

Questioning Theoretical Primitives in Linguistic Inquiry

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Questioning Theoretical Primitives in Linguistic Inquiry

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Volume 76

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Papers in honor of Ricardo Otheguy

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First principles in linguistic inquiry

Daniel Erker and Naomi Shin

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This book is a collection of papers dedicated to Ricardo Otheguy. The contributors to the volume, who rank among the leading scholars in linguistics, were encouraged to imbue their writing with the characteristics that have defined Ricardo's numerous contributions to the field: a commitment to clarity at the level of first principles, a penetrating skepticism towards presumed truths, and a mindfulness of the responsibility of linguists to foster public understanding of language. As his many students, collaborators, and colleagues can attest, these attributes have shaped Ricardo's teaching, research, and professional interactions. This impression is not limited to those who know him personally, however, because Ricardo's socially mindful, fiercely skeptical pursuit of clarity and truth on the topic of human language has its fullest articulation in his writing – from his very first publication to those currently under review.

In 1973, Otheguy made his debut as a published scholar with a challenge to Germán de Granda, the leading proponent of the view that the Spanish of Africans in the colonial Antilles should be viewed as a creole. Otheguy was driven to write the paper not because he disagreed with Granda's classification of *habla bozal antillana* as an Afro-Caribbean creole, but because he thought that Granda was "right for the wrong reasons" (Otheguy, 1973: p. 326). In an analytical tour de force, Otheguy reassessed Granda's data, uncovering the phonological, morphological, and syntactic evidence he thought necessary to make a compelling case for the creole roots of the present day Spanish Caribbean.

Forty two years later, in a 2015 paper written with Ofelía García and Wallis Reid, we find Otheguy engaged in a complementary pursuit, namely, critiquing a position he and his collaborators view as being wrong for the right reasons. In that paper, Otheguy et al. argue that *code-switching* is a fundamentally defective concept, even when used by those who rightly champion the linguistic behavior of multilinguals as masterful and virtuosic. This is because the idea itself rests upon two faulty assumptions, one built on top of the other. The first assumption is that named languages, whether described in terms of nation states or social groups, are coherent linguistic objects. The second assumption is that the minds of multilinguals contain

two (or more) named languages, hermetically sealed off from each other. Otheguy et al. dismantle each of these assumptions, reminding us that named languages can never successfully refer to the linguistic knowledge and behavior of individual language users. Instead, they can only meaningfully point to the social history of people and the groups to which they belong. With the named language off the table as an option for describing the linguistic content of human minds, the case for code-switching crumbles, as it no longer makes sense to talk about the alternation between two named linguistic systems. As an alternative, Ricardo, Ofelía, and Wallis endorse *translanguaging*, a concept that refocuses attention on individual language users and sees their linguistic behavior as a process of selecting features from a unitary repertoire, or idiolect.

These younger and older Otheguy – the ones on display in these papers from 1973 and 2015 – are not identical to one another. As his career progressed, so did the degree of difficulty and the variety of the topics Ricardo engaged. The remarkably broad range of challenging themes that he has written impactfully on is consistent with the fact that he has always eschewed a scholarly identity articulated in terms of named schools of linguistic thought. It is true that he wears well the hats of a Diverian, a dialectologist, a functionalist, and a variationist sociolinguist. Yet, while these elements are central to Ricardo’s intellectual style, they by no means exhaust it. Instead, they are better seen as familiar items in an even larger intellectual wardrobe, one from which the appropriate dress is selected on the basis of the scholarly climate that Ricardo needs to weather. This flexibility has made it possible for him to increase our understanding of a collection of topics that might otherwise seem disparate, *e.g.*, the analytical value of traditional grammatical categories such as *reflexive* and *subject* (Otheguy, 2002), the variable pronominal behavior of Spanish speakers in New York City (Otheguy & Zentella, 2012), the linguistic and social coherence of *Spanglish* (Otheguy, 2009), and the utility of the notion of *incompleteness* in the context of language acquisition (2016), among others.

Over time and across intellectual stylings, Ricardo’s work is bound by an unwavering impulse to expose what has been taken for granted, to challenge it if need be, and to offer a course correction. More importantly, reading his work is to be reminded of the higher purpose of linguistics, which is the illumination of the human condition. With this clearly in mind, it is easier to appreciate the stakes of our work: getting the linguistics wrong amounts to getting people wrong. The real reason Ricardo took aim at Granda in 1973 was not to repair a faulty morphophonological analysis. It was to better represent the linguistic experiences of the people whose lives constitute the story of Spanish in the Caribbean. While the ire he directs towards code-switching, named languages, and incomplete acquisition has a genuine intellectual basis, it arises from concerns that when these ideas are uncritically

brought to bear on the linguistic experiences of individuals (and of young people, in particular), they are likely to perpetuate misunderstanding.

In designing this volume, we hoped to honor Ricardo by asking contributors to channel their own *inner-Otheguy*. To our delight, they not only gamely agreed, but each of them also knew exactly what it was that we were asking them to do – a testament in its own right to the clarity of Ricardo's intellectual style and bearing. The resulting volume is a collection of papers that ask decidedly primitive questions. By this, we do not mean that their concerns are unsophisticated or rudimentary. To the contrary, the authors included in this volume offer subtle, complex, and deeply thoughtful discussions of an array of pressing issues, ranging from the nature of grammar and the role of politics in linguistics to the value of null elements in structural analysis and the definition of the sociolinguistic variable. What makes these papers *primitive* is that they engage their topics in the service of concerns that are foundational in nature. In other words, while they seek to illuminate particular phenomena or argue for a certain interpretation of a given set of facts, they share a deeper goal of shedding light on the enterprise of linguistic inquiry itself.

Each contribution to this volume reminds us of the need to routinely revisit, clarify, and assess our positions on what constitutes the elemental content, methodology, and goals of linguistics. This is a necessity because, in the study of human language, deciding how and where to begin is itself a theoretical move, one that has substantial downstream effects on what the ensuing investigation will (and can) discover. In the same way that the location and structure of an observatory shapes how its users view, and therefore, describe and explain the cosmos, so does the nature of the edifice of linguistic science shape how we see and understand language. This book amounts to a self-inspection of the load-bearing components of that edifice. In other words, in this volume, a collection of leading linguists can be seen doing the work of their craft in order to probe the soundness of the very categories, concepts, and units of observation that make this work possible.

These foundational components are what the first half of the book's title is meant to capture. By *first principles*, we refer to the theoretical primitives that guide linguists in their search for an understanding of human language. An insight shared by these papers is that such primitives have a deep impact on how that search unfolds. First names determine the types of questions that are deemed valid, the kinds of data that are considered relevant, and the sets of goals that are celebrated as worthy of the field. Among those interrogated here are such familiar primitives as *the linguistic sign, a language, structural relations, noun, verb, grammar, acquisition, bilingual, heritage language, linguistic variable, falsifiability, and the envelope of variation*. Although the contributors to the present volume work across a range of research traditions (e.g. generativist, variationist, functionalist), they are, like the

work of the scholar they are meant to honor, bound together by a concern for transparency and deliberation at the outset of analysis. In some cases, they argue that a particular primitive should be thrown out and replaced. In others, recalibration is suggested instead. Together they make a powerful case for careful consideration of where we begin in linguistic inquiry.

In the following we provide a brief overview of the contents of the volume:

Shana Poplack examines what she calls ‘the Doctrine of Form-Function Symmetry’, the notion that differences in linguistic form neatly correspond to differences in meaning. Working from a data-driven, variationist perspective, Poplack deftly highlights the limitations of this doctrine. On the basis of robust empirical evidence, she demonstrates that the usage of French subjunctive and indicative forms is decidedly resistant to the notion of form-function symmetry. Her chapter emphasizes the importance of grounding linguistic theory in the facts of language use. Similarly, **Gregory Guy** stresses the role of variation in linguistic inquiry, arguing that the linguistic competence of human beings is fundamentally probabilistic in nature. Guy tours a range of phenomena that present intractable problems for the view that the human language faculty is solely attuned to the categorical and qualitative. Continuing with the theme of variation, **Catherine Travis** and **Rena Torres Cacoulios** take aim at conventional conceptions of grammatical person. As they note, third person is often considered a ‘non-person’ in grammatical terms; however, their careful analysis of patterns of Spanish discourse uncovers systematic differences in the use of 1sg and 3sg subject pronouns, leading them to conclude that third person is a *transient person* in discourse.

The next three chapters adopt a form of radical functionalism known as Columbia School Linguistics. Each of these chapters amounts to a challenge of traditional grammatical categories. For example, **Wallis Reid** rejects the categories of nouns and verbs in English. Noting that many words, like *walk* and *talk*, function both as what are typically considered nouns and verbs, he argues that speakers manipulate their linguistic resources for expressive purposes. Reid also rejects conventional grammatical gender categories as analytical starting points. Instead, he starts with a distributional problem, which is the observation that certain forms tend to co-occur. He solves this distributional problem by positing a communicative strategy whereby speakers “choose all gendered signs that describe the same thing (i.e., that have the same conceptual ‘referent’) from the same gender class.” This, he notes, is not a syntactic rule, as it can be eschewed for communicative purposes. In other words, he arrives at a different solution to the reason for gender classes precisely because he began his analysis without the assumption of gender classes. Similarly, **Joseph Davis** tackles a concept deeply embedded in a linguistic theory: null structural elements. Davis argues that null forms are warranted when the analysis of structure depends simultaneously on what it is and what it is not. For example,

he writes: “The distribution of [Italian] *si* can be accounted for only by an analytical appeal to those very semantic substances in the network of systems of which *si* is a part, which are *not* signaled by *si*. *Si* is present at a point in a text because of what *si* is not.” Nancy Stern also adopts a Columbia School approach in her analysis of constructions with three participants (so-called ditransitives). Stern argues that the distribution of participants is constrained by their varying degrees of control over the event. For example, she shows that in *The girl gave the wall a push*, the wall has a higher degree of control than we typically expect from inanimate objects. Stern thus is able to show the semantic consequence of choosing a three-participant construction instead of one that only has two participants.

The final four chapters of the volume focus on Spanish speakers in the United States, reviewing and critiquing the goals of linguistic inquiry, the coherence of named languages, and the value of ‘incomplete’ as a modifier of language acquisition. Ana Celia Zentella discusses the dearth of Latin@s (or, using her term, LatinUs) in linguistics and the social and intellectual repercussions of their absence has on the field. She advocates a “re-imagined linguistics that strengthens connections to family and community, and underscores the role of language in the pursuit of social justice.” Rachel Varra’s chapter heeds Zentella’s call for a stronger connection to community. She confronts Otheguy’s own stance on Spanglish, which he sees as a term that perpetuates the myth of hybrid linguistic systems and creates confusion around patterns of variation that would, in other contexts, be characterized as innovation in or simply a different variety of Spanish (e.g., Otheguy & Stern, 2011). Varra considers criteria routinely used in the granting of named language status, and she concludes that Spanglish satisfies all of them.

The last two chapters of the volume engage the concept of *incomplete acquisition*, a notion that Otheguy (2016) sees as wrong-headed, particularly in its assumption that monolingual, educated speakers are appropriate measuring sticks for gauging the completeness of the grammars of bilinguals. Acknowledging Otheguy’s views, Carmen Silva-Corvalán nonetheless argues that some speakers do fail to acquire specific features that are clearly present in the linguistic input. She draws on her study of her own grandchildren to show how these children were exposed to, but did not acquire, particular linguistic features. Marcel den Dikken offers a contrary view, arguing that as lens for viewing the behavior of Spanish speakers in NYC, incomplete acquisition presents a distorted picture. Taking a generative syntactic approach to the variable use of subjunctive verbal morphology, den Dikken observes that it is not the subjunctive *per se* but rather its distribution that appears to be undergoing intergenerational change. He concludes that while the distribution of the subjunctive among Spanish speakers raised in New York City may differ from that seen among monolinguals, the variation in distribution does not amount to evidence that the subjunctive has been incompletely acquired.

