evidence we do not know, that Newton's experiments in this field were substantially the same as those devised by Whiston: let a centre of a magnet be placed very far away from a magnetic needle along a line perpendicular to the magnetic meridian and originating from the needle's centre, and let the action of the magnet deviate the needle at an angle of α with the meridian. If F is the resultant force of all the forces with which the Earth acts on the top half of the needle, then the restoring (mechanical) force that tends to bring the needle back along the meridian is proportional to $F \sin \alpha$; on the other hand, supposing like Whiston that because of the large distance between the magnet and needle, it can be taken to act like the Earth, with a force F' applied to the same point where F is applied, then the deviating force of the magnet on the needle will be proportional to $F' \sin (90° - \alpha)$. At equilibrium, one has

$$F \sin \alpha = F' \sin(90^o - \alpha)$$

or

$$\frac{F'}{F} = \frac{\sin \alpha}{\sin (90° - \alpha)}$$

For a different position of the deviating magnet, one has a different formula analogous to the one above. By comparing these two formulas, one can deduce how the forces exerted by the magnet on the needle vary with distance. The results of the experiments conducted by Le Seur and Jacquier agreed with those obtained by Whiston and showed that the force exerted

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[461] P. van Musschenbroek, *Physicae experimentales et geometricae*, Lugduni Batavorum 1729, p. 227. Wilson's methods that we have described are also detailed in this volume.

by the magnet on the needle varies with the inverse of the cube of the distance between the centre of the magnet and the centre of the needle.[462]

Whiston's method is now applied to all types of portable magnetometers with the only modification that the deviating magnet is arranged with its axis normal to the axis of the already deviated needle. Musschenbroek had also somewhat modified the method to compare the horizontal component of the Earth's magnetism with the force exerted on the needle by a magnet.

Many scientists of the time, however, had the impression that all studies of the Earth's magnetism remained vague, owing to the fact that nobody had yet been able to define the specific quantities that needed to be measured. It was therefore wiser to concentrate their efforts for the moment on the study of the two quantities that had been precisely defined: magnetic declination and inclination. Much work had been done on the first, especially because of how valuable a knowledge of declination is to navigation, while inclination had been less thoroughly studied, both because it did not appear to be of practical use and because it was more difficult to measure.

The Académie des Sciences of Paris perceived this defect in the current state of studies and (in 1741) proposed a competition for the best way to build an inclination compass. Daniel Bernoulli earned the prize in 1743 with a paper that we will discuss shortly, while Euler came in second place.

Bernoulli wrote that at the time, even if inclination compasses were built as carefully as possible, there were as many different compasses as inclinations observed in the same place. These differences, even for the most able of experimenters, could reach 10-12 degrees. Bernoulli identified the causes of this serious situation as the effect of air resistance on the needle and the fact that the weight of the needle is not homogeneously distributed, altering its measured magnetic moment.

To reduce the first cause of error, Bernoulli suggested the use of a technique that had already been employed by Graham and Musschenbroek (and is still used today): resting the axis of the needle on two glass bars placed on the same horizontal plane.[463]

Compensating for the second error was must more difficult because even the most skilled artisans could not guarantee that any given needle's centre of mass was located exactly on its rotational axis. Even when certain techniques were successful (Bernoulli describes one such technique, which he modified immediately after), this solution is only provisional because the inevitable curvature of the needle due to its weight soon uncalibrates the

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[462] I. Newton. *Philosophia naturalis principia mathematica*, edited by Th. Le Seur and F. Jacquier, Geneva, 1743, Vol. 3, t. I, pp. 40-43.

[463] R*ecueil des pièces qui ont remporté les prix de l'Académie royale des sciences*, 1752, *Pièces sur les boussoles d'inclination*, pp. 6-11.

device. After finding the equation of the curve into which the needle bends because of its own weight, Bernoulli suggested that shorter and thicker needles be built. The error due to the bending of the needle can be eliminated in various ways: placing two needles in a cross, one made of steel and the other of copper, with their relative centres of mass on the axis of rotation, and choosing the needles in such a way that following their respective bending, the centre of mass of the system remains directly above the point of attachment to the compass; adding a small flap with a moving weight above the needle, where the position of the weight can be adjusted to compensate for the bending of the needle and maintain the centre of mass of the system above the point of attachment.

However, according to Bernoulli, the best inclination compass could be obtained by exploiting another, similar principle. Let *o* be the axis of rotation of a needle of mass *P* and let *G* be the centre of mass of the needle, where *G* is not along the axis *o*; for the needle to be in horizontal equilibrium, a small weight *p* must be added at a distance *l* from the axis. If now the needle is to be inclined at an angle *α*, then weight *p* must be moved to a different distance *l'*. Bernoulli wrote the two equilibrium conditions for the two different positions and derived the relation

$$l - l' = l \cot \beta \tan \alpha$$

where *β* is the angle between the line passing through *G* and normal to the axis (when the needle is horizontal) and the vertical; *β* can be experimentally determined because it is also the angle between the needle and the horizontal when the weight *P* is removed.[464]

The inclination compass, therefore, is built as follows. First, an unmagnetized needle is carefully constructed and placed in the compass. To keep the needle in horizontal equilibrium, a sliding weight *p* is attached on one side at a certain distance from the axis. The position of the weight *p* is marked, and then it is brought further away from the axis in increments of 5 degrees; for each position of the weight the corresponding inclination is marked along the needle. Lastly, the needle is magnetized in such a way that (for the Northern hemisphere) the North pole is on the side of the needle carrying the weight *p*. The compass is then ready for use. To measure a magnetic inclination, one lets the weight *p* slide until the inclination it marks along the needle is the same as the actual inclination of the needle: this is the true magnetic inclination of the location and is independent of any small imperfections of the apparatus.

[464] *Ibid.*, pp. 44-55.

However, this device was not convenient for measurements at sea because the rocking of a boat would continuously shift a compass placed on a horizontal table. Bernoulli recommended another device for maritime use: a vertical needle bisecting a dowel whose ends are attached to the centres of two identical, perfectly spherical and homogeneous balls. These two balls are immersed in two cylindrical containers of mercury through two holes large enough to allow the needle to move, which therefore spontaneously aligns itself along the local magnetic meridian. Because of its autoregulation and negligible friction, Bernoulli recommended the device for terrestrial use as well, and it can perhaps be considered to be the precursor to the liquid compass.

Euler's aforementioned paper[465] can be considered a complement to that of Bernoulli. While the latter was only concerned with the mechanical construction of the needle, Euler also considered a second cause of error in measurements arising from the fact that the vertical plane containing the needle does not coincide with the magnetic meridian.

The mathematical study of the oscillatory motion of an inclination needle shows the necessity and importance of orienting the needle's axis of rotation normal to the magnetic meridian. To obtain this, Euler proposed the following device, in essence the modern inclinometer. A magnetic needle is fixed to two dowels, each attached at an end to the same vertical disk that is free to rotate along a horizontal axis. The vertical disk, in turn, rests on a support that can rotate along a graduated horizontal base. When the vertical disk is oriented along the geographic meridian and the needle's axis of rotation is vertical, the direction indicated is the declination. If, starting from the position of the needle, the support is rotated by 90° and then vertical disk is rotated by 90° about its own axis, then the needle's axis of rotation will be normal to the magnetic meridian, and one can therefore obtain the true magnetic inclination. Euler also discussed using the instrument much like how it is used today: if the magnetic declination at a certain point (which varies in time, as Euler also was aware) is known, the instrument can be replaced by a simple vertical board on which a graduated disk is drawn where a magnetic needle can oscillate. When this board is oriented along the direction of the local magnetic meridian, the needle will directly point in the direction of magnetic inclination.

With Bernoulli and Euler's papers, the problem of measuring magnetic inclination moved beyond the purely empirical domain. The scientists

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465 L. Euler, *De observatione inclinationis magneticae dissertatio*, in *Recueil de pièces qui ont remporté les prix de l'Académie royale des sciences*, 1752, *Pièces sur les boussoles d'inclinaison*, pp. 63-69.

demonstrated that the obstacles left to surpass were not only practical, but truly conceptual in nature.

After sending off his manuscript, Bernoulli actually built the instrument he described and measured an inclination of 71°30’ in Basel. He also sent a prototype to Euler, as his son later documented in a 1755 paper for the Academy of sciences of Berlin. The same instrument was used by Jacques-André Mallet (1740-1790), who was sent by the Saint Petersburg Academy of sciences on a scientific mission to Ponoi, in Lapland, to conduct magnetic and meteorological observations. On this occasion, Mallet attempted to compare the number of oscillations of a declination needle (per fixed time) in two different places. However, the Swiss scientists’ instrument was clearly imprecise, as he obtained the same number of oscillations in Ponoi and Saint Petersburg, which differ by 7’8” in latitude. These unfortunate experiments by Mallet led some scientists, even including Cavendish, to believe the intensity of the Earth’s magnetism to be constant at every point on the Earth’s surface.

Both Le Monnier and Borda doubted this constancy, but the latter was nevertheless unable to find differences in magnetic intensity between Paris, Santa Cruz de Tenerife, Gorée, or Guadaloupe. Robert de Lamanon, one of Jean-François de la Pelouse’s companions on his journey around the world (1785-87), noted a variation in intensity from the variation in the period of oscillation of an inclination needle: he communicated these results, which remained unpublished, to Condorcet, the secretary of the Académie des sciences of Paris.

Important in this period was Coulomb’s clarificatory work framing the study of terrestrial magnetism in the larger field of magnetism (without adjectives) and explicitly stated its laws, which had been tacitly postulated by his predecessors. As early as in a 1777 paper on magnets, and more fully in the aforementioned (§ 7.33) 1785 paper, Coulomb showed that the Earth’s magnetism acts on the poles of a magnet with equal and opposite forces in the direction of the magnetic meridian, whatever the position of the magnet and the strength of its magnetization. In the seventh (1789) paper from the great series that we mentioned in the previous section, Coulomb set out to establish a relationship for such forces acting on magnets made of the same substance but of different sizes; the strength of magnetization at every point in a magnet; a theory of magnetism; and the best way to magnetize natural materials.

The theories of magnetism immediately preceding Coulomb were the vortex theory, by then discredited but which nevertheless had even counted Euler among its supporters, Aepinus’ unitary theory, and the opposing dualistic theory. After quickly dispatching the first theory, which seemed to contradict experiments, Coulomb spent considerable time on both Aepinus' theory, based on the unitarity of the electric fluid, and the dualistic theory, which postulated the existence of two magnetic fluids that would separate to opposite poles during the act of magnetization. On the mathematical level, these two theories are equivalent, and Coulomb objected that the forces on the ends of a magnetized needle remain equal and opposite even when one end is cut: an experimental result incompatible with either. Theory and experiment can be reconciled by supposing that the two magnetic fluids (or equivalently Aepinus' single fluid) are found in equal measure in every "molecule" of iron, and that inside the molecule they are free to move to its edges but cannot escape it, being bound by adhesion with matter. In short, every magnet is a collection of small, oriented elementary magnets: basically, Castelli's hypothesis (§ 5.36) resurfaced a century and a half later.

Coulomb's paper ends with some practical suggestions for the construction of magnets that have been more or less followed into our time: cutting magnetic needles into lozenges and not rectangular prisms, as was the practice in Coulomb's day; magnetizing using the "double contact" method introduced by Aepinus and improved by Coulomb, who placed the iron bar to be magnetized on the opposite poles of two strong magnets; making powerful magnets using stacks of magnetic sheets; and increasing the range of magnets by placing two magnetic bands of opposite polarities next to each other, a device that could replace the weaker horseshoe magnet.

Returning to scientific work after the upheaval of the French revolution, Coulomb once again dealt with magnetism in several studies. In a 1799 paper, he criticized the design of the inclination compass suggested by Bernoulli, raising the following points: variations in temperature cause variations in the distance of the counterweight from the centre of gravity; the frequent movements of the counterweight along the needle alter its shape and thus the centre of gravity of the system; and the needle necessarily becomes slightly demagnetized with use.

Coulomb followed these criticisms with the description of a new approach in 1803, based on the idea of determining the magnetic inclination from measurements of the horizontal and vertical components of the force with which the Earth acts on a needle. For the horizontal component, he determined the magnetic moment of the forces that cause a needle to oscillate in the horizontal plane. This moment, as he himself had demonstrated following Euler's previously cited paper, is given by $Pl^2/3\lambda$, where P is the weight of the needle, l is half of its length, and λ is the length of the isochronous pendulum. The moment of the vertical component is determined by balancing an unmagnetized needle and then balancing it with a counterweight after magnetization, and repeating the measurements when the polarity is inverted. The average of these two values is the exact magnetic moment, independent of the inevitable shift in the centre of gravity of the needle from the balancing, any possible initial magnetization of the needle, or its curvature. The ratio of the two moments is equal to the ratio of the two components of the force, so if x is the inclination angle one has

$$\tan x = \frac{M_2}{M_1}$$

where M_2 is the vertical moment and M_1 is the horizontal moment.

The procedure requires a rather lengthy numerical calculation and, moreover, is not applicable at sea. Coulomb therefore suggested a quicker but less exact method that gives excellent results when the chief problem is, as often occurs, that the needle's centre of gravity is not situated exactly on its axis of rotation. Following his approach, the magnetic inclination is measured in an ordinary manner, then the magnetization of the needle is reversed and the measurement is repeated: the average of the two measurements gives an approximate value for the inclination. Coulomb experimented on two needles using this technique: with the first, which had a rectangular longitudinal cross-section, he obtained an inclination of 69°41', while with the second, of rhomboidal cross-section, he obtained an inclination of 69°29'. The two measurements were so close that the method earned the scientist's full confidence.

An important step forward in the study of terrestrial magnetism was taken by Alexander von Humboldt (1769-1859), the original discoverer of the variation in intensity of the Earth's magnetism with latitude. Asked by Borda to observe the oscillations of an inclination needle at different latitudes, Humboldt returned from his circumnavigation in 1804 (he had been sailing since 1798) with so many observations that, along with Biot, he wrote a detailed paper that clearly established that three chief properties must be studied: declination, inclination, and intensity. Inclination measurements at 104 different locations on the globe demonstrated beyond a shadow of doubt that the intensity of the Earth's magnetism varies from place to place.

Humboldt typically measured the frequency of a needle's oscillations along the magnetic meridian and in the plane perpendicular to it. Laplace set out to repeat Humboldt's observations to check if the differences he had observed could be ascribed to magnetic, thermal, or mechanical variations of the needle. His test was the following: let F be the total intensity of the Earth's magnetism; when the needle oscillates in the plane normal to the meridian, the force that causes the oscillations is the vertical component of F, namely $F \sin i$, where i is the inclination of the needle. Now, if m and n are the oscillation frequencies in the plane of the meridian and in the perpendicular plane, respectively, one has

$$\frac{F \sin i}{F} = \frac{n^2}{m^2}$$

from which the inclination i can then be determined.[466] The observed values of i nearly coincided with those calculated using this formula, and therefore Humboldt's measurements were trusted and accepted by the scientific community, as were the terms *isogonic lines*, *isocline*, and *isodynamic*, which he introduced.

After the German scientist's work, experimental measurements of the Earth's magnetism multiplied considerably, conducted by both physicists and navigators, to the extent that in 1819 Christopher Hantseen (1784-1873), a Norwegian interested in terrestrial magnetism, was able to draw a magnetic map (§ 3.18) that also indicated isodynamic lines.

By the first decades of the 19th century, a rough-and-ready method to measure the intensity of the Earth's magnetism had become widespread: if n is the frequency of oscillations of a magnetic declination needle, $H = kn^2$ is the horizontal component of the Earth's magnetism, where k is a coefficient that depends on the geometric and magnetic conditions of the needle. If i is the local magnetic inclination, the total intensity is then $F = H/\cos i$.

Poisson noted that the variation in k makes measurements difficult to compare to each other (we have often seen that the need for absolute standards of measure became increasingly evident in the 18th century). To fix this, he suggested a new method that appears to have never been applied.

In Poisson's method, a first magnetic needle is hung from its centre of mass such that it is free to oscillate and its period is measured. The same procedure is applied to a second needle. The centres of mass of the two needles are then aligned along Earth's magnetic field, and the period of each needle is measured while keeping the other fixed. There are thus seven quantities: the distance between the centres of mass, the two moments of inertia of the needles about the vertical axis through their centres of mass, and the four periods of oscillation. Poisson showed that there exists a certain function F of these seven quantities such that

466 A. von Humbolt and J.-B. Biot, *Sur les variations du magnétisme terrestre à différentes latitudes*, in "Journal de physique, de chimie, histoire naturelle et arts", 59, 1804, pp. 431-32.

$$\phi^2 = fF$$

where φ is the intensity of the Earth's magnetism and f is a constant. This relation then allows for a comparison the intensity of the Earth's magnetic field at different points and moments in time.[467]

Poisson's criticism was reiterated by Karl Friedrich Gauss, born in Brunswick on 20 April 1777 and the director of the astronomical observatory at Göttingen from 1807 until his death, on 23 February 1855. An 18th century style polymath, Gauss made seminal contributions to mathematics, astronomy, geodesy, and physics, in particular to problems concerning systems of lenses (*Dioptrische Untersuchungen*, 1840) and to electric and magnetic research.

He began his studies of terrestrial magnetism around 1830 and read his first paper[468], now a classic, on 15 December 1832 to the Akademie der Wissenschaften of Göttingen. The proceedings of the academy, however, were published much later (in 1841), and Gauss's paper was therefore disseminated in its German version, published in volume 28 of Poggendorff's "Annalen", and its French version, published in volume 57 (1834) of "Annales de chimie et de physique".

In the introductory section of his paper, Gauss reasserted the age-old aspiration for measurements that are comparable to each other even when conducted at different locations or times, an aspiration, in other words, for absolute measurements. He then observed that three fundamental mechanical units are necessary to define the absolute unit of magnetism, proposing to use length, (gravitational) mass, and time. From these units, he first derived a unit of force, then a unit of "magnetic quantity", and finally a unit to measure the intensity of the Earth's magnetic field ("the magnetism which acts on a unit of free magnetic quantity with a unit force"), which would later be called a *gauss*.