In closing, permit us to make one final observation about the scholar whose linguistics is celebrated and honored by the following pages: In addition to being a lifelong student of language, Ricardo is also an avid baseball fan. He watches the game much like he observes linguistic phenomena – with an eye towards the fundamentals. More often than not, the truth of a game lies not in the scoreboard itself. Indeed, the number of runs, hits, and errors that each team accumulates over nine innings are the downstream effects of more fundamental actions: Was the pitcher gripping the ball across the seams all night? Was the center fielder catching fly balls on his throwing side? Was the lead-off hitter working the pitch count? The outcome of our work as linguists depends on similarly fundamental moves, and the extent to which we are able to tell the truth about language depends on how carefully we consider the very first moves we make.

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Categories of grammar and categories of speech

When the quest for symmetry meets inherent variability

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This chapter tracks the response to morphosyntactic variability in a massive corpus of prescriptive grammars of French dating from the 16th century through the present, and relates it to current mainstream approaches. Analysis shows that although variant *forms* have been recognized since the earliest times, only rarely have they been acknowledged as variant expressions of the same meaning or *function*. Instead three major strategies are marshaled to factor variability out. Their aim is not to prescribe or even describe, but simply to associate each form with a dedicated context of occurrence, in keeping with the dictates of the traditional grammatical categories from which they derive. This state of affairs is encapsulated in the *Doctrine of Form-Function Symmetry*. Although it fails to account for the data of spontaneous speech (which reveals asymmetry in the form of robust variability subject to regular conditioning instead), it continues to mold both prescriptive and formal linguistic treatments of variability, contributing to the growing gulf between prescription, description, and actual usage.

Keywords: prescription, praxis, variability, form-function symmetry, French, grammatical tradition, variationist sociolinguistics, Columbia School, linguistic variable, usage data

1. Introduction

The genesis of the long-term project described in this chapter owes much to what I learned from Ricardo many years ago about the Columbia School perspective on language and linguistic analysis. So, I am delighted at the opportunity to include this report in a Festschrift in his honor. I was particularly struck by his perspicacious observation that in many current treatments of linguistic phenomena, traditional grammatical categories were “promoted to the status of explanatory constructs”

(Otheguy, 2002: p. 374). He lamented the trend whereby claims about the way language is used somehow morph into the *data* of language use, regardless of their fit with actual linguistic observations of that data, while the many cases where claims and usage fail to coincide are handled as exceptions. The treatment of the dative in one grammar (Smyth, 1920), as elucidated by Huffman (2001), exemplifies:

(...) along with the statement of the dative as case of the indirect object, we find a list of no fewer than ninety-nine uses of the dative which are *not* the indirect object. This includes an entire page of instances of the dative as *direct object* (§§ 1460–1466), the well-known phenomenon of “case government.” The list is heterogeneous, open-ended, and includes many verbs that take either the accusative or the dative, seemingly indiscriminately. Other “uses,” such as “dative of military accompaniment,” “dative of the possessor,” and “dative of price,” obviously reflect contextual elements other than the dative itself. (Huffman, 2001: p. 39)

Too often this results in the “construction of elaborate intellectual edifices upon invalid initial assumptions” (Huffman, 2001: p. 45).

My first serious personal encounter with these issues originated in early attempts to describe the use of the French subjunctive (Poplack, 1990, 1992). Years of high school and college French, coupled with a lengthy sojourn in Paris including a stint at the Sorbonne, suggested that with a modicum of effort, it should be possible to figure out what was going on. But when I began extracting tokens from a large corpus of spontaneous French speech, I found that some subjunctives were missing in contexts prescribed to take them, as in the example reproduced in (1), while others turned up in contexts where they appeared unwarranted (2). And when I asked the speakers for clarification, most responded that they couldn’t provide any because they themselves didn’t use the subjunctive. (They turned out to be wrong about not using it, but right about not knowing under what conditions. But I didn’t know that then.) So, I decided to consult a grammar. I learned that the subjunctive was only licensed under certain governors, but some of those that were attested in my corpus, like *c’est cool que* in (3), did not figure among them.

- (1) Fallait tu *mets*_[IND] un chapeau pour aller à l’église. (20C.064.2119)¹
 ‘You had to put on a hat to go to church.’

1. Codes in parentheses refer to corpus (20C = *Corpus du français parlé à Ottawa-Hull* [Poplack, 1989], 21C = *Français en contexte, milieux scolaire et social* [Poplack & Bourdages, 2005; Poplack, 2015]), speaker, and line number. Where data from more than one individual appear in an example, each is identified by a speaker number in brackets. The text of this and ensuing spoken-language examples reproduces verbatim the audio recordings constituting the corpus in question (as detailed in Poplack [1989]). The data displayed in Table 3 and Figures 5, 7, 10, and 15 derive from these corpora and a third corpus of 19th century speech (19C = *Récits du français québécois d’autrefois* [Poplack & St-Amand, 2007]).

- (2) Tu vas devenir gros après que tu *aies*_[SUBJ] pris ça. (21C.168.959)
 ‘You’re going to get fat after you’ve taken that.’
- (3) D’une façon c’est cool qu’on *soit*_[SUBJ] seize. (21C.109.356)
 ‘In a way, it’s cool that there are sixteen of us.’

I also learned that the subjunctive was endowed with a very complex semantics, although its precise nature remained somewhat murky. What did emerge clearly was that absent the subjunctive *form*, the utterance would convey a different meaning. But it was difficult to reconcile this notion with the many utterances in the corpus where speakers alternated among various tenses in the *same* context, as illustrated in (4), and as far as I could tell, these appeared to be saying the same thing rather than making distinct semantic contributions.

- (4) a. J’aimerais ça qu’ils la *fassent*_[SUBJ] petite comme ça. (20C.019.959)
 ‘I’d like them to make it small like that.’
- b. J’aimerais ça qu’ils *savent*_[IND], toutes les jeunes. (20C.041.2316)
 ‘I’d like them to know, all the kids.’
- c. J’aimerais que ça *serait*_[COND] cinq cennes. (20C.041.785)
 ‘I’d like it to be five cents.’

This state of affairs is of course reminiscent of the classic cases of inherent variability that are the bread and butter of variationist sociolinguists. Here different variant forms may alternate in a specific context (in variationist terms, the *variable context*) with no change in referential meaning. Does such an account fit with the usage facts of the French subjunctive? Or, as per standard assumptions, are speakers rather alternating among different forms with the goal of conveying different meanings? In the latter case, the alternating forms could not be construed as variants of a linguistic variable, for which semantic or functional equivalence is a *sine qua non*. The answer to this question rests on the feasibility of discerning what (if anything) the subjunctive “means,” ascertaining whether that meaning was in fact conveyed in any particular instantiation, and delimiting the contexts in which subjunctive is available to express it (in variationist terms, *circumscribing* the variable context). Since speakers themselves proved unable to furnish this information, I consulted another grammar, which, unexpectedly, prescribed something different from the first. And further consultation, rather than resolving the matter, only compounded the confusion. This state of affairs was the catalyst for the comparative study of the origins and treatment of morphosyntactic variability in the prescriptive and descriptive linguistic traditions described in ensuing sections. The results provide clear empirical confirmation of the Columbia School credo that traditional grammatical categories cannot simply be equated with the data of language use; indeed, just as its disciples cautioned, unreflecting appeal to them often obscures the way

language is actually used. I argue that they also yield important insights into the way variability is handled in the contemporary canon, by both mainstream linguists and sociolinguists.

2. The *Recueil historique de grammaires du français* (RHGF)

At the core of this project is the *Recueil historique de grammaires du français* (RHGF) (Poplack, Jarmasz, Dion, & Rosen, 2015), a corpus of prescriptive grammars of French published between 1530 and 1998. Its aim is to furnish a diachronic window on the evolution of ratified usage, i.e., “standard” language, or the “norm.” The grammars are distributed across five periods we identified as pertinent for linguistic analysis (Table 1). Here we will be particularly concerned with Period V (1950–1999), an era that we may associate with the flowering of modern linguistics. As we will see, it represents an important bellwether of the treatment of linguistic variability.

Table 1. Distribution of grammars constituting the RHGF

Period		% corpus	N grammars
I	1500–1699	17	10
II	1700–1799	23	14
III	1800–1899	45	28
IV	1900–1949	18	11
V	1950–1999	60	37
Total		100	163

To be sure, the goal of the normative enterprise is to “fix” the language; in other words, to render it *invariant*, and variationists like myself focus on variability. Fortuitously, however, the areas most frequently targeted by grammarians are precisely those where speakers have a *choice* between different ways of expressing the same thing, i.e., those hosting what variationists recognize as inherent variability. A key discovery of the work reported here is that although grammarians rarely if ever explicitly acknowledged such variability, they have worked long and hard to eradicate it. While their efforts have not had a discernible effect on the evolution of vernacular speech, I submit that they have shaped the mainstream linguistic response to variation, and specifically to the variationist paradigm more generally, to this day.

2.1 The diachronic underpinnings of synchronic variability

What kind of evidence would be relevant to such a claim? Starting from a massive corpus of contemporary everyday speech (the *Ottawa-Hull French Corpus* [Poplack, 1989]), our team identified several cases of morphosyntactic variability, often salient and stigmatized in the community, and systematically mined the RHGF grammars for any mentions that could shed light on how they evolved to ascertain their current status. The type of mentions we targeted *attest* to the variability, as in Bouhours' (1675) observation that people have a lot of trouble deciding whether to use auxiliary *avoir* or *être* in compound tenses (5). They also allow us to approximately *date* it: the citation in (5) reveals that auxiliary alternation existed prior to 1675, when the grammar was published. Other mentions help us to infer any *social meaning* that might have been ascribed to the variants, as in Brunot's (1965) qualification of the conditional in *si*-clauses as "common" (6). Most revealing are those that allow us to deduce *linguistic conditioning* of the variability, whether *contextual*, as in (7), where the futurate present variant is admitted only in temporally disambiguated contexts, or *semantic* (8), as when doubt and desire are invoked as meanings of the subjunctive.

- (5) *Il a passé, il est passé.* J' Ay veü des gens bien en peine de sçavoir lequel il faut dire. (Bouhours, 1675: p. 384)²
 'He passed [conjugated with both auxiliaries *avoir* and *être*]. I have seen people having a lot of trouble deciding which one to say.'
- (6) Le conditionnel [après *si*] ne se rencontre qu'en langue populaire. (Brunot, 1965: p. 890)
 'The conditional [after *si*] is only found in common speech.'
- (7) Le présent (...) se met au lieu du futur de l'indicatif; mais alors il est toujours accompagné de quelque nom ou adverbe de temps qui marque le futur. (Vallart, 1744: p. 237)
 'The present (...) is used instead of the indicative future, but then it is always accompanied by some noun or temporal adverb marking the future.'
- (8) On met le verbe au subjonctif, quand par ce verbe on veut marquer une chose qui tient du doute ou du souhait, sans affirmer absolument qu'elle est, étoit, a été, sera, seroit, ou auroit été. (Wailly, 1768: p. 277)
 'We put the verb in the subjunctive when by that verb we want to mark a thing that is in doubt or wished for, without absolutely asserting that it is, was, has been, will be, would be or would have been.'

2. Examples reproduced from the RHGF are faithful to the original orthography. Typographical conventions follow those of the publisher.