Gauss's model of a magnet was similar to Coulomb's, but replaced the real distribution of two magnetic fluids with a fictitious surface distribution such that the resulting external effects remained the same. Calling the difference between the amounts of boreal and austral fluid in an infinitesimal magnetic element the *free magnetism* of the element, he found that the integral of this quantity over an entire magnet is zero. Defining the free magnetic moment along an axis as the integral over a whole magnetic body

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467 S.-D. Poisson, in "Nouveau bulletin des sciences par la Societé philomatique de Paris", 1825, pp. 182-84; 1826, pp. 119-21.

468 K. F. Gauss, *Intensitas vis magneticae terrestris ad mensuram absolutam revocata*, in "Commentiones Societatis regiae Gottingensis recentiores. Classis mathematicae", 8, 1832-37, pp. 1-44.

of the product of free magnetic elements and their distance from a plane normal to the axis, Gauss showed that there is a direction for which this moment is maximized. He called this direction the *magnetic axis* and the corresponding moment the *magnetic moment*.

When every particle in a magnetic fluid is subjected to a constant force (in magnitude and direction, where the direction is opposite for boreal and austral magnetic particles), or, to use modern language, when a magnetic body is in a uniform magnetic field, the net torque is $PM \sin \alpha$, where M is the magnetic moment, P is the field strength and α is the angle between the external field and the magnetic axis of the body. It follows that the Earth's magnetism acts on a needle to cause an apparent shift in its centre of gravity. To determine magnetic inclination, one therefore needs to preemptively know the centre of mass of the needle. This is difficult to measure for an unmagnetized needle, as even mechanical manipulations can slightly magnetize it. Gauss recommended the approach proposed by Coulomb and observed that the difference between the apparent and true centres of mass cannot be greater than a certain limiting value, which for the best needles and in regions where the tangential component of the Earth's field is greatest does not surpass 0.6 mm: beyond this, extreme mechanical precision is necessary in the construction of inclination needles.

If M is the magnetic moment of a declination needle, K is its moment of inertia with respect to the axis of oscillation, T is the tangential component of the Earth's magnetic field, and t is the duration of a half-period, the laws of oscillatory motion give

$$TM = \frac{\pi^2 K}{t^2}$$

This formula is valid if the needle is subjected only to the action of the Earth's magnetic field. If the needle is hanging from a string, however, the effect of torsion cannot be neglected in experiments. In such situations, t must be multiplied by a corrective coefficient that, Gauss explains, must be found experimentally.

To calculate TM one must then find K. As an idealized mathematical calculation could prove deceptive for a complex apparatus, Gauss came up with a modification to Coulomb's method of oscillations (§ 7.33). A transverse beam is attached to the needle in question. Two identical weights of known mass hang from the beam on opposite sides of the needle, horizontally aligned along the beam and equidistant from the point where the needle and beam intersect. By performing three experiments, one can determine the period of oscillation of the needle without the beam and the needle with the beam and weights at two different distances. By inserting these three data points into the equation above one can the calculate *TM* and the moments of inertia of the needle and beam.[469]

The calculation of the ratio *M/T* is much more complicated from a mathematical point of view. The physical idea employed by Gauss was analogous to the one already used by Whiston: when a magnetic needle is brought near another magnetic needle horizontally suspended from a vertical axis, the orientation of the second needle changes, where the angle of deviation is related to *M* and *T* by an expression involving *M/T*. This problem, as Gauss wrote, can also be solved using the method of oscillations, that is, deducing *M/T* from the change in the period of the second needle in the presence of the first, but the deviation method gives more reliable results.

Though the physical idea is simple, the mathematical calculation is laborious, and the final expression for *M/T* is complicated. The derivation and expression simplify, however, when the deviating magnet is in one of the following two positions (today called Gauss' *first* and *second positions*, respectively): the axis of the deviating magnet is normal to the magnetic meridian and crosses it at the centre of the deviated magnet; the centre of the deviating magnet is along the magnetic meridian, which passes through the centre of the deviated magnet, while its axis is normal to the meridian. Of these two positions, the first is preferable according to Gauss, from which one can deduce the relation (if the torsion of the string from which the second needle hangs is neglected)

$$\frac{M}{T} = \frac{R^3}{2}\tan\alpha$$

where *R* is the distance between the centres of the two needles and α is the deviation of the hanging one. This relationship becomes more complicated if one does not neglect the torsion of the string.

Nowadays, the *solenoid method* is preferred to Gauss', though it requires measuring the intensity of electric current. The ability to do this came only with the so-called tangent compass[470], whose use requires the knowledge of the horizontal component of the Earth's magnetic field at least one point. One can see that, besides their intrinsic value, the study of the Earth's magnetism was critical for (absolute) electromagnetic measurements.

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[469] The analytical developments associated with this problem and other problems pertaining to terrestrial magnetism can be found in M. Gliozzi, *Lo studio del magnetismo terrestre dal 1700 al 1832*, in "Periodico di matematiche", 4th series, 42, 1964, pp. 1-24.

[470] §3.6 in: M. Gliozzi, *A History of Physics over the Last Two Centuries*. Cambridge Scholars Publishing, in press 2022.

Fittingly, Gauss' paper ended with a now classic observation regarding systems of measure. The observation, which in reality had already been brought to attention in 1822 by Jean-Baptise-Joseph Fourier in his *Théorie analytique de la chaleur*, consists in noting that the units of measure of a quantity change if the fundamental units defined at the beginning of the paper are changed. For example, the unit k representing the measure of T (the horizontal component of the Earth's magnetic field) becomes

$$k' = k\sqrt{\frac{p}{rs^2}}$$

if the fundamental units change such that r is the ratio between the old and new units of length, p is the analogous ratio for units of mass, and s the ratio for units of time; obviously the formula is different for quantities involving other fundamental units. These "dimensional equations" were written in this form until Maxwell, where this specific meaning of the term "dimension" had been introduced by Fourier.

7.35 Luigi Galvani

As we have seen (§ 7.25), the suspicion and hope that the novel phenomenon of electricity could cure or at least ameliorate human suffering arose after the first experiments on electric discharges. The discovery of the Leiden jar confirmed these suspicions and made hopes swell. When Franklin finally drew electricity from the clouds and Le Monnier obtained electrical signals even with a clear sky, it seemed that "all of nature became electricity". And if all of nature was electric, the thinking went, so too must human physical, and even spiritual life be regulated by the flow of this mysterious substance through veins and muscles. It was in this way that the idea of *animal electricity* – a supreme regulator of the lives of animals in general, and human beings in particular – arose.

In 1773, John Walsh (1726-1795) published an article in which he demonstrated the electric nature of the famous behaviour exhibited by the electric ray fishes. Not content with the mechanical explanations given for the phenomenon of the electric ray, s'Gravesande and Musschenbroek had also supposed it to have an electrical origin, though they did not confirm this through experiment. On the other hand, Pierre Bayen (1745-1798) had conducted some experiments on the matter, though they had gone unnoticed. Walsh's paper, therefore, seemed novel and caused quite a stir; he experimentally showed that manta ray phenomena could be reproduced using artificial electricity. Written in the form of a letter to Franklin, Walsh's paper ended with the following words: "I rejoice in addressing these communications to you. He, who predicted and shewed [*sic*] that electricity wings the formidable bolt of the atmosphere, will hear with attention, that in the deep it speeds a humbler bolt, silent and invisible: He, who analysed the electrified Phial, will hear with pleasure that its laws prevail in animate Phials: He, who by Reason became an electrician, will hear with reverence of an instinctive electrician, gifted in his birth with a wonderful apparatus, and with the skill to use it."[471]

Walsh's writings were followed by numerous other papers on the electric ray, both physical and anatomical, including Cavendish's aforementioned (§§ 7.28, 7.32) 1776 paper, in which the scientist described an "artificial torpedo [electric ray]" powered by electricity from a Leiden jar. The curious contraption was immersed in salt water of salinity similar to ocean water. The results obtained were identical to those produced by the fish.

As research in the field exploded, two opposing camps of physicists emerged: those who believed that animal electricity was limited to "electric fishes", and those who held that it applied to all animals. For their part, physiologists of the time speculated (without any experimental evidence) that *animal spirits* similar to the electric fluid (though by no means more clearly defined) were responsible for bringing sensations to the brain by traveling through nerves, and ultimately for provoking the contraction of voluntary muscles following a voluntary reaction.

In parallel, and partly influenced by the theories of animal electricity, hypnotic phenomena, known since the times of the ancient Persian magi, were attributed, owing to the intensified study of magnetism, to *animal magnetism*, conceived of as a vital fluid that cured a number of ills when transmitted from one living being to another. The Austrian doctor Franz Anton Mesmer (1734-1815) was a great promoter of this theory, which was roundly condemned by medical schools and viewed with suspicion even by the police at the time. The term "mesmerism", used to describe the phenomenon for which he advocated, later gave way to "hypnotism" in the 19th century.