These are the data from which we assess quantitatively four key characteristics of a (candidate) variable: its overall *salience* to grammarians, as expressed by the proportion of grammars mentioning it, the *type of treatment* accorded its variant expressions, the (cross-temporal and cross-grammar) *consistency* of that treatment, and its *persistence* over time. In keeping with the *Principle of Accountability* (Labov, 1972), we consider not only mentions of each of the competing variants constituting the variable, but also the grammars that fail to acknowledge them. In this way, we can detect trends, as opposed to isolated examples, which in turn enable us to trace the trajectory by which synchronic developments in the grammatical sector in question came about. Another major goal of the project is to compare this normative activity with *praxis* – actual speaker behavior (e.g., Elsig & Poplack, 2006, 2009; Poplack & Dion, 2009; Poplack, Lealess, & Dion, 2013; Poplack et al., 2015; Willis, 2000).

3. Findings

Such is the generalized faith in the infallibility of grammars that when usage does not conform to what (we think) they prescribe, the discrepancy tends to be attributed to linguistic change. In this connection, a first surprising finding to emerge from our meta-analysis of the RHGF is that virtually all of the variation examined, far from constituting recent alterations, had been attested since the earliest times. Another is that grammarians have always been aware of it. This could be inferred from three lines of evidence.

The first comes from a measure of the *persistence* of prescriptive dictates. Grammars display great volatility in this regard. A prime example comes from the treatment of elements prescribed to govern a subjunctive in an embedded clause: no fewer than 785 have been cited over the duration! Moreover, when broken down by period (Figure 1), it is plain that the number of governors prescribed per grammar has risen steadily over the five centuries examined, reaching a (bewildering) zenith of 131 in the Modern Linguistics Period (V).

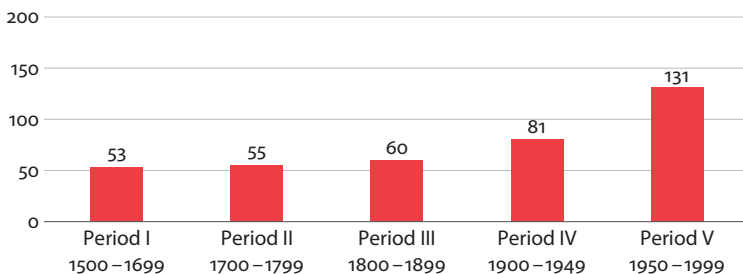


Figure 1. Maximum number of subjunctive governors prescribed per period

Remarkably few of these prescriptions have persisted over time, however (Figure 2). Instead, regardless of whether they involved verbal governors, non-verbal governors, or semantic classes of governors, well over half (56%–62%) of all such injunctions were prescribed in one period only, never to recur.

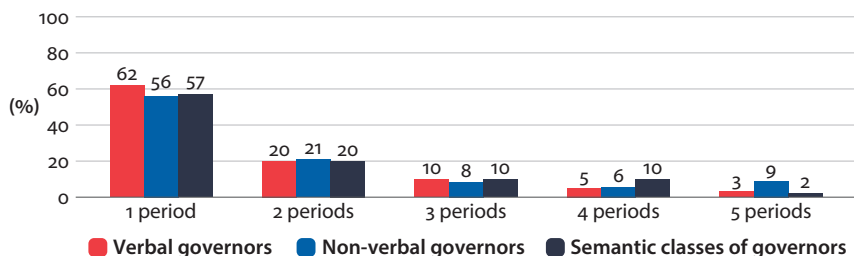


Figure 2. Persistence of prescriptions involving subjunctive governors

This leads to the next line of evidence: the pervasive lack of consistency over which meaning, function, or context of use should be associated with which variant, both across and within grammars. Readings and contexts of use tend to be presented contrastively, implying that they are isomorphic with forms; however, systematic comparison reveals that the same ones are often assigned to different forms while a single form may be assigned contradictory functions (Poplack & Dion, 2009). This is nicely illustrated by the treatment of the variable expression of proximate future temporal reference, via synthetic (SF; (9a)), periphrastic (PF; (9b)), and futurate present (P; (9c)) variants.

- (9) a. Mais le français c'est sûr ça *arrivera*_[SF] pas demain là. (21C.306.405)
 'But French, it *won't* happen tomorrow for sure.'
- b. Là il y en a une des blessées qui *va revenir*_[PF] là comme demain. (21C.109.295)
 'One of the wounded is *going to return* like tomorrow.'
- c. Fait que là je *commence*_[P] demain. (21C.150.16)
 'So I *start* tomorrow.'

Table 2 depicts some of these discrepancies. To name but a few, the synthetic variant is said by grammarians to denote certainty, but also doubt; definiteness and indefiniteness; fear, but also hope. And each of the three future variants has been variously prescribed to express proximate as well as remote futures.

Finally, which variables are *salient* to grammarians and when they become so also appears completely arbitrary. Some garner much attention, others less, with no detectable motivation, and this also varies with time. Figure 3 shows that the two apparently least conspicuous variables in the aggregate, the expression of the

Table 2. Readings and contexts of use ascribed to each variant by RHGF grammarians (adapted from Table 6 of Poplack & Dion, 2009)

Synthetic future	Periphrastic future	Futurate present
Proximate/immediate	Proximate/immediate	Proximate/immediate
Distal	Distal	Distal
Certain/probable	Certain/probable	Certain/probable
Intended/resolved/guaranteed	Intended/resolved/guaranteed	Intended/resolved/guaranteed
Progressive	Progressive	
Definite	Definite	
Incipient	Incipient	
Neutral	Imminent	Imminent
Non-progressive	Linked to present	Linked to present
Doubtful/uncertain	Inevitable	Inevitable
Distinct from present	Assured	Assured
Indefinite	In speech	In speech
Not conditional	Informal style	Informal style
Obligation		
Unfinished action	Reinforced future	Impatience
Feared	True	With temporal modification
Desired	In writing	Vivid style
Hope/possibility/hypothesis	With certain verbs	Consequence of preceding
With negation	In subordinate clauses	discourse
With adverbial modification	Without temporal modification	

future and tense selection in protases of hypothetical *si*-complexes, illustrated in (10), become increasingly salient in the 20th century (Periods IV and V). This is illustrated in Figure 4.

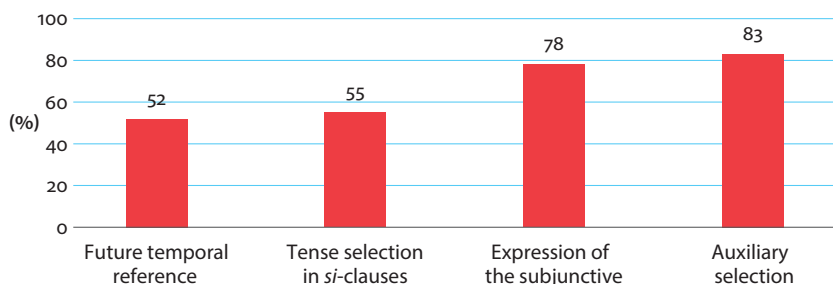


Figure 3. Salience (as measured by pertinent mentions) to RHGF grammarians of four morphosyntactic variables

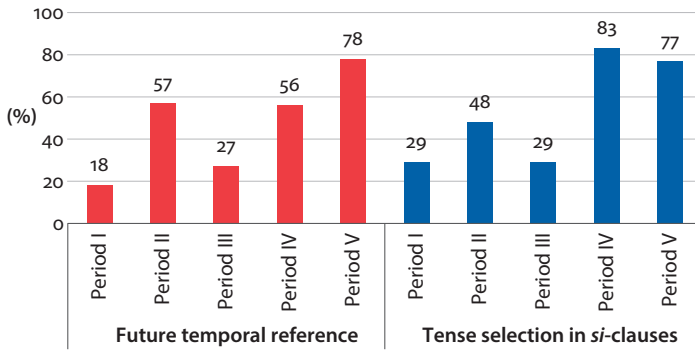


Figure 4. Salience to RHGF grammarians of future temporal reference and tense selection in protases of hypothetical *si*-complexes over time

- (10) a. Peut-être que si je les *relirais*_[COND] là, ça serait mieux. (21C.051.86)
 ‘Maybe if I *would reread* them, it would be better.’
- b. Si on *voulait*_[IMP] s’envoyer une avion de papier à l’autre classe on pourrait. (21C.051.118)
 ‘If we *wanted* to send a paper airplane to the other class, we could.’

Analysis of the spontaneous speech of this period shows that these two variables happen to be the sites of vigorous change in progress (Figure 5), with the colloquial periphrastic and the stigmatized conditional variants making huge gains at the expense of their ratified counterparts.

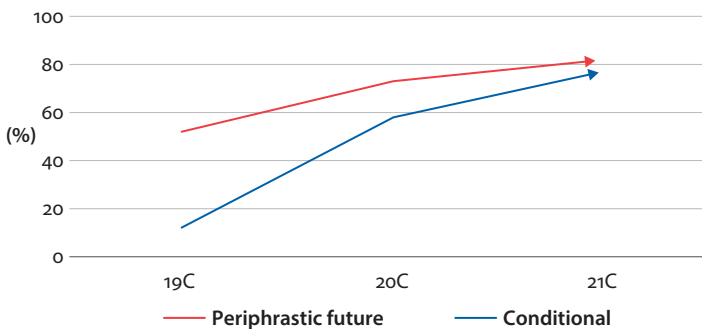


Figure 5. Trajectory of variants of salient variables in *usage* over time

It is, of course, incumbent upon grammarians to notice ongoing changes. Nonetheless, they barely register the alternation between 3rd p. sg. indefinite pronoun *on* and 1st p. pl. *nous* (11) until the most recent periods either (Figure 6), although *on* was already virtually categorical in this context by the 19th century (Blondeau, 2007; Figure 7).

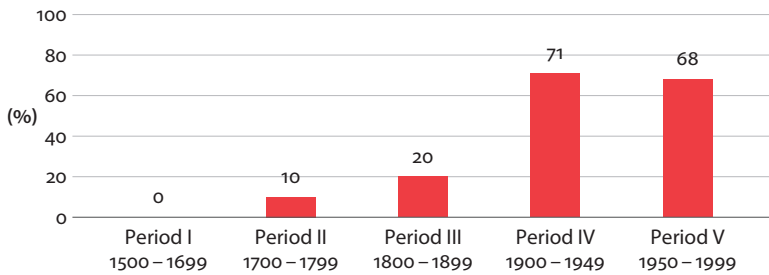


Figure 6. Acknowledgment of *nous/on* variability by RHGF grammarians (subsample) over time

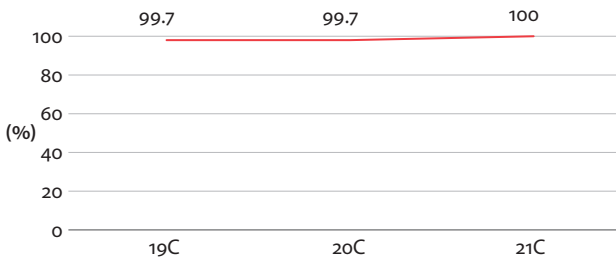


Figure 7. Overall distribution of *on* in *usage* over time

- (11) [036] *On* était, comme on dit, craintifs. [1] Ouais. [036] Sur certaines choses, *nous* étions craintiques– craintifs. (20C.036.545)
 ‘[036] We were, as they say, fearful. [1] Yeah. [036] About certain things, we were fearful– fearful.’

The volatility, inconsistency, and arbitrariness of grammatical injunctions are all signs that variability has long been widespread. We turn next to the question of how grammarians have handled it.

3.1 The normative treatment of variability

Prescriptive tradition has had a long history of targeting variability, largely in its ongoing quest to eradicate it. How is this achieved? Our meta-analysis of the RHGF turned up a variety of avenues. Some grammarians simply ignore it. This is the strategy Girard (1747/1982) adopts in (12), by citing only the synthetic, but not the other variants of future temporal reference.