It was this great sea of unfounded hypotheses, muddled ideas, erroneous analogies, and vague guesses that begot the work of Luigi Galvani (Fig. 7.12). Born in Bologna on 9 September 1737, Galvani initially studied grammar and rhetoric, and dedicated himself to medicine after being persuaded to not pursue his vocation for monastic life. After graduating with a degree in medicine in 1759, he began his scientific career as an "alumnus" of the Academy of Sciences of Bologna Institute. In 1766, he became a

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471 J. Walsh, *Of the Electric Property of the Torpedo*, in "Philosophical Transactions", 63, 1773, pp. 478-89.

professor of anatomy, and in 1782 switched to professor of obstetrics, a position he held until 1798, when, because of his allegiance to the pope, he refused to swear an oath of loyalty to the newly formed Cisalpine Republic and was expelled from his academic post and reduced to poverty. He died in Bologna on 4 December 1798. A man of even temperament, Galvani dedicated himself to scientific research and practicing medicine, living a mostly comfortable life in an ancient university until his last decade, where he was beset with domestic tragedies like the death of his wife (1790), from which he could not consoled.

Galvani began his anatomical studies of the muscular movement of frogs in 1773, and in 1780 he conducted the first electro-physiological experiments on these animals. After eleven years of research and experiments, he communicated his results in the famous work titled *De viribus electricitatis in motu muscolari commentarius*, published in 1791 in the "Commentarii" of the Accademia of Bologna and republished the following year by Giovanni Aldini (1762-1834), a nephew of Galvani, who added some notes and a dissertation.[472]

Galvani recounted the steps leading to his discovery as follows: "I dissected a frog, prepared it as is indicated in the figure (Fig. 7.13), and placed it on top of a table, completely separate and not a short distance away from the conducting piece of an electric generator; when one of my assistants accidentally touched the internal crural nerves of the frog with the tip of a scalpel, he observed all of the muscles in the frog's leg suddenly contract as if they had been afflicted by the most violent of tonic convulsions. While I was attempting other novel electric experiments, another one of my assistants that was standing closer to me observed that the phenomenon appeared to occur precisely when a spark was released by the conductor. Astonished by the novelty of the event, he immediately informed me, as I was entirely absorbed in other thoughts. Immediately, I felt an incredible desire to repeat the experiment and bring to light that which was still mysterious of the phenomenon."[473]

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[472] In 1937 Enrico Benassi was responsible for the first Italian translation, published with the Italian and the original Latin text side-by-side in L. Galvani, *Memorie ed esperimenti inediti*, Cappelli, Bologna 1937, pp. 83-192. The translation was republished in L. Galvani, *Opere scelte*, ed. G. Barbensi, Utet, Turin 1967, pp. 241-345.

[473] L. Galvani, *Opere edite ed inedite*, ed. Academy of Sciences of Bologna Institute, Bologna 1841, p. 63.

Fig. 7.12 – Luigi Galvani
Source: Alinari

Numerous later experiments allowed Galvani to confirm that the phenomenon occurred exactly as his assistant had described: every time the generator produced a spark, the frog was jerked by a convulsive tremor, provided, however, that its nerves were simultaneously touched by the experimenter with a conductor. The same results were also obtained with other animals, both warm and cold-blooded, and even when the spark was produced by a Leiden jar or an electrophorus.

It remained to be tested, Galvani continued in the second part of the paper, whether atmospheric electricity also produces the same effects as artificial electricity. To this end, he erected a tall conducting structure on the roof of his house and attached a frog to it by its crural nerves, while tying the frog's feet with a very long iron wire that was immersed in a well of water at its other end. Galvani observed that "every time that lighting struck, at the same instant all of the frog's muscles were overcome with violent and repeated contractions."[474] Furthermore, the contractions not only occurred when lighting struck, but also when dark, heavy clouds hung over the tip of the conductor.

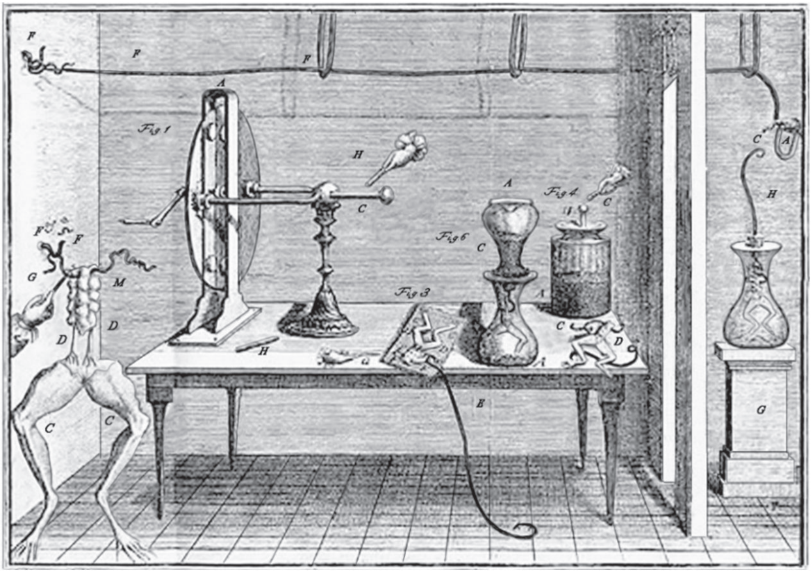

Fig. 7.13: Galvani's first experiments.
S*ource*: L. Galvani, *Memorie ed esperimenti inediti*, Cappelli, Bologna 1937

Based on these experiments, Galvani continued in the third part of the paper, he was prompted to test whether the contractions of the frog could occur not only due under a stormy sky, but also with the "placid electricity of a clear sky". He thus prepared several frogs and hung them using copper hooks from the railing that surrounded a hanging garden outside his house. In this way, he observed contractions in several instances, attributing them to the changes in atmospheric electricity, saying "it is easy to mislead ourselves when experimenting and think we have seen what we desire to see and find."[475] Yet soon he realized that the contractions were entirely unrelated to the changing state of the atmosphere. Galvani therefore decided to continue his experiments inside his house. He prepared a frog, placed it

[474] *Ibid.*, p. 76.
[475] *Ibid.,* p. 79.

on top of a table, and drove a hook into its spinal marrow, attaching the hook's other end to other parts of the frog, seeing "the same contractions, the same motion". He changed the experiment in many ways, but always obtained the same results. "These occurrences," Galvani commented, "did not fail to astonish me, and I began to have some doubts regarding the electricity inherent in an animal. It seemed to me that during the phenomenon the fluid flowed from nerves to muscles and a circuit like a Leiden jar was formed."[476]

Later experiments transformed this doubt into certainty: every time that the muscles and nerves of a freshly skinned frog were connected by a metallic wire, contractions were immediately observed. However, there was one experimental detail notable enough that Galvani made explicit mention of it: the contractions were more pronounced if the metallic wire was made up of two different metals: "Thus, for example, if the both the wire and the hook were iron, the contractions were often absent or extremely feeble. If, instead, one of the two was iron and the other made of copper, or better yet, silver (indeed, silver appears the most adept of the metals in transporting animal electricity), the contractions produced where much starker and lasted longer."[477] From these experiments, Galvani thought it justified to begin the fourth and last part of the paper by making the following affirmation: "Based on what is hitherto known and explored, I find it fairly clear that there exists in animals an electricity that I have taken the liberty, along with Bertholonius and others, of calling 'animal electricity'."[478]

The fourth section of the paper is then dedicated to the demonstration that animal electricity has the same characteristics and properties of the electricity produced by generators. Galvani later more fully described his theory of animal electricity in a 1795 paper (published in 1797) written as a letter to Lazzaro Spallanzani: electricity accumulates in muscle fibres to produce a state of imbalance; then, through the nerve connected to the muscle, it travels into the metallic wire and eventually returns to the muscle; in other words, according to Galvani, muscles and nerves are analogous to the two foils in a Leiden jar.

7.36 Alessandro Volta

In complete contrast with Galvani's spiritual nature, his cloistered life inside his city and university, and his scarce personal relations with the scientists of the time was the temperament of his great rival Alessandro

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[476] *Ibid.*, p. 80.
[477] *Ibid.*, p. 84.
[478] *Ibid.*, p. 100.

Volta (Fig. 7.14): extroverted, fun-loving, athletic, a tourist for both work and pleasure, and a lover of wordplay, fine dining, and women.