- (12) Lorsqu'on représente l'événement comme devant positivement arriver dans la suite, cela fait le temps avenir; qu'on nomme FUTUR, tel qu'on le voit dans cette phrase: *je me donnerai de la peine; mais j'en viendrai à bout.*

(Girard, 1747/1982: p. 20)

'When the event is presented as definitively taking place at a time to come, that calls for the future tense, which we call [synthetic] future, as illustrated in this sentence: *I will work hard but I will prevail.*'

But many more acknowledge variability, or at least the variant *forms* participating in it. They fail to recognize them as variant expressions of the same referential meaning or function, however. Instead, they seek to establish *symmetry*: that idyllic grammatical state where every form has its dedicated function and vice versa. If forms are found to be competing, then either one of them must be ratified and the other(s) rejected, or each has to be assigned an exclusive locus of occurrence. In this way, "vacuous" variability or "free" variation can be factored out. Poplack et al. (2015) found that grammarians employ three main strategies to attain this goal: associating each variant with a different type of speaker (or register), assigning each variant a specific linguistic context, and/or matching each variant with a dedicated meaning. The following sections explore how they accomplish this.

3.1.1 *Prescriptive strategies for factoring out variability*

3.1.1.1 *Social and stylistic*

The contemporary tendency of both professional linguists and naïve speakers to ascribe different variants to different sectors of society and to imbue them with the characteristics they associate with their (perceived) users has a deep history in the prescriptive tradition. A particularly illustrative example involves the variable choice of the conditional instead of the standard imperfect in protases of hypothetical *si*-complexes. Early on, the conditional was considered to "go well" with *si* (13), but subsequent grammarians have steadfastly proscribed this form in this context (14).

- (13) *Quand. Lorsque. Si. Si tost que. Si est-ce-que.* Vont bien avec le second imparfait, & second plusqueparfait & futur.

(Maupas, 1632: p. 186)

'*Si* [and other conjunctions]. Go well with the conditional and the past conditional and the [synthetic] future.'

- (14) a. *Mais: Si vous m'auriez averti; Si je n'aurais su; Si j'aurais été* &c. seroient des solécismes.

(Mauvillon, 1754: p. 527)

'But: *if you would have warned me, if I would not have known, if I would have been*, would be grammatical errors.'

- b. C'est une faute d'employer le conditionnel après la conjonction *si*.
(Carpentier, 1860: p. 81)
'It is a mistake to use the conditional after the conjunction *si*.'
- c. La grammaire officielle condamne maintenant l'emploi du conditionnel dans ces cas et demande l'imparfait.
(Nyrop, 1935: p. 368)
'Official grammar currently condemns the use of the conditional in these cases and requires the imperfect.'
- d. Attention! il ne faut pas dire: 'Si j'aurais ..., si je pourrais ...,' mais 'Si j'avais ..., si je pouvais ...'
(Bergeron, 1972: p. 80)
'Warning! Do not say: 'If I would have ..., if I would be able to ...,' but 'If I had ..., if I could'
- e. Dans les propositions de condition introduites par *si*, le bon usage n'admet pas le conditionnel.
(Grevisse, 1998: p. 353)
'In conditional clauses headed by *si*, good usage does not tolerate the conditional.'

Injunctions against the conditional again proliferate after 1950 (Figure 8), but no attempt has ever been made to “explain” the prohibition (or the variation) on linguistic grounds. Here, the appeal is only to the properties of the *speaker*. The proscribed variant is not the province of educated adults, but only of foreigners, the uneducated, the masses, and children (15).

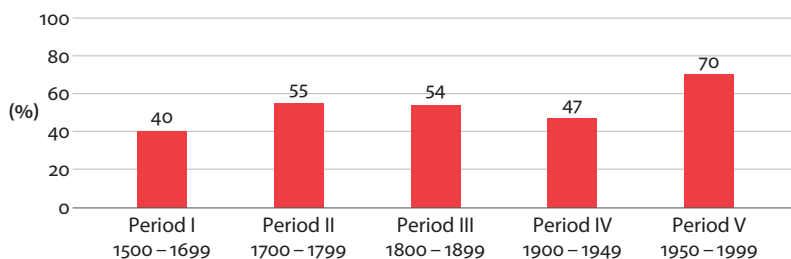


Figure 8. Proportion of RHGF grammars specifically excluding the conditional from protases of hypothetical *si*-complexes over time

- (15) a. Mais seuls les étrangers ignorant le génie de notre langue peuvent préférer *si vous viendriez*.
(Dauzat, 1943: p. 178)
'But only foreigners unfamiliar with the spirit of our language could proffer *if you would come*.'
- b. D'ailleurs le peuple ne se prive jamais de dire *si j'aurais su* ou *si j'aurais eu*.
(Martinon, 1950: p. 362, fn. 1)
'In fact, the masses never miss a chance to say, *if I would have known* or *if I would have had*.'

- c. Le fameux: *Si j'aurais su, j'aurais pas venu*, du langage enfantin, est d'une parfaite logique. (Charaudeau, 1992: p. 474)
'The famous: If I would have known, I wouldn't have come, of child language, is perfectly logical.'
- d. Dans l'usage familier (...), le conditionnel s'emploie aussi dans la proposition introduite par *si*. (Riegel, Pellat, & Rioul, 1998: p. 318)
'In informal usage (...), the conditional is also used in clauses introduced by *si*.'

Figure 9 shows how these appreciations have evolved over time. Note how long the conditional was qualified as an “error,” made largely by foreigners. Not until the 20th century do grammarians begin to acknowledge that it is also used by native francophones, if only those of the lowest classes. The “erroneous” attribution continues right into the Modern Linguistics Period, where it now rivals the novel “childish” association. This finding achieves full significance when we consider that, in the community, the conditional has risen – vertiginously! – to become the current majority variant by far (Figure 10), and its users include native francophones of all stripes, from the “ignorant” to the intellectual, child to adult.

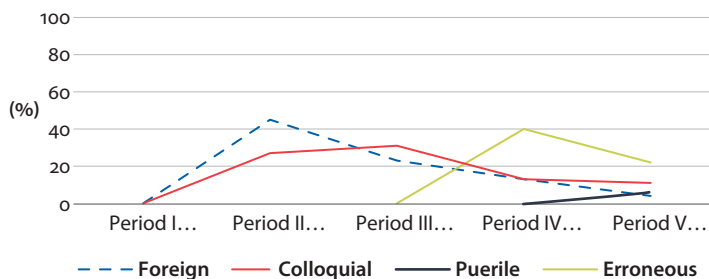


Figure 9. Social connotations ascribed by RHGF grammarians to the conditional in protases of hypothetical *si*-complexes over time

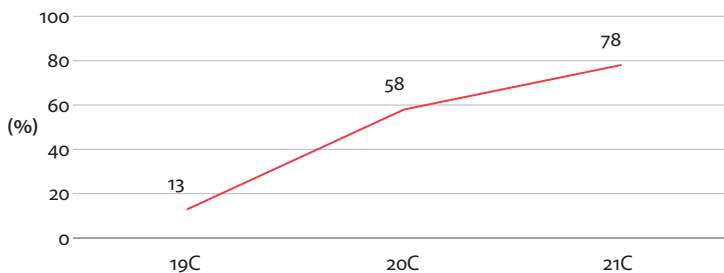


Figure 10. Rate of conditional selection in protases of hypothetical *si*-complexes in usage over time

3.1.1.2 Contextual conditioning

Another strategy favored by grammarians for factoring out variability is to restrict variants to particular linguistic contexts. This can be illustrated by the fate of the future temporal reference variants, all of which have been attested since the earliest grammars. Here again, while cheerfully acknowledging the various *forms*, grammarians fail to identify them as alternate expressions of the same referential meaning. Instead, they strive to match them with dedicated linguistic conditions or contexts. As detailed in Poplack and Dion (2009), large numbers of these were invoked. The most recurrent involves proximity of the future predication, and the claim that different variants are reserved for differing degrees of temporal distance. But as shown in Table 2, there is little consensus as to which variant expresses which; all of them have been associated with both proximate and distal futures. Still, once the associations between context and variant are tallied, the one linking the periphrastic variant with proximity does turn out to enjoy the greatest amount of inter-grammar agreement (59%); this is also the only association that persists over all five periods (Poplack & Dion, 2009). A variety of other elements relating to discourse context have also been said to influence variant selection, albeit at far lower agreement levels. These are depicted in Figure 11.

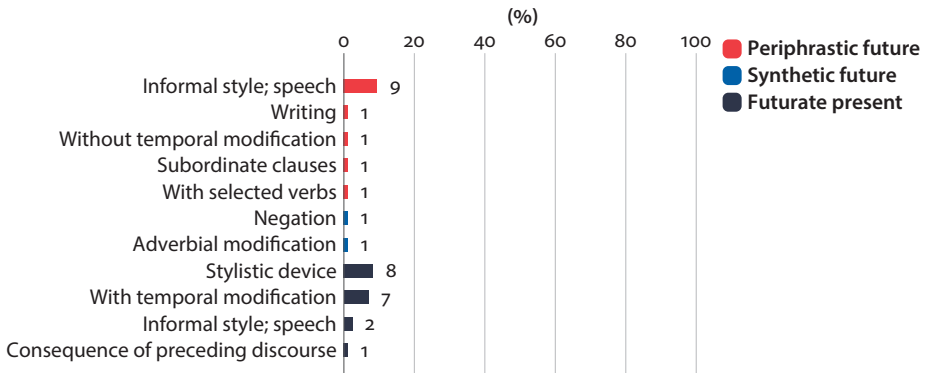


Figure 11. Inter-grammar agreement on contextual conditioning of future temporal reference variants

The preponderance of contexts associated with the periphrastic and present variants relative to the synthetic future suggests that the latter is the default marker of futurity, while the others are reserved for more specialized purposes. When we examine the way the variants were actually *used* in the speech of the same periods, however, we find almost no common ground between these injunctions and community norms. As can be seen in the results of the variable rule analysis (Table 3), which

operationalizes temporal distance and other contexts claimed to account for the variability, proximity in the future has no effect on variant choice in contemporary Laurentian French, and its effect was minimal (indeed, inferior to that of any other context) in the 19th century. Ironically, however, the overwhelming predictor of variant choice in speech *is* in fact contextual. The synthetic variant, far from functioning as the default, is instead almost categorically restricted to negative contexts, as illustrated in (9a). Reference to this effect is all but absent in the five centuries of grammatical tradition studied.

Table 3. Multivariate analysis of the contribution of linguistic context to the selection of the synthetic (over the periphrastic) variant in *usage* over time (reproduced from Table 2, Poplack et al., 2015)

	19th century			20th century		
Corrected mean	0.354			0.155		
Overall rate	39%			22%		
Total N	(1663/4293)			(725/3357)		
	prob.	%	N	prob.	%	N
POLARITY						
Negative	.99	99	451/457	.99	97	456/471
Affirmative	.37	32	1211/3834	.31	9	269/2886
Range	62			68		
SPEECH STYLE						
Formal	.64	51	109/212	.81	42	22/53
Informal	.49	38	1554/4081	.49	22	695/3203
Range	15			32		
ADVERBIAL SPECIFICATION						
Presence	.65	47	238/502	.59	13	47/358
Absence	.48	37	1381/3739	.49	21	592/2877
Range	17			10		
TEMPORAL DISTANCE						
Distal	.60	41	193/476	.48	16	79/495
Proximal	.50	30	342/1153	.51	18	119/679
Range	10			3		

3.1.1.3 Semantic assignments

Of all of the strategies for factoring out variability, however, the grammarian's ace-in-the-hole resides in the *meanings* he attributes to competing variants. Nowhere is this more evident than in the treatment of the subjunctive. On the many occasions when subjunctives fail to appear in “subjunctive-selecting” contexts, or

show up in presumed indicative contexts, the explanations proffered are invariably semantic. In keeping with this program, a remarkable total of 76 distinct readings have been assigned to the subjunctive variant over the duration. Predictably, they include the prototypical meanings conventionally attributed to the (Latin) subjunctive, such as doubt, desire, and volition. But these are far exceeded by much more elusive concepts like “psychic energy” (LeBidois & LeBidois, 1971: p. 501), “doubtful modesty” (Fischer & Hacquard, 1959: p. 328), “sentiment” (Laurence, 1957: p. 140), and “ideas” (Bruneau & Heulluy, 1950: p. 145; Sternon, 1954: p. 231), among many others. The affect, emotions, and even the soul of the speaker are also frequently invoked, as illustrated in (16). The fact that such readings are generally impossible to decode, let alone operationalize and test, conspire in making them so difficult to challenge.

- (16) a. Le subjonctif exprime les dispositions de l’âme relativement à des faits.
(Crouzet, Berthet, & Galliot, 1912, Section 400)
‘The subjunctive expresses the dispositions of the soul relative to facts.’
- b. Toutes les fois que la parole est comme chargée de sentiment, dans toutes les phrases qui supposent une tension et un élan de l’âme, le subjonctif a ses raisons suffisantes en soi-même. (LeBidois & LeBidois, 1971: p. 510)
‘Whenever speech is charged with feeling, in all sentences that imply a tension and an impulse of the soul, the subjunctive is justified.’

Here too, only a very small minority of meanings has persisted across the five periods investigated; indeed, nearly half of them are ephemeral (Figure 12). Even the most persistent are treated inconsistently. For instance, only 11 of the 76 readings documented achieved so much as a 10% inter-grammar agreement rate (Figure 13). The frontrunner, *doubt*, is cited by only a third of the RHGF grammars.

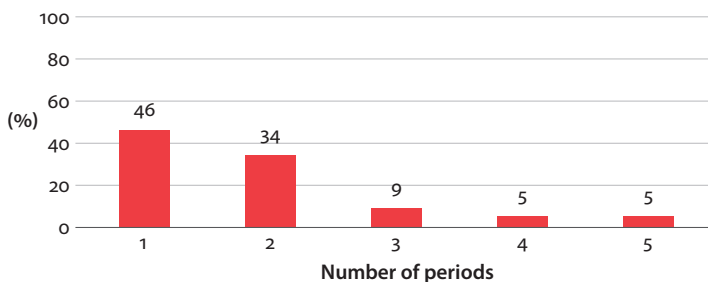


Figure 12. Persistence of semantic readings assigned to the subjunctive

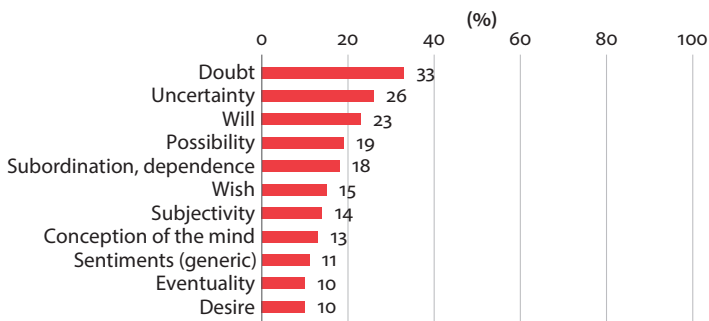


Figure 13. Semantic readings ascribed to the subjunctive achieving inter-grammar agreement of 10% or better

Interestingly, many of the meanings ascribed to the subjunctive coincide with those ascribed to the future variants (17). And as we saw in Figure 11, they enjoy just as little consensus.

- (17) a. [Le futur périphrastique] présente la réalisation du procès comme plus assurée et sa réalité comme plus certaine que le futur, qui laisse subsister un doute. (Riegel et al., 1998: p. 315)
 ‘[The periphrastic future] presents the reality and the realization of the process as more certain than the synthetic future, which implies some doubt.’
- b. Le futur simple (...) est souvent “chargé” de notions diverses (idées de possibilité, d’obligation, etc.) et de sentiments (désir, crainte, etc.) (Brunot & Bruneau, 1969: p. 337)
 ‘The synthetic future (...) is often “loaded” with different notions (ideas of possibility, obligation, etc.) and feelings (desire, fear, etc.)’

It is instructive to compare these prescriptive injunctions with the way speakers actually use the subjunctive. I noted above that local francophones had reported that they *didn’t* use it. We have since learned that they in fact use it more in the aggregate than in other Romance languages recently studied in this connection (Poplack et al., 2018). The question is: *what* do they use it for? Since so many of the 76 readings attributed to this grammatical category reside in speaker intent or attitude, few could be operationalized and tested. Still, Poplack et al. (2013) did try to capture some of them through intervening variables. However, analysis of nearly 5000 contexts in which the subjunctive *could have been* selected in spontaneous speech showed that any apparent semantic effect was actually an epiphenomenon of the overriding effect of another factor: lexical identity of the governor.

Since the 19th century (at least), just four governors (*falloir* ‘be necessary,’ *vouloir* ‘want,’ *aimer* ‘love,’ and *pour que* ‘so that’) have together represented up to

three-quarters of all the governors occurring in spontaneous speech (Figure 14). Rates of subjunctive selection with these governors are high and rising (Figure 15), and when they are paired with a small cohort of four frequent and irregular *embedded* verbs (*aller* ‘go,’ *avoir* ‘have,’ *être* ‘be,’ and *faire* ‘make’), those numbers skyrocket. This is what accounts for the vast majority of all subjunctive morphology, not the way the speaker envisions or emotes about the predication.

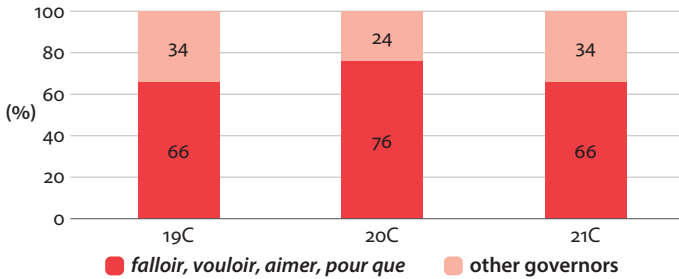


Figure 14. Distribution of subjunctive governors in *usage* over time

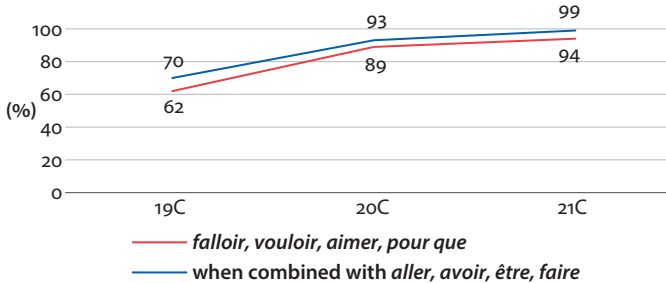


Figure 15. Rate of subjunctive selection in frequent governor and embedded verb pairs in *usage* over time

These quantitative patterns are invisible to any but systematic quantitative analysis. It thus comes as no surprise that they too have failed to be acknowledged in the normative tradition.

Summarizing, the foregoing results raise the inevitable question of why so many different associations have been invoked for these and other variants we have studied. Why have so few recurred over time, and why have they been marked by such inconsistency? The very volatility and idiosyncrasy of these normative treatments – not only longitudinally, but within the same time frame, and even the same grammar! – militate against the idea that the disparities result from actual linguistic change. Rather, I have suggested that they derive from the goal of eradicating “vacuous” variability and establishing *form-function symmetry*. For

some variables, the preferred method to achieve this is through social attributions or contextual associations. For most, it is by imbuing each variant with a meaning, whatever it may be, so long as it is distinct from that of its counterpart(s) in the grammatical sector in question. In all of these cases, the implication is that the associations between variants and domains of use are *privative*: the speakers who use one variant are not the same ones who use the other, or the context felicitous for one variant automatically excludes its competitor, etc. All of this suggests that the aim of the normative enterprise is not to prescribe the *correct* use, but to bring order to the perceived chaos of *variable* use. If the former were known, there would be consensus among grammarians with respect to prescribing it. Instead, we have seen that there is at best scant agreement on which meaning or function to assign to which variant. This explains why grammarians end up ascribing the same ones to different variants, and contradictory ones to a single variant. In the few cases where consensus can even be invoked, it is only at a very low level. We interpret this to mean that (a) the competing variants *can* express the same meanings, and (b) the meanings are *not* entirely coterminous with the variants. Of course, this is exactly the kind of form-function *asymmetry* that is characteristic of inherent variability.

Importantly, however, rather than accept the existence of variability, grammarians appear to have multiplied their efforts to stamp it out. This movement gains momentum in the 20th century, especially during the period we have associated with the flowering of modern linguistics. The proportion of grammars emitting prescriptions in general nearly doubles during this time (e.g., Figure 4). The meanings and functions assigned to variants mount dramatically. As illustrated in Figure 16, nearly two-thirds of those associated with future temporal reference, for example, first appeared in this period. The number of subjunctive governors prescribed per grammar increased by 60% (Figure 1), and most of the idiosyncratic (i.e., novel) meanings and governors associated with it were also introduced at this time, as can be seen in Figures 17 and 18. Condemnation of the conditional jumps by 49% (Figure 8). These examples could be multiplied.

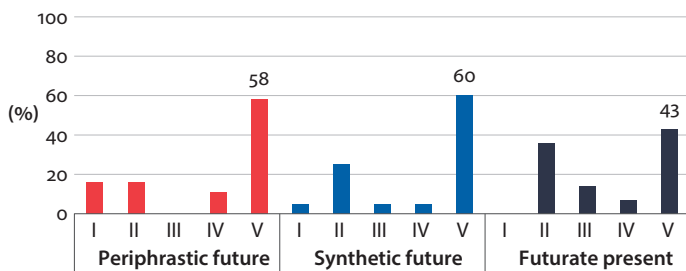


Figure 16. Period in which meaning was first invoked: future temporal reference

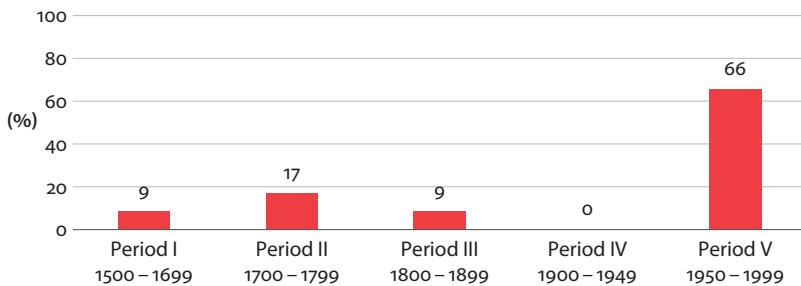


Figure 17. Idiosyncratic assignment of subjunctive meanings by RHGF grammarians over time

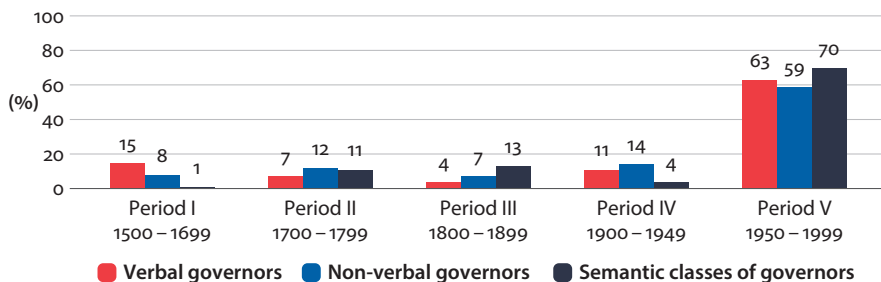


Figure 18. Idiosyncratic assignment of subjunctive governors by RHGF grammarians over time

Paradoxically, these spurts of normative activity seem to increase as the prescribed variants are *decreasing* in rate or productivity. Herculean efforts to re-establish form-function symmetry are thus a hallmark of the 20th century, especially the last half. This suggests that variability has become more salient and less palatable in the Modern Linguistics Period.