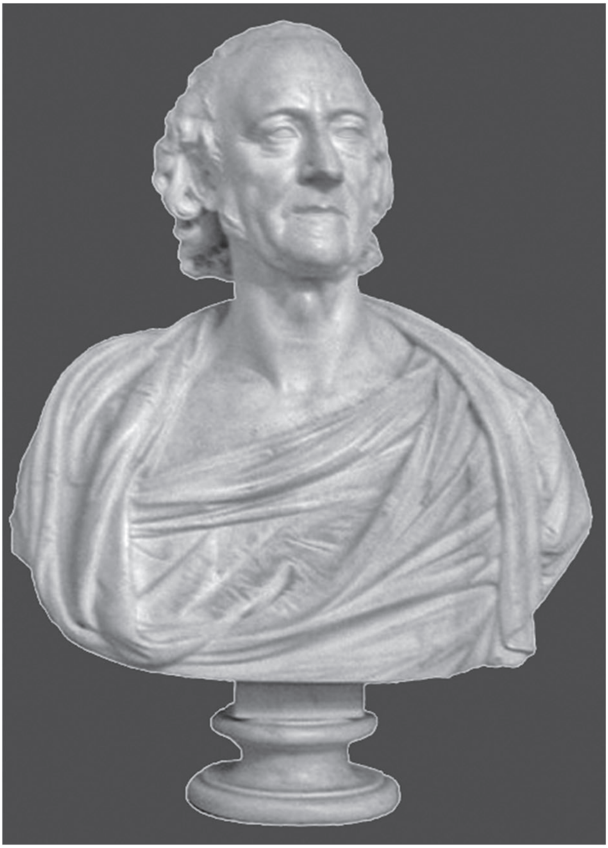

Fig. 7.14 –Alessandro Volta
Source: Alinari

Born in Como on 18 February 1745 to Filippo Volta, a man of modest means but noble descent, he received his first humanist education in the city's schools. With the help of his friend, an unusual monk, and without the guidance of a teacher, he soon began to study electric phenomena and, at the age of eighteen, he opened a correspondence with the famous electrologists of the time, like Beccaria and Nollet. In 1769, he published his first paper (discussed in § 7.30), which led him to build the electrophorus in 1775. In 1774 he was appointed "regent", or director, of the public schools of Como, and the following year he became a professor of physics at the same schools. Volta then moved to the University of Pavia in 1778, where he taught experimental physics for 35 years. In 1776, he discovered "flammable air originating from swamps", namely methane, wrote important papers on the topic, invented the electric "pistol", improved the audiometer, and travelled to Switzerland to study, where he met many scientists, including De Saussure, to whom he sent the aforementioned epistolary paper (§ 7.32). In 1780 he invented the capacitor (§ 7.32) and began a long scientific voyage through Switzerland, Belgium, the Netherlands, France, and England, allowing him to form close friendships with the eminent scientists of the time. In the two years period between 1786 and 1787, he studied the open questions of electrometry and electric meteorology (§ 7.31), and in 1791 he discovered the law of isobaric dilation of air, which he published three years later ([479]). Volta's great rivalry with Galvani began in 1799, which eventually brought him to the creation of the voltaic pile at the end of 1799, though he did not announce his invention until the following year, as we will soon detail. Napoleon named him senator of the Kingdom of Italy in 1809 and Count in 1814. That same year, he published his last great paper on the equivalence of electric and galvanic fluid; after this, his physical and especially mental faculties gradually weakened. Having retired to private life in Como, he died at the dawn of 5 March 1827.

Volta belonged to the faction of physicists entirely sceptical of *animal electricity*, and would never have repeated Galvani's experiments if he had not been asked by his physiological colleagues at the University of Pavia. Once he had verified the phenomena described by Galvani, however, his scepticism crumbled: "Finally," he wrote in a letter sent to Galvani on 3 April 1792, "I am converted, after I began to witness and spectate the miracles myself, and perhaps I have passed from incredulity to fanaticism."[480]

Volta may have passed to fanaticism, but not without judgement. On the 5th of May, in a public letter to the university, he referred to the experiments of Galvani, praising their acumen and lauding the importance of the discovery and the possibility of marvellous future developments, but began to make a few calls for greater quantitative rigour in the study of the phenomenon, because "what good can one do if things are not reduced to degrees and measures, especially in physics? How can the causes be assessed if one does not determine the quality as well as the quantity and

[479] § 2.2 in: M. Gliozzi, *A History of Physics over the Last Two Centuries.* Cambridge Scholars Publishing, in press 2022.
[480] Volta, *Le opere* cit., vol, 1, p. 26.

intensity of the effects?"[481] Volta began his personal studies on the matter in a direction influenced by his inclination towards physics: measuring physical quantities associated with the phenomenon.

His activity throughout the preceding decade, with the invention of the capacitor, the construction and comparison of electrometers, the clear demarcation of new quantities, and the experimental adroitness acquired from extensive practice quickly led Volta to recognize frogs as very sensitive *animal electrometers*, discover that electric current irritates nerves to cause muscle convulsion, and turn his attention to a physical detail already noted by Galvani (§ 7.35), who, however, had not considered it particularly important. On the other hand, Volta wrote: "That … for which I have not yet succeeded in finding a satisfactory explanation is the necessity of two different armatures… sometimes I wonder whether in reality the two metal conductors, either different in nature or applied in a different manner at two points of the animal, simply give the electric fluid, which is believed to naturally tend to move from one point to the other, a pathway to flow once the they come in contact with each other. In short, whether they are merely passive, or rather active agents that themselves move the electric fluid in the animal, and from the equilibrium in which it rests, cause it to then enter one armature and the exit through the other armature, breaking the equilibrium."[482]

At this point of his research, Volta, with the deep intuition of a great physicist, connected two apparently very distant phenomena: the sensitivity of the frog to electric current and the sensitivity of the eye to light. Thus was born the brilliant *Memoria seconda*[483] of 14 May 1792, written at the nadir of Volta's scientific inspiration, in which he rejected Galvani's interpretation: for Volta, the convulsions of the frog are not due to the supposed "animal capacitor", but rather to the irritation of its nerves caused by the electric fluid, which itself is moved by the "difference of metals" that make up the arc. The "flow of electric fluid" not only provokes the irritation of nerves associated with motion, but it also affects nerves associated with sense, triggering a sensation, as the following experiment showed: creating an arc between two different metals, the first placed at "one point of the tongue, the other on another point", one can taste an "acidic" or "alkaline tending towards bitter" flavour. In this way, in addition to the electroscope and the capacitor, Volta added a new and very sensitive instrument, his own tongue, and, some time later, his eye. When a short time later Aldini, hoping to devalue Volta's experiment, remarked that the tongue experiment had been

[481] *Ibid.*, p. 27.
[482] *Ibid.*, pp. 39-40.
[483] *Ibid.*, p. 41.

described in 1752 by the philosopher Johann Georg Sulzer (1720-1779), Volta replied that his own experiments were independent of Sultzer's "sole and solitary" observation.

If Galvani's experiments had "brought on a state of excitement", as Eusebio Valli (1762-1816) observed in the first of his letters on animal electricity[484], Volta's experiments caused an uproar, and in the second half of 1792 saw an explosion in articles on animal electricity that lasted twenty years and was comparable to the one that followed the first experiment with a Leiden jar. Galvani and Volta's experiments were repeated and changed, scientists took the side of the former or the latter, and many new relationships were discovered. Out of the multitude of scientists involved there were four key players in the story: on one hand Volta; on the other Galvani, his nephew Aldini, and the Tuscan doctor Valli.[485]

Volta's *Memoria seconda* was met with irreverent reply by Aldini, who claimed that the new experiments fit without conflict in the Galvanian framework, and were thus only an extension of Galvani's theory. Volta replied with a third paper published in November of 1792, in which, departing from the reserved tone of *Memoria seconda*, he formulated his own exclusively physical theory that contrasted with Galvani's interpretation: Galvani's experiments reveal exclusively physical phenomena, and the frog is a very sensitive instrument for measuring the electric current generated by the imbalance that arises when metallic conductors come into contact with liquid conductors. One end of the metallic conductor pulls electric fluid from the flesh of the frog where it is attached, while the other pushes the fluid.

In 1793 the Galvanists kept quiet, sharpening their weapons; Volta took advantage of this, both to finish his work on the uniform expansion of air (§ 2.2) and to advance his new research into contact electricity. In particular, he used this time to construct the Volta "scale", in which metallic conductors were ordered according to "their differing abilities to drive electric fluid and move it through humid conductors." The scale, "drafted at the beginning of 1793", contained twenty-five substances: zinc, tin foil, and lead at the top and gold, silver, manganese, copper, graphite, and charcoal at the bottom.

The peace lasted only one year. In 1794, the Galvanians mounted their offensive: Galvani himself was at the helm, authoring an anonymously

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[484] The letter, published in Como on 5 April 1792, was translated and published in the 41st tome of the "Observations sur la physique, sur l'histoire naturelle et sur les arts" by François Rozier, a Parisian scientific journal that was fairly popular at the time.

[485] For more details, cf. our own Introduzione a Volta, *Opere scelte* cit., from which this chapter heavily draws.

published pamphlet in mid-April, followed by another short work by Aldini that came out at the beginning of June. Galvani observed contractions in the frog even without the use of the metallic arc by bending its crural nerves to touch its leg muscle. Aldini, adjusting an experiment carried out by Galvani, also observed contractions using an arc made from a single metal. These new experiments dealt a serious blow to Volta's point of view: many scientists who were beginning to accept or had already accepted his theory were shaken in their beliefs and turned back to Galvani. Volta, however, could not stand being silenced so easily and struck back with an imprudent response[486]: to Galvani, he replied that the contractions observed could be attributed to a mechanical stimulus, dogmatically asserting that "nothing at all can ever be obtained …with only humid deferents"[487]; to Aldini, he replied that the convulsions that the younger scientist had observed were explained by the inevitable inhomogeneities at the edges of the mono-metallic arc, because only the contact between two heterogeneous bodies can produce contractions in a frog. With these words, Volta was arbitrarily inverting the law of contacts.