4. Contemporary treatment of variability

From the few comparisons with usage presented here (see Elsig & Poplack, 2006, 2009; Lemay, 2009; Leroux, 2007; Miller, 2007; Miller & Dion, 2009; Willis, 2000; for others), it is clear that prescriptive dictates have not exerted much effect on the evolution of spontaneous speech. In the grammatical sectors we have studied, non-standard variants continue to prevail, and where change is attested, it is rarely in the direction of the prescribed variant. Nonetheless, they have significantly shaped the thinking of other grammarians, as well as that of linguists. In fact, the

preoccupations of the normative tradition, though often roundly rejected and even ridiculed, have filtered down almost unaltered to many contemporary analyses. Given the advances of modern linguistic theory in almost every other area, it is particularly remarkable that the treatment of variability has changed so little. On the contrary, many contemporary mainstream accounts are direct inheritances from prescriptive injunctions. Baunaz and Puskas' (2014: p. 242) claim that the subjunctive is "systematically associated with the subject's emotive experience" is but one notable example. But the major throwback is the enshrinement of what I refer to as the *Doctrine of Form-Function Symmetry*. It is neatly encapsulated in Bolinger's (1977) famous dictum in (18), in Goldberg's (1995) *Principle of No Synonymy* (19), and many other formulations (e.g. 20), both explicit and implicit.

- (18) The natural condition of a language is to preserve one form for one meaning and one meaning for one form. (Bolinger, 1977: p. x)
- (19) If two constructions are syntactically distinct, they must be semantically or pragmatically distinct. (Goldberg, 1995: p. 67)
- (20) Wherever there is a difference in *form* in a language, there is a difference in *meaning*. (Clark, 1987: p. 1)

Espousal of the Doctrine of Form-Function Symmetry, in conjunction with its corollary, *categoricity*, means that much contemporary mainstream syntactic and semantic literature, just like its normative counterpart, must continue to devote itself to the problem of "unwarranted" variation. This rears its head in the myriad instances where the "wrong" variant is selected to express the apparently intended meaning, or where both variants alternate to express a single meaning that should purportedly be associated with only one, or when contexts whose semantic features were predicted to link them with one variant surface with the other. All of these asymmetries and mismatches are of course symptoms of inherent variability, and as we have shown, this is what characterizes the way the variants we have studied are actually used in spoken discourse. The extent and nature of this variability have remained opaque to most formalists and many functionalists, since belief in the primacy of speaker intent, the fundamental nature of grammatical categories, and the Doctrine of Form-Function Symmetry together conspire to obscure its existence.

The formal linguistic literature is replete with alternative proposals to explain these recalcitrant cases. They all have in common the intent to eliminate vacuous variation and restore form-function symmetry. Preceding sections revealed how the prescriptive tradition appeals to three major avenues to accomplish this. The social route has long been rejected by the mainstream as outside the purview of linguistics proper. The appeal to context may not entirely succeed in ruling out two ways of saying the same thing. This explains the pre-eminence of meaning in

explaining variant choice, echoing the time-honored prescriptive tradition. Even when consensus on the precise semantic contribution of a particular variant is lacking (as continues to be the case more often than not), the conviction that each makes a distinct one is widely endorsed. Where meaning A is intended, variant A will categorically be selected and vice versa, because the semantic features “requiring” one variant must naturally exclude the other.

Among the variety of avenues to which formalists appeal to support this position is to reanalyze the semantics of the variant or the semantics of the context until the desired match is achieved (e.g., Abouda, 2002; Giannakidou, 1999; Giorgi, 2009). Another is to impose a semantic contrast on a constructed example. This is how Schlenker (2005: p. 23), for instance, “explains” the putative difference between subjunctive and indicative in a sentence like *Jean se lamente qu’il pleut*_{IND}/*pleuve*_{SUBJ} ‘John is sorry that it’s raining’ (21).

- (21) To my ear the subjunctive version is rather neutral, but the indicative requires a particular situation – one in which Jean says something, to others or himself, to the effect that he is unhappy that it is raining. (Schlenker, 2005: p. 23)

This is, of course, entirely reminiscent of the traditional normative exegeses of what literary figures meant when they did or did not use a variant, like the one proposed by Bescherelle in 1877 (p. 22):

- (22) When Mme de Sevigné says: *Il me semble que mon coeur veuille*_[SUBJ] *se fendre*, ‘it seems to me that my heart wants_[SUBJ] to break,’ she is not at all convinced of what she is asserting; it is as if she said: I am tempted to believe that my heart wants to break. This is not at all the case when Voltaire says: *Il me semble que Corneille a donné*_[IND] *des modèles de tous les genres*, ‘it seems to me that Corneille has given_[IND] models of all genres.’ Voltaire is asserting here a positive fact, about which he has no doubt, he is convinced of it, he has examined it and judged. From these observations, and even more from our citations, we believe, against the grammarians, that one should use: 1° the indicative every time one asserts (...) a fact of which one is entirely convinced; 2° the subjunctive in the opposite case. (Bescherelle, 1877: p. 651, translation ours)

But the most powerful weapon in the contemporary quest for form-function symmetry remains the appeal to the *speaker* – her intentions, convictions, emotions, desires, indeed her very soul, in the terminology of traditional grammarians. As Bolinger (1977) also apprises us:

- (23) Linguistic meaning expresses our attitudes towards the person we are speaking to, “how we feel about the reliability of our message, how we situate ourselves in the events we report.” (Bolinger, 1977: p. 4)

From here it is a short step to inferring, as did Confais (1995) with respect to future temporal reference, for example, that:

- (24) The synthetic future variant functions less as a vehicle of the speaker's *conviction* with regard to the non-verifiable content of his utterance than as a sign of his *engagement vis-à-vis* his utterance. [This variant] is used more to "interest" the interlocutor, to console, reassure, promise, give instructions, and so on.
(Confais, 1995: p. 401, translation ours)

Now, since the speaker is the ultimate arbiter of what s/he wanted to express, failing all else, the analyst can always resort to the inference that if s/he selected variant A, s/he must have wanted to express meaning A. And since no one but the speaker is privy to that information, the Doctrine of Form-Function Symmetry remains unchallenged, *even when analysts cannot agree on what the speaker meant*.

5. Discussion

Why do these disagreements arise? Where forms appear to be alternating in the same context, proponents of unique form-function relationships attempt first to pinpoint what distinguishes the forms, and then to link them to categorical co-occurrences with something else. Where this fails, as we have seen to be the case for all the variables reviewed here and many more, the alternating forms themselves are said to be the sole bearers of the proposed functional, semantic, or pragmatic distinctions. These in turn derive from those considered to be inherent in the grammatical categories to which the forms "belong." But in practice, the nature or even existence of these distinctions, in the absence of any categorical surface correlate, is strictly a matter of individual interpretation. This is especially true when variant selection is relegated to semantic, pragmatic, or psychological motivations. Invoking them licenses the analyst to explain alternation among forms as resulting from differences in the way the speaker *envisages* the event, rather than (what may appear *prima facie* to be) random occurrences.

Particularly disturbing about these developments is that for many of these variables, such interpretations have transformed themselves into (apparently widely shared) *intuitions* about variant use, and thence into the *data* of language use. These in turn serve as the basis for numbers of elegant and highly ramified theories that have little if any basis in the *facts* of language use, exactly as adumbrated by the Columbia School. This raises the epistemological question of whether a grammatical category or a linguistic form can embody a meaning when it is not used by speakers to express it.

Ironically, so pervasive is the Doctrine of Form-Function Symmetry that it is also at the heart of *sociolinguists'* rejection of syntactic variation. The controversy that raged in the 1970s over the existence of the syntactic variable (e.g., Labov, 1978; Lavandera, 1978; Romaine, 1981) revolved around the very same issues. Linguists were willing to concede that phonological variants may lack a dedicated linguistic meaning (in variationist terms, they all refer to the same state of affairs), but they insisted that variation above the level of phonology carried meaning *by definition*. A *sociolinguist* may be prepared to expand the purview of meaning to the social and stylistic, as was Lavandera (1978). But the possibility that variants of a variable could alternate with *no* change in meaning (as in Weiner and Labov's [1983] famous construal of the agentless passive, for example) continues to meet resistance. Attributing distinct roles to each variant restores the desired isomorphic relation between function and form, while implicitly rejecting the possibility of bona fide grammatical variation.

The uniquely variationist perspective on the alternation among variants beyond the phonological is of course that, even if different meanings or functions did play a role in variant selection, those distinctions need not apply every time one of the variants is used. This is because in certain contexts (specifically, in the *variable context*), such distinctions can be *neutralized*, and neutralization is the fundamental mechanism of variation and change (Sankoff, 1988).³ The evidence we have been accruing from years of systematic confrontation with the data of morphosyntactic variation in spontaneous speech fails to validate virtually all of these prescriptive and formal linguistic treatments, revealing robust variability instead. Whether or not the variability is (socio)linguistically arbitrary, it is subject to regular conditioning. This conditioning is profoundly entrenched in the *community norm*, but much of it is opaque to intuition and introspection. This is why it remains invisible to those who confine themselves to these methods of linguistic analysis. Such discrepancies in the treatments of variability, coupled with the confusion between the categories of grammar and those of speech, are responsible in large part for the growing gulf between usage, prescription, and much linguistic description.

3. Of course, such neutralization cannot simply be inferred; it can only be established on the basis of systematic confrontation – in discourse – of variant forms with putative meanings or functions.

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Letter from Ricardo Otheguy to Shana Poplack

Dear Shana

I found your paper so original and so different that it was a real pleasure to read and re-read. I had never seen a survey covering the treatment of forms through the centuries in grammars that have so consistently failed to come up with the correct analysis due, in part, to the refusal to study the forms with sufficient grounding in actual usage. And I had never seen the extensive survey followed by studies of these same forms in variation. What a great idea, so original and well executed!

I was glad to see that you found part of your inspiration in the point so often made in the Columbia School (CS) literature about the lack of validity of the traditional syntactic categories. There's more that needs to be said about this problem of the traditional categories, and more of a connection that needs to be made to what you call the Doctrine of Form-Function Symmetry; to the old and vexed question of referential equivalence; and to the matter of explanation in variation studies.

From the standpoint of CS, the traditional analyses are invalid not only because, as you have so well documented, they're not based on observations of fact and thus fail empirically at every turn, but also because the categories of analysis are not treated as testable hypotheses. Many linguists facing a new language, or a new problem in a familiar language, start off by assuming that their observations, and later their analysis, can be articulated in terms of Sentences, Subjects, Predicates, Tenses, Moods, Aspects, Subject-Verb Agreement, Null and Overt Pronouns, Direct and Indirect Objects, etc. And along with these come Definiteness, Impersonal, Reflexive, Demonstratives, Complements, and all the other categories of the *a priori* inventory that has weighed down syntactic analysis for centuries and does still today. The linguist seldom stops to think that these categories were never intended to be part of a scientific linguistics, as they in fact predate science in general and linguistics in particular.

However, more generally and beyond the issue of the *a priori* nature of the traditional syntactic categories and their empirical failure, the problem is that we have no reason to assume that the syntax of human languages is organized along the lines laid out by the Sentence and its parts. To be sure, we have to start somewhere, something has to be assumed, a handle is needed to make the initial observations.

But as William Diver often stated in his writings, all that we are entitled to assume *a priori* is that languages are being used to communicate and that, consequently, like all systems of communication, the sounds of speech (or the scratches on the page or the gestures of the deaf) must be manifestations of strings of forms with content (Diver and CS call them signals with meanings).

So what's so radical about that? Doesn't everybody know that forms have meanings? Doesn't your paper review, in fact, many efforts at meaning assignment by French grammarians? The problem is that the meanings and functions that linguists tend to attribute to the forms that they're interested in (verb endings, pronouns, prepositions, active and passive constructions, etc.) almost never make any sense. As you have so well documented, the scholar flails about, looking at the many different conceptual notions that are found in the different contexts where the form appears, never getting anywhere. Your list of the 76 meanings that French grammarians over the centuries have attributed to the subjunctive and the indicative, or the different meanings that linguists have attributed to different forms of the future in different languages, are great examples of that. Your survey lays bare this problem so relentlessly that, if it weren't such a record of human failure, it would almost be comical.

From a CS point of view, the reason for this failure is that the notions that the scholar has been regarding as the meanings of these forms *are not meanings at all*. In other words, they are not concepts residing in the finite and arbitrary grammar of the language. To the contrary, they're part of the infinite and ever varying set of notions found in what CS calls *messages*. These notions that the grammarians you studied have been working and reworking to try to get a handle on the forms of French are simply descriptions of aspects of *the content of the communications*, not descriptions of *the content of the forms* of French. The grammarians see the forms and do grasp, more or less, what's being communicated, but have developed no techniques to isolate the notional contribution of the form from the contribution of everything else occurring around it and, more important, have developed no understanding of what a true linguistic meaning really is.

The grammarians in your survey fail because they've made the oldest mistake in the book: they've confused speech with language; they've taken the products of the system for the system itself; have confused language use with language structure (or, in the old phrase, they've confused the car's engine with the car's trips to the supermarket, to the school, to the office, etc.). In this case, they've looked at the evanescent, unique notional effect of deploying forms in an infinite variety of contexts and mistakenly thought that they were looking at the content of these forms. In CS terms, they've failed to make the key distinction between meaning and message.

The evident failure of what you call the Doctrine of Form-Function Symmetry stems from this confusion. Proponents of the symmetry have taken forms (words,

affixes, inflections, constructions) that are indisputable facts of language, and have tried to pair them with what are clearly not facts of language at all, namely the many and sundry message effects associated with specific contextualized moments of use where the forms appear. The Doctrine fails because it purports to be offering a match between form and meaning when all it's doing is matching form and fragments of messages. That's a recipe for guaranteed failure; there's never a form-message pairing that extends beyond the scope of a particular instance of use, and sometimes there isn't even that, because the content of a meaning often does not appear directly in the message (more on this important point later). The forms of languages do not bear a symmetrical relationship with the notional categories of messages or parts of messages. They bear a symmetrical relationship only with their meaning.

I love the sarcasm when you speak of the 'idyllic grammatical state where every form has its dedicated function and vice versa.' But as it turns out, it is true that under the proper analysis, under an analysis that distinguishes meaning from message, an analysis guided by a correct understanding of what a meaning is, an analysis, that is, that none of the French grammarians in the survey ever performed, there is symmetry after all. Forms do have dedicated particular notional content, but this content is not what is usually called a function, nor is it a meaning of the sort the scholars in your survey imagined. In other words, for the incurable romantics in CS who have developed research strategies to turn the idyll into analytical success, the forms of language do have dedicated meanings.

Now, these meanings to which the forms are dedicated are unobservable; they are theoretical entities. They are postulated by the linguist through analysis, an analysis that, usually, starts by clearing the deck of all the traditional categories and making a fresh start. Meanings, as proposed and tested by the CS linguist, constitute hypotheses intended to explain the distribution of the form. There is, then, I would say, and *pace* my friend Shana's wonderful sarcasm, a true Doctrine of Symmetry, but it is a Form-Meaning Symmetry, where meaning is understood as the stable and, at any synchronic stage, permanent property of a form, where a meaning is the constant contribution that a form makes to every message, a contribution that is sometimes direct and fairly easy to establish, but that most of the time is quite indirect and demanding of considerable analytical work; it's a contribution that is at times readily visible in the message, but that often is not. The linguist's job is to find these meanings, to formulate them explicitly, and to test them and make predictions based on them, bearing in mind all along that success requires turning a blind eye to the false guide posts provided by the tradition. To find, for example, the meaning of the forms *this* and *that* of English, one has to start by eliminating the categories of Demonstrative and Sentential Complement, and work on the very different effects the meaning of the single form *that* has in, for example, *That is my*

friend's house, and in *I know that he'll never come back to live in Manhattan again*, or that the single form *this* has in *This is my friend's house*, and in *I know this: he'll never come back to live in Manhattan again*.

This arduous task of the CS linguist is made particularly difficult because the forms in many cases already have *names* provided by the tradition. And there's a great temptation to confuse the name with the meaning. For example, faced in Spanish with forms that are named 'the Future tense' (e.g., *tendrá* 'he will have'), it is easy to make the mistake of thinking that the form actually has a meaning of FUTURE TENSE, and to set about trying to understand its distribution in those terms. One then discovers, when even minimal attention is paid to the facts of usage, that the form is involved in all sorts of messages that contradict the notion of Future, as in the familiar cases of present-time conjecture (*Hace años que no lo veo, pero ya hoy tendrá por lo menos 50 años* 'I haven't seen him in many years but today he must be already at least 50 years old'). Simple examples like this one could be multiplied for the purpose of illustration. The form named Plural in English, [-s, -z, -ez], may mean MORE THAN ONE as its name suggests, or it may not; it is certainly used for 'More than one' messages (*the bottle holds two gallons*), but it is also used for 'Less than one' messages (*the bottle holds 0.5 gallons*). So, does the Plural mean MORE THAN ONE? Maybe not, maybe it has a different meaning, one that has to be discovered by the linguist.

Seen through CS eyes, your survey is a magnificent record of the repeated mistake of looking at a form's local function or message in a set of particular contexts, and thinking, usually entangled in the net of confusion created by the traditional categories, and often guided by the form's traditional name, that one has gotten hold of its meaning, not realizing that its meaning is not amenable to simple observation. As with any other unobservable, one must be reminded, once again, that a meaning is a hypothesis that has to be treated as testable and expected to yield testable predictions. To insist as does CS on the meaning-message distinction is to recognize that even though messages are on the observation side, meanings are not. A meaning is always on the theoretical side, and is always a provisional, explanatory hypothesis subject to disconfirmation.

The problem from a CS point of view, then, is not, as I think your paper suggests, that Bolinger, Clark, Confais, Goldberg, and all the others are making unwarranted appeals to the speaker, the content of whose mind only the speaker knows. The problem is that Bolinger and all the rest have not performed the meaning analyses that would have to accompany their pronouncements. I don't think we can shy away from mentalism as if the mere mention of the speaker's intentions invalidated all analyses. I have no doubt that speakers intend to communicate using the forms of the language. Nor do I have any reason to doubt that these forms have stable meanings. What I do find is that, outside CS, there has been little awareness of what

is required to discover what these meanings are. And this is so, to repeat, mostly because discovering them usually requires, first, that the traditional categories of the sentence and the names of the forms be set aside, and that the hypothesized meanings not be drawn from fragments of the message, but be formulated in a manner that can be testable and generative of predictions.

Before getting to variationist studies, a final point about one idea in your paper that is worth seeing in light of the CS distinction between meaning and message. And it is the idea of *reference*. Reference, I think we would all agree, is not an element of language at all; it is an element of the use of language, a fact of the message, of the communication. To *refer* is to do something with the tools of language, it is not itself a linguistic tool. Reference is on the side of speech, of use, of the trips to the supermarket; it is not on the side of the engine that is the grammar (the collections of meanings) of the language. The facts of reference may be useful for the task of discovering meanings, but they're not meanings themselves. The Spanish form that makes reference to future time and to present time (as in the conjectural messages) and that grammarians have called the Future is not likely to have FUTURE as its meaning.

So let me finally get to what you say about variation.

Faced with the centuries-long failure of the grammarians that is so originally documented in your paper, the CS literature and the variationist literature have extracted different lessons. Both lessons are important and, I think, probably compatible. Let me comment on one final aspect of your paper with this in mind.

One of the central points of your paper is that the forms that the grammarians have so spectacularly failed to understand are forms in variation. In other words, the forms that the grammarians have so unsuccessfully tried to study in different languages need to be recognized as cases of variability. That, you say, is the point the French grammarians missed. These forms, as you well point out, are known to be in variation, among other reasons, because, often in the same context, they alternate with one another while saying the same thing, making the same reference.

I have a very positive reaction to this part of your paper, but also a concern having to do with theoretical coherence. (My interest in theoretical coherence, which I know from experience is something that many linguists tend to find a tedious subject, is reminiscent of Saussure's claim that linguists tend to be very good at what they do, but not very good at understanding what they do.) The positive reaction to what you say about the forms in question being in variation comes from my belief that the success of variationist analyses (including, I hope, my and my students' modest efforts at understanding Spanish *yo, tú, él, ella*, etc. in New York) is among the happiest facts in the often unhappy discipline of linguistics. I think that the proposals first put forth by Labov have yielded studies at the highest levels of accuracy, descriptions that capture the speech of communities in ways that few

other theoretical approaches have come close to capturing. And as in your recent demonstration about the nearly instantaneous adaptation of other-language forms, and their consequent status as borrowings, and not switches, these studies have not only provided extraordinarily accurate descriptions, but also theoretically coherent paths to new kinds of understandings.

But I have a concern, having to do with what I read (and I hope I read it right!) as the reiteration in your paper of the old idea that the forms that appear in variable contexts, and that we regard as in variation, are in some sense equivalent, are in some sense making the same reference, are saying the same thing. As I see it, thinking this way is how we got to the untenability of the Doctrine of Form-Function Symmetry in the first place. This problem, the old Lavandera problem if we can call it that, comes from the mistaken belief that in order to have variability we have to see our way clear to a theoretical conception of the variants as in some sense synonymous, at least synonymous in the particular context of variation under study. But we don't have to do that. From the CS perspective, the two forms in variation may be making the same reference; that's a fact of the message. And they at some level may be 'saying the same thing'; that too is a fact of the message. But that does not make them equivalent, and that is not what makes them amenable to a variationist treatment.

Convergence in reference happens all the time in situations where sociolinguists have not invoked the notion of variation, probably correctly so. The reason is that reference usually constitutes a very superficial analysis of the message. This point has been made for years, usually in discussions of larger expressions. Two expressions making the same reference often share no other notional import, because they say very different things about the same referent. To take some of the old saws from philosophy, do we want to say that the two expressions that have the same referential meaning, *the morning star* and *the evening star*, are in variation? And let's take then the smaller forms that are the usual concern of the variationist. Do we want to say that the English forms called singular and plural are in variation because of usages like *The dog that is beaten is traumatized* and *Dogs that are beaten are traumatized*? The reference is the same, a generic reference to the animal, but the form -0 that is called the singular and the form -S that is called the plural are contributing to two different messages, and we don't usually study singulars and plurals as variants, don't usually say that this is a context of variation for -0 and -S, any more than we would say that we have a variable context in *I own one dog* and *I own two dogs*. The conclusion arrived at by a scholar familiar with both variation and CS theory would be that whatever it is that licenses the notion of variation, it is not sameness of reference.