A scientist is truly on the ropes when he is forced to resort to controversial expedients, dogmatic affirmations, and arbitrary inversions of propositions. In this already dire situation, Volta's theory suffered an even greater blow. In a short and terse letter, Valli showed that the convulsions of the frog could be obtained by delicately bending its leg, without any applied pressure or sudden jerks, onto its spine so as to form a sort of arc. The observation was rather surprising; Valli, after describing some variations of the phenomenon, took on the assured tone of victory, though not without a note of derision directed at Volta. "One has motion," he wrote, "without the concourse of metals, hence metals are not the motors of electricity; it is not they who give rise to disequilibrium, they do not possess any magic virtues… From this moment, animal electricity ceases to be a problem. I am proud to have contributed to the triumph of a discovery that is the most beautiful and interesting of our century."[488]

His pride may have been justified, but his affirmation that metals are not motors of electricity was not. Indeed, upon closer inspection, Galvani's interpretation did not seem irreconcilable with Volta's after Valli's

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[486] A. Volta, *Nuova memoria sull'elettricità animale in alcune lettere al signor Abate Anton Maria Vassalli. Lettera seconda* (1794) in Id., *Le opera* cit., Vol. 1, p. 369.

[487] *Translator's note*: Humid deferent is Volta's terminology to refer to an organic carrier of electric current, like the crural nerve used by Galvani in his frog experiment.

[488] E. Valli, *Lettera XI sull'elettricità animale*, Mantova, 15 October 1794, pp. XVIII-XIX.

experiments, rather it simply referred to a different class of phenomena. In other words, Galvani's research was physiological, while Volta's was physical: in principle, there was no reason to compare the two. Seeing the possibility of reconciling the two theories, Volta rejected it, arguing that when all the relevant phenomena can be explained "solely by principle of the action of dissimilar conductors, a principle demonstrated by many other experimental tests that clearly speak for themselves and are unparalleled in number, why should we resort to another principle that is merely supposed, not proven, of an active electricity belonging to animal organs? Why introduce two completely different principles for entirely similar phenomena of the same type?"[489]

The contact principle had been "demonstrated", but not fully; it was "clear and spoke for itself", but not very forcefully. Volta's stance revealed his renewed mistrust directed at any hypothesis of animal electricity. However, he did not resort to controversial expedients; instead, Volta carefully repeated Valli's experiments, as he was suspicious of several experimental details that Valli himself had called attention to (in particular, the requirement that the parts of the frog that come into contact must be unwashed, dirty, and soaked in blood "or in another more or less viscous humour"). Volta examined these critically and was able to interpret them, without excessive logical gymnastics, as an extension of the principle of contact between metals (or conductors of the first class) and humid conductors (or conductors of the second class) to the case of contact between two humid conductors. It was on this extension of the contact principle that Volta conducted a series of critical experiments that he detailed in a brilliant work published in October 1795.

Yet, despite Volta's interpretative and experimental success, it was clear that the Galvanians remained in advantage. Indeed, while they succeeded in ruling out any physical means in the frog circuit, Volta had not been able to rule out organic means (the frog, its tongue, its eye). Until Volta was able to demonstrate the existence of contact electricity without the use of biological indicators, the results of his experiments could always be attributed to animal electricity. Volta had understood that the frog was not a physical electroscope: an electroscope measures tension (voltage), while the frog gives the "passage" of electric fluid, i.e. current. And because the physics of the time did not yet know of any instruments that measure current, the frog could not be replaced by a physical instrument as long as the experimental setup remained the classic Galvanian one. It was therefore necessary to modify the experimental apparatuses in such a way that the

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[489] Volta, *Nuova memoria sull'elettricità animale* cit., Id., *Lettera terza* (1796), in Id., *Le opere* cit., Vol. 1, p. 289.

phenomena associated with "passage of fluid" were transformed into ones associated with tension.

Having understood this concept, in August of 1796 Volta was able to translate it into experimental terms and measure the tension that arises from the contact between two heterogeneous conductors using only physical devices. At first, in 1796, he employed the duplicator, an instrument invented by Bennet, refined by Cavallo, and later improved in 1788 by William Nicholson. The duplicator consists of a weakly charged metallic disk that, when rotated by a crank, passes underneath two other fixed metallic disks and acts on them electrostatically. Through an interplay of alternating contacts, the charge on the three disks can increase with every rotation of the moveable disk. After a long time of using this "excellent little device", Volta realized that it could be replaced with the more advantageous condensing electroscope (§ 7.32). Extensive experimentation with the new devices led him to further extend the contact principle: an electric imbalance is caused by the contact of any two conductors, but this imbalance is small when both conductors are of the second class, average when one is of the first class and the other of the second, and large for contacts between two conductors of the first class.

This was to be the definitive theory, vigorously opposed and passionately defended for over a century and a half: there still is no final word on the "Volta effect"[490]. In a letter to his friend Friedrich Albert Gren (1760-1798), Volta described the law concerning the algebraic sum of contact tensions and the law concerning the principle of series connection.

All of a sudden, on 20 March 1800, Volta wrote a letter to Joseph Banks (1743-182)), president of the Royal Society, that began as follows: "After a long silence, for which I shall offer no apology, I have the pleasure of communicating to you, and through you to the Royal Society, some striking results I have obtained in pursuing my experiments on electricity… The principal of these results, which comprehends nearly all the rest, is the construction of an apparatus having a resemblance in its effects (that is to say, in the shock it is capable of arms, etc. Experience) to the Leiden flask, or rather, to an electric battery weakly charged acting incessantly, which

[490] *Translator's note*: While initially the "Volta effect", or "contact electricity" stood in opposition to Galvani's "animal electricity" as the more physically motivated theory, later advances, particularly in solid state physics, showed that the phenomena collected under the term "contact electricity" are actually different and can be ascribed to various causes. For example, the band theory idea of a Fermi level made gives a precise meaning to the potential difference between two metals in contact at equilibrium (See Ashcroft and Mermin, *Solid State Physics*, Saunders College Publishing, 1976)

should charge itself after each explosion; and, in a word, which should have an inexhaustible charge, a perpetual action or impulse on the electric fluid."[491]

It was a curious letter in which Volta announced the construction of the voltaic pile. We say curious for three main reasons. First, because it failed to describe the thought process that had led him from his ideas in 1796 to the invention, thus leaving a lacuna in the study of the evolution of Volta's ideas that even his handwritten papers cannot fill. In addition, Volta presented the apparatus as a reconstruction or imitation of the electric organ of electric fishes, which, in his entire scientific career, he had never studied of his own volition (the subject only appears in a didactic letter he wrote in 1782 to Madame Lenoir de Nanteuil of Paris), seemingly ignoring the physical question at hand. Lastly, in terms of disorganization and repetition, the paper is among the most poorly written by the scientist from Como.

Numerous hypotheses have been advanced to explain the strange behaviour of the scientist: perhaps impatience, emotion, or the desire to demonstrate the equality of electric fluid and "galvanic fluid" right away were the cause of the unusual presentation of the invention, which still today surprises students. More simply, we believe that the columnar form of the electromotive apparatus – the first device built by Volta, immediately followed by the device formed by a ring of cups – must have brought to mind the electric organ of the fish and brought Volta to reread an old paper by Cavendish (§ 7.32), which he would later publicly remember with admiration. In it, Cavendish compared the electric organ of the torpedo fish to a battery of extremely high capacitance and low tension, a comparison reprised by Volta. The rereading of the paper must have struck Volta's imagination, persuading him that presenting his own apparatus to English scientists as an imitation of the electric organ of the torpedo, following the footsteps of their own Cavendish, was the best way to convince them on the spot of the equivalence between electric fluid and galvanic fluid.

The appearance of the voltaic pile, which spread rapidly throughout Europe, marked a new era in the history of physics, and even perhaps in the history of civilization. Research quickly became oriented in three intertwined and mutually influencing directions: the study of the new and surprising phenomena, especially of chemical nature, exhibited by the pile, which were immediately noted by Volta; the construction of increasingly powerful and more convenient piles, or piles tailor-made for particular uses; and the study of the "fluid" moved by the pile.

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[491] *Ibid.,* Vol. 1, p. 565. The original letter is written in French, but the Italian translation can be found in Volta, *Opere scelte* cit., pp. 514-34, while the English translation can be found in *The Philosophical Magazine*, p. 289, September 1800.

Volta paid little attention to the first two research directions, an attitude criticized by many historians, who are used to putting the past on trial and teaching lessons, with the convenient benefit of hindsight, to the scientists of previous eras. Pushed the controversy with the Galvanists and his own passion for metrology, Volta dedicated almost all of his efforts to the third problem. The Galvanists, beaten in the battle to locate the electric fluid, had hypothesized the existence of a new agent called *galvanic fluid* a few years before the invention of the pile (according to our research, the term *galvanism* was introduced by Gren in 1796), a substance related to but distinct from electric fluid, and responsible for both properly galvanic phenomena and those exhibited by the voltaic pile. They based their interpretation on three phenomena: the fact that either voltaic piles did not produce electrometric signals at all or produced very weak ones; that some bodies, though conductors of electric fluid, appeared to be insulators for the fluid in voltaic piles; and that the fluid in the voltaic pile inexplicably exhibited eye-catching chemical and physiological effects, while corresponding effects were much weaker for the fluid in electrostatic machines.

This interpretation was once again met with a response from Volta, this time in the form of the famous *Memoria sull'identità del fluido elettrico col fluido galvanico*, read in Autumn of 1801 in three sessions at the Institut de France with Napoleon present, a moment that marked the apex of Volta's scientific career. Supported by a rich and varied compendium of quantitative experimental data, Volta countered that the presumed difference in the behaviour of the two fluids was due to their different tensions and to the different quantities of fluid moved by the apparatuses; the effects would be identical, even quantitatively, if the products of tension and the quantity of moving fluid were the same. In light of the experimental facts, the equivalence between electric fluid and galvanic fluid seemed to him so evident that it would have been "pertinacious and a real scandal to continue to deny or doubt [it]."[492]

Yet there were those who continued to deny and doubt, though ever more feebly, for several decades: in practice, the controversy was finally put to rest in 1833 by Faraday. While Italian scientists (Vassalli Eandi, Aldini, Valli, and others) were the most ardent opponents of Volta's ideas, there was no dearth of scientists from other countries that added their own new criticisms to the older ones. For example, in 1829 Humphry Davy (1778-1829), and three years later his less famous brother John (1790-1868) doubted the equivalence of the two fluids. In 1832, William Ritchie (1790-1837) opposed Volta's theory with the observation that frictional electricity

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[492] A. Volta, *Memoria sull'identità del fluido elettrico col fluido galvanico*, in Id., *Le opere* cit., Vol. 2, p. 45.

is distributed around metals, while "Voltian" electricity is distributed *inside* metals.

The obstinacy of Volta's opponents was nourished by several scientific and emotive reasons that are difficult to comprehend today: loyalty to Galvani's memory; the difficulty of relating Volta's complicated idea of tension and his even more arcane, inchoate conception of electric energy, with its one intensive parameter and one extensive parameter, tension and quantity of electric fluid; astonishment at the surprising effects of the fluid in the voltaic pile. In our view, however, the primary reason behind opposition to Volta's theory was of a psychological nature, even though the opposing scientists themselves may not have realized it: the unease associated with attributing a biological process solely to physical phenomena, a feeling that in some became a veritable trauma complete with philosophical and religious implications.

The initial deafness of the scientific environment, especially in Italy, to Volta's theory pushed him to write a new paper on the voltaic pile in 1804-05 with almost the same title as the one from 1801. After many attempts, the paper was finally published in a pamphlet by Pietro Configliachi (1777-1844) in 1814, who wrote an ambiguous preface that led 19th century historians to doubt the authenticity of the paper itself. Yet the rediscovery and the publishing of Volta's original manuscript in 1901 quashed all doubt. The paper, which can be considered Volta's scientific will and testament, is a rewriting and extension of the paper read to the Institut de France in 1801: it even includes a note on an interesting experimental study on the relationship between tension and explosive distance, with the discovery of the law according to which "the distances that the spark can reach grow almost exactly with the tension."[493]

The Volta's discovery radically changed the human life and, we will allow ourselves to say, the face of the Earth. Humanity would have to wait 142 years for the advent of nuclear energy to possess an instrument whose consequences are comparable to those of the voltaic pile.

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493 A. Volta, *L'identità del fluido elettrico col così detto fluido galvanico vittoriosamente dimostrata con nuove esperienze ed osservazioni,* in Id., *Le opere* cit., Vol. 2, p. 205.

Bibliography

Arago, François. 1992. *Oeuvres complètes.* New York: Readex.

Archimedes, 2002. *The Works of Archimedes*, edited by T. L. Heath. New York: Dover.

Bacon, Roger. 1900. *Opus majus*, edited by J.H: Bridges. London: Williams and Norgate.

Bloch, Leon. 1908. *La philosophie de Newton*. Paris: Alcan.

Boll, Marcel. 1944. *L'occultisme devant la science*. Paris: Puf.

Brewster, David. 2010. *Memories of the Life, Writings, and Discoveries of Sir Isaac Newton.* Cambridge: Cambridge University Press. Doi:10.1017/CB09780511792687

Brown, Harcourt. 1934. *Scientific Organizations in the Seventeenth Century France*. Baltimore: Williams & Wilkins.

Brunet, Pierre. 1926. *Les physiciens hollandais et la methode expérimentale en France au XVII siècle*. Paris: Blanchard.

Brunet, Pierre. 1931. *L'introduction des théories de Newton en France au XVIII siècle (avant 1738)*. Paris: Hermann.

Brunet, Pierre. 1938. *Etude historique sur le principe de la moindre action*. Paris: Hermann.

Brunet, Pierre, Mieli, Aldo. 1935. *Histoire des sciences: antiquité*. Paris: Payot.

Buchwald, Jed, and Fox, Robert. 2013. *The Oxford Handbook of the History of Physics.* New York: Oxford University Press.

Carli, Alarico, and Favaro, Antonio. 1896. *Bibliografia galileiana*. Rome: Ministero della Pubblica Istruzione

Clagett, Marshall. 1959. *The Science of Mechanics in the Middle Ages.* Madison:University of Wisconsin Press.

Clavelin, Maurice. 1968. *La philosophie naturelle de Galilée*. Paris: Colin.

Cohen, I. Bernard. 1956. *Franklin and Newton. An Inquiry into Speculative Newtonian Experimental Science and Franklin Work in Electricity as an Example Thereof*. Philadelphia: American Philosophical Society.

Cohen, I. Bernard, and Smith, George, ed. 2002. *The Cambridge Companion to Newton*. Cambridge- New York: Cambridge University Press.

Copernicus, Nicolaus. 1973-92. *Oeuvres complètes,* 4 vols. Paris: CNRS (Centre National de la Recherche Scientifique).

Crombie, A. C. 1957. *Augustine to Galileo: the history of science A. D. 400-1650*. London: Heinemann.

Crommelin, Claude August. 1951. *Descriptive Catalogue of the Physical Instruments of the 18th Century at Leyden*. Leyden: Rijksmuseum voor de Geschiedenis der Natuurwetenschappen

D'Alembert, Jean-Baptiste.2002. Oeuvres complètes. Paris: CNRS (Centre National de la Recherche Scientifique).

Dampier, William. 1948. *A History of Science and Its Relations with Philosophy and Religion.* Cambridge: Cambridge University Press.

Daumas, Maurice. 1953. *Les instruments scientifiques aux XVII*^e^ *et XVIII*^e^ *siècles.* Paris: Puf.

De Santillana, Giorgio. 1955. *The crime of Galileo*. Chicago: University of Chicago Press.

Descartes, René. 1897-1910. *Oeuvres,* edited by C. Adam and P.Tannery. Paris: Cerf.

Drachmann, A.G. 1963. *The Mechanical Technology of Greek and Roman Antiquity*. Copenhagen: Munksgaard.

Dugas, René. 1954. *Histoire de la mécanique*. Neuchatel: Griffon.

Dugas, René. 1954. *La mécanique au XVII*^e^ *siècle.* Neuchatel: Griffon.

Duhem, Pierre. 1905-06. *Les origines de la statique,* 2 vols. Paris: Hermann.

Duhem, Pierre. 1906-13. *Etudes sur Léonard de Vinci. Ceux qu'il a lu et ceux qui l'ont lu,* 3 vols. Paris: Hermann.

Duhem, Pierre. 1913-1959. *Le système du monde. Histoire des doctrines cosmologiques de Platon à Copérnic,* 10 vols. Paris: Hermann.

Euler, Leonhard. 1802. *Letters to a German Princess*. Translated by Henry Hunter. London: Murray and Highley.

Franklin, Benjamin. *Autobiography*. 1908. London: J.M. Dent & Sons.

Galilei, Galileo. 1992. *Le opere,* 20 vols. New York: Readex.

Galvani, Luigi. 1967. *Opere scelte,* edited by G. Barbensi. Turin: Utet.

Garber, Elizabeth. 1998. *The Language of Physics. The Calculus and the Development of Theoretical Physics in Europe, 1750-1914*. Boston-Basel-Berlin: Birkhauser.

Gillispie, C. C., ed. 1970-80. *Dictionary of Scientific Biography,* 16 vols. New York: Scribner.

Gliozzi, Mario. 2022. *A History of Physics over the Last Two Centuries.* Cambridge Scholars Publishing, in press.

Grant, Edward. 1997. *The Foundations of Modern Science in the Middle Ages.* Cambridge: Cambridge University Press.

Guillaume. 1838-41. *Histoire des sciences mathématiques en Italie depuis la renaissance des lettres jusqu'à la fin du dixseptième siècle,* 4 vols. Paris: Renouard.

Hakfoort, Casper. 1994. *Optics in the Age of Euler. Conceptions of the Nature of Light, 1700-1795.* New York: Cambridge University Press.

Hall, A. Rupert. 1954. *The Scientific Revolution, 1500-1800.* London: Longmans & Green.

Hall, A. Rupert. 1963. *From Galileo to Newton,* 1630-1720. London: Collins.

Hall, A. Rupert, and Hall, M. Boas, ed. 1962. *Unpublished Scientific Papers of Isaac Newton. A Selection from the Portsmouth Collection in University Library Cambridge.* New York: Cambridge University Press.

Halliwell, James Orchard. 1841. *A Collection of Letters Illustrative of the Progress of Science in England from the Reign of Queen Elizabeth to that of Charles the Second.* London: Historical Society of Science.

Harman, Peter M. 1993. *After Newton. Essays on Natural Philosophy.* Aldershot: Variorum.

Hart, Ivor B. 1961. *The World of Leonardo da Vinci, Man of Science, Engineer and Dreamer of Flight.* London: Macdonald.

Hartley, Harold. 1960. *The Royal Society, Its Origins and Founders*. London: Royal Society.

Heiberg, Johan Ludvig. 1922. *Mathematics and Physical Science In Classical Antiquity.* Translated by D. C. Macgregor. London: H.Milford, Oxford University Press.

Heilbron, J. L. 2018. *The History of Physics: a very short introduction*. New York: Oxford University Press.

Hoppe, Edmund. 1926. *Geschichte der Optik.* Leipzig: Weber.

Huygens, Christiaan. 1888-1950. *Oeuvres complètes*. edited by the Société hollandaise des sciences, 22 vols. La Haye: Nijhoff.

Jammer, Max. 199*3. Concepts of Space. The History of the Theories of Space in Physics.* New York: Dover.

Jammer, Max. 1957. *Concepts of Force. A Study in the Foundation of Dynamics.* Cambridge (Mass): Harward University Press.

Jeans, James. 1930. *The Mysterious Universe.* New York: Macmillan.

Juskevic, A. P., and Winter, E., and Hoffmann P., and Kopelevic, J. C. 1959-*61. Die Berliner und die Peterburger Academie der Wissenchaften im Briefwechsel Leonard Eulers, 1730-1763,* 2 vols. Berlin: Akademie-Verlag.

Kepler, Johannes. 1866. *Opera omnia,* edited by C. Frisch, 8 vols. Frankfurt and Erlangen: Heyder & Zimmer.

Kepler, Johannes. 1972. *Dissertatio cum Nuncio Sidereo accedit Narratio de quatuor Iovis satellibus,* edited by Elio Pasoli and Giorgio Tabarroni. Turin: Bottega d'Erasmo.

Koyré, Alexandre. 1939. *Etudes Galiléennes.* Paris: Hermann.

Koyré, Alexandre. *1961. La révolution astronomique. Copernic, Kepler, Borelli.* Paris: Hermann.

Kraus, Paul.1942-43. *Jabir Ibn Hayyan, Contribution à l'histoire des idées scientifiques dans l'Islam,* 2 vols. Paris: Institute d'Egypte.

Lagrange, Joseph-Louis. 1867-92. *Oeuvres.* Paris: Gauthier-Villars.

Laplace, Pierre Simon. 1843-47. *Oeuvres*. Paris: Imprimerie Royale.

Leonardo da Vinci. 1894-1904. *Il Codice atlantico,* edited by Regia Accademia dei Lincei. Milan: Hoepli. Libri,

Lindley, David. 2001. *Boltzmann's Atom: The Great Debate that Launched a Revolution in Physics.* New York: Free Press.

Livingston, James, D. 1996. *Driving Force. The Natural Magic of Magnets.* Cambridge (Mass): Harward University Press.

Luthy, Ch., and Murdoch, J. E., and Newmam, W. R., 2001. *Late Medieval and Early Modern Corpuscular Matter Theories. Medieval and Early Modern Science.* New York: Brill Academic Publishing.

Mach, Ernst. 1896. *Die Principien der Warmelehre historisch-kritisch entwickelt.* Leipzig: Barth.

Mach, Ernst. 1953. *The Principles of Physical Optics.* New York: Dutton.

Mach, Ernst. 2013. *The Science of Mechanics: A Critical and Historical Exposition of its Principles.* Translated by McCormack, T. Cambridge: Cambridge University Press.

Maclaurin, Colin. 1748. *An Account of Sir Isaac Newton Philosophical Discoveries*. London: Miller-Nourse.

Martin, Thomas, ed. 1932-36. *Faraday's Diary,* 7 vols. London: Bell.

McMullin, Ernan, ed. 1967. *Bibliografia galileiana,* in "Galileo Man of Science". New York: Basic Books.

Menshutkin, Boris. 1952. *Russia's Lomonosov, Chemist, Coutier, Physicist, Poet*. Princeton: Princeton University Press.

Metzger, Helen. 1930. *Newton, Stahl, Boerhave et la doctrine chimique.* Paris: Alcan.

Motz, Lloyd and Weaver, Jefferson Hane. 1989. *The Story of Physics.* New York: Plenum Press.

Mouy, Paul. 1934. *Le développement de la phyisique cartésienne 1646-1712.* Paris: Vrin.

Newton, Isaac. 1687. *Philosophiae Naturalis Principia Mathematica.* London.

Newton, Isaac, 1846. *Newton's Principia. The Mathematical Principles of Natural Philosophy,* Translated by Andrew Motte. New York: Daniel Adee.

Newton, Isaac. 1958. *Papers and Letters on Natural Philosophy and Related Documents,* edited by I. B. Cohen and R. E Schofield. Cambridge: Cambridge University Press.

Ornstein, Martha. 1963. *The Role of Scientific Societies in the Seventeenth Century.* Hamden (Conn): Anchon Books.

Poggendorff, Johann Christian. 1863. *Biographisch-literarisches Handworterbuch zur Gaschichte der exacten Wissenschaften,* 2 vols. Leipzig: Barth.

Porter, Neil A. 1998. *Physicists in Confilcts. From Antiquity to the New Millenium.* Bristol-Philadelphia: Institute of Physics Publishing.

Randall, John Herman. 1961. *The School of Padua and the Emergence of Modern Science*. Padova: Antenore.

Rey, Abel. 1930-46. *La science dans l'antiquité,* 4 vols. Paris: La Renaiissance du livre. Royal Society, ed. 1947. *Newton Tercentenary Celebrations.* Cambridge: The Royal Society.

Sambursky, Samuel. 1956. *The Physical World of the Greeks*. New York: McMillan.

Sarton, George. 1927-48. *Introduction to the History of Science,* 3 vols. Baltimore: William & Wilkins.

Shelby, T. M. 1952. *French Inventions of the Eighteenth Century*. Lexinton: University of Kentucky Press.

Speyer, Edward. 1994. *Six Roads from Newton. Great Discoveries in Physics.* New York: Wiley.

Taton, René, ed.1957-64. *Histoire générale des sciences,* 3 vols. Paris: Puf.

Thorndike, Lynn. 1964. *A History of Magic and Experimental Sciences during the First Thirteen Centuries of Our Era,* 2 vols. New York: Columbia University Press.

Thorndike, Lynn, and Kibre, Pearl. 1963. *A Catalogue of Incipits of Medieval Scientific Writings in Latin.* Cambridge (Mass): The Mediaeval Academy of America.

Todhunter, Isaac. 1873. *A History of Mathematical Theories of Attraction and the Figure of the Earth. From the time of Newton to that of Laplace,* 2 vols. London: MacMillan.

Torricelli, Evangelista. 1919. *Opere.* edited by Gino Loria and Giuseppe Vassura. Faenza: Montanari.

Trancer, More L. 1934. *Isaac Newton.* New York-London: Schribner.

Truesdell, Clifford. 1968. *Essays on the History of Mechanics.* Berlin-Heidelberg-New York: Springer.

Turnbull, Herbert Western, and Scott, J. F., ed. 1959. *The Correspondence of Isaac Newton,* 7 vols. Cambridge: Cambridge University Press.

Vailati, Ezio, ed. 1997. *Leibniz and Clarke. A Study of Their Correspondence.* New York: Oxford University Press.

Verga, Ettore. 1931. *Bibliografia vinciana 1493-1930,* 2 vols. Bologna: Zanichelli.

Verschuur, Gerrit, L. 1993. *Hidden Attraction. The History and Mystery of Magnetism.* New York: Oxford University Press.

Volta, Alessandro. 1918-29. *Le opere,* 7 vols. Milan: Hoepli.

Volta, Alessandro. 1949-55. *Epistolario,* 5 vols. Bologna: Zanichelli.

Volta, Alessandro. 1967. *Opere scelte,* edited by Mario Gliozzi. Turin: Utet.

Von Laue, Max. 1950. *History of Physics*. New York: Academic Press.

Weaver, Jefferson Hane, ed. 1987. *The World of Physics. A Small Library of the Literature of Physics from Antiquity to the Present,* 3 vols. New York: Simon & Schuster.

Wolf, Abraham. 1935. *A History of Science, Technology and Philosophy in the 16th and 17th Centuries.* London: Allen & Unwin.

Zubov, Vasilii Pavlovich. 1968. *Leonardo da Vinci, 1452-1519.* Translated by David H. Kraus. Cambridge (Mass): Harward University Press.

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