In familiar sociolinguistic variants in Spanish like the synthetic and periphrastic future, it is not the sameness of reference of expressions containing these forms,

their saying the same thing, that qualifies *Llamará mañana* and *Va a llamar mañana* (both translating ‘he will call tomorrow’) as containing instances of the two sociolinguistic variants. And in the Spanish variants usually called overt and null pronouns that I studied, it is not referential equivalence that qualifies *Dijo que él venía* and *Dijo que venía* (both translating ‘he said he would come’) as the proper object of sociolinguistic study. Under analysis, each of the forms in the pairs of expressions is used for different reasons and responds to different communicative goals. These expressions only ‘say the same thing’ if one concentrates on the roughest of referential equivalences and ignores all the ways in which they’re different. The forms are contributing to referentially equivalent expressions that, however, communicate different messages. But the forms are, nevertheless, in variation. We should *not* have variation on the one side and differences of meaning on the other. When meanings are properly understood, we have *both* variation *and* different meanings, and different messages too, but for the rough superficial referential equivalence.

The theoretical choice, then, is not between thinking of Weiner & Labov’s two forms (two constructions in their case) as either, under one view, involving the same reference and consisting of the same constitutive meaning or meanings, and thus being eligible as sociolinguistic variants, or, under another view, as involving different references and consisting of different constitutive meaning or meanings, and being thus ineligible as sociolinguistic variants. No, I think what makes more sense is to think of them as making, if you like, the same reference, but as still having other, more subtle than reference, differences of message, and certainly as having different constitutive meaning or meanings, *and still* being eligible as sociolinguistic variants. In other words, syntactic variants in a proper sociolinguistic analysis can be the study of forms that have different meanings in the CS sense of the term.

When considered with a sustained interest in theoretical coherence, the very successful variationist studies with which we’ve become familiar should not be seen as efforts separate from the overall effort of understanding the distribution of forms. There’s really no theoretical linguistics and variationist linguistics; they’re both the same. A variationist analysis, on the CS view, need not, and probably should not, be conceived of as a separate project, but as an intrinsically theoretical project that’s hacking away at the very dense and complicated thicket that’s the study of the syntax underlying actually occurring speech. For a CS linguist, then, syntactic analysis and variationist analysis are, in this sense, one and the same.

So if not reference, then, what qualifies two forms for sociolinguistic treatment? What is a variable context? If we ask CS linguists who’ve thought about this problem, the answer they give is that variable contexts are those where two forms constitute *two viable expressive choices*, contexts where the user of the language finds, systematically, that two forms can be usefully deployed (to produce subtle differences in the message or to attain other important communicative goals).

When writing in Spanish, the late Erica García liked to describe variation as: “comparabilidad de unidades lingüísticas con cierta equivalencia comunicativa y parcial superposición distribucional.”

And finally, so what? Why is this a better way of thinking about variation? Because this is the approach to variation *that most clearly opens a path to explanation*. In other words, this is the approach that can best make sense in a theoretically coherent fashion of internal factor groups (the external or social factor groups are another story). When, as in the proposal reiterated in your paper, the variable contexts are theoretically conceived of as fully equivalent and the forms that vary are essentially taken as synonymous, there is no reason for the user of the language to choose one or the other. To be sure, sociolinguists have been known to say that in some cases the use of one or another variant is ‘motivated,’ but they cannot in fact say so coherently; there can be no motivation for choosing between forms that are really saying the same thing, unless we understand that their synonymy is only at the superficial level of coreference, in which case there’s no synonymy.

Putting it another way, where sociolinguistic variants are synonymous, and when so are the alternate expressions where they each appear, there is no theoretically coherent reason for one of the factors of an internal factor group to come out as preferred (to show up with a value of greater than 0.50 in Varbrul or greater than 1.00 in SPSS) and for another factor to come out as dispreferred (with a value of under 0.50 in Varbrul or under 1.00 in SPSS). If the Spanish overt and null pronouns are two variants of exactly the same linguistic unit, and if expressions with and without these forms are saying exactly the same thing, there’s no way to coherently ask the question (never mind answering it) why in the many variationist studies of these forms does the Switch Reference factor group always come out the same (switch reference favors overts). Only if we admit that there is a variation, but that there’s also a difference between overts and nulls can we make sense of the results (this usually means that what we have is not a null pronoun but an absent pronoun, but that’s another story). If the Spanish periphrastic future and the synthetic future are the same, there’s no reason for the factor ‘direct speech’ to favor the periphrastic future and for the factor ‘quoted, referred, or indirect speech’ to favor the synthetic future, as CS researchers studying these forms in Argentina have shown. So the answer to the So What question, the reason that it matters how we understand variation, the reason that theoretical coherence counts, and the reason that we still have to listen to Saussure’s injunction to not only do it, but also to understand what we’re doing, is that we want to develop a connection between the great power of variationist results and the goal of explaining those results.

I’ve asked Wallis Reid, who is the most senior, and I think the most insightful, of CS scholars actively working today, to answer the question of what is variation, and how the answer connects with explanation. Wally says: “[When] variation is defined

as speakers choosing (or not choosing) signs from among those whose meanings make them viable expressive options in certain contexts, a grammatical explanation is always a strong possibility ... The advantage of [the CS conception of two different meanings in variation] is that it integrates the account of variation – that is, the choice among signs of comparable expressive value – into a comprehensive account of the distribution of the signals of those (same) signs.”

Well, my dear Shana, I think I will stop for now. Again, many thanks for your paper, for your wonderful teachings to all of us over the years. Te deseo un 2018 muy feliz, con mucha vida y mucha lingüística. Un saludo muy cordial,

Ricardo

Note from the editors

Ricardo Otheguy wrote this letter to Shana Poplack after she invited him to read her chapter in this volume. The exchange between these two prominent scholars shines a light on the relationship between variationist and cognitive-functional frameworks, highlighting points of convergence as well as divergence in their approach to describing and explaining linguistic phenomena (see also Otheguy 2012).

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Variable grammars

Competence as a statistical abstraction from performance. Constructing theories from data

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Linguists generally postulate a mental grammar which children infer from the speech they encounter, and then use to generate their own speech productions. This grammar is often assumed to be invariant and categorical. Language in use, however, is massively variable: the child encounters diversity at the level of dialect, sociolect, and idiolect. Furthermore, all units of language have multiple realizations and fuzzy boundaries. This raises a fundamental question: if the data is variable, even continuous, how does the child arrive at a grammar that is categorical and discrete? I argue that the system that a learner infers is not invariant and discrete, but rather one that recognizes, incorporates, manipulates, and generates variability.

Keywords: child language acquisition, variable grammar, variable rules, variationist sociolinguistics, linguistic variation

1. Introduction

The pioneering work by Cedergren and Sankoff (1974) on the place of variation in linguistic theory begins with the statement “Speech performances are here considered as statistical samples drawn from a probabilistic language competence.” The paper articulates the foundational ‘variable rule’ model for generating a quantitative representation of the distribution of a variable in speech from a set of linguistic (and possibly extra-linguistic) predictors. Hence, it focuses mainly on the question of production: how an underlying competence can generate “orderly heterogeneity” (Weinreich, Labov, & Herzog, 1968): the regular patterns of variability that are evident in speech. As the above quotation indicates, the authors affirm an abstract competence distinct from performance, but one that is probabilistic rather than deterministic. Implicitly, this model also raises questions about acquisition: how

the child develops probabilistic linguistic competence through observation and interaction.

The prevailing view of acquisition among linguists postulates a mental linguistic system – a grammar – which children infer from the speech they encounter (perhaps aided by a prewired universal grammar), and then use to generate their own speech productions. We emulate this process in our own work, by inferring theories of grammar from the speech we observe (perhaps aided by ‘intuitions’ which are presumed to provide fairly direct access to the grammar). In many theoretical traditions, this grammar is assumed to be invariant and categorical. Language in use, however, is massively variable: the child learner and the linguist encounter diversity at the level of dialect, sociolect, and idiolect, as well as stylistic variation within the usage of each individual. Language usage also exhibits what Weinreich et al. (1968) call ‘inherent variability’: all units of language (articulatory gestures, phonemes, syntactic structures, semantic interpretations, etc.) have multiple realizations and fuzzy boundaries. This raises a fundamental question: if the data is variable, and in some cases even continuous, how does the child arrive at a grammar that is categorical and discrete?

There are two logical approaches to this question. The one that prevails in most formal theoretical linguistics postulates that language acquisition is an abstracting, generalization-seeking process, which overrides observed diversity in the pursuit of categorical rules and units of grammar. This projects the theory-constructing predilections of the linguist onto the mental processes of the child. This approach relegates the diversity of speech to grammar-external processes in production – the deformations that Chomsky famously attributed to “memory limitations, distractions, shifts of attention and interest, and errors” (Chomsky, 1965: p. 3). But there is a second alternative that obviates the question: acknowledge that the linguistic system that the child learns is not invariant and discrete, but rather is one that recognizes, incorporates, manipulates, and generates variability. In other words, the mental grammar reflects the “probabilistic language competence” that Cedergren and Sankoff postulate.

This is the approach that is argued for here. Probabilistic variability is not just an inherent property of language use, but of linguistic competence itself. The mental linguistic system that comprehends ‘orderly heterogeneity’ in the input and produces it in the output incorporates probabilistic processing in its internal structure. For the linguist, recognizing the probabilistic capacity of linguistic competence provides the basis not only for a theory of language use, but also for an adequate theory of language.

Evidence for this probabilistic competence is apparent in all facets of language. It is abundantly clear that speakers have an exquisitely subtle capacity for understanding social variability. Simply by hearing the voice of a person speaking a

language that we know, we are able to make good to excellent estimates of whether they are a native speaker or not, what dialect they speak, the speaker's sex, approximate age, and physical or emotional states like sleepiness, illness, anger, tension, humor, level of formality or politeness, familiarity with the interlocutor, etc. In some communities, we may also be able to identify social class or educational level. Besides recognizing and correctly interpreting the linguistic cues associated with these social variables, we have a variable productive capacity – more limited in scope, but universally present in all human beings. We all vary stylistically, adapting our speech to interlocutors, social settings, topics, locations, and purposes; we also manipulate sociolinguistic variables to construct our own social identities, to conduct our relationships with others, and to communicate stances and emotions. The variable elements of language involved in these processes permeate the linguistic system, including phonetic, phonological, morphological, syntactic, and lexical elements.

It is also abundantly clear that variability of this sort is not random and disorganized, but orderly. Linguistic variation is orderly in terms of its systematic relationships with the social dimensions described above, and also in terms of its systematic conditioning by linguistic structure. A half-century of research on linguistic variation shows how variables of all kinds are constrained by the linguistic contexts in which they occur, appearing more frequently under one condition and less under another. The variant realizations of such a variable are analogous to allophones or allomorphs, but instead of occurring categorically in one context and never in another, they are probabilistically distributed in regular patterns across the spectrum of relevant environments.

This linguistic conditioning of variables, the relative favorability or unfavorability of particular contexts to the occurrence of a given variant, provides essential clues to the structure of the linguistic system, just as much as the categorical distributions privileged in much of formal linguistic theory. It is certainly true that categorical conditions abound, giving evidence about language structure: in English, /ŋ/ only occurs in syllable-final position, never initial, and articles only occur pre-head; in German, verbs are always final in subordinate clauses. But probabilistic phenomena also provide significant evidence about grammatical structure. In English, /θ/ is usually, but not always, word-initial or final, while /ð/ is usually intervocalic. This distribution reflects the fact that these were historical allophones of a single phoneme, voiced intervocalically, voiceless elsewhere. The occurrence of final /ð/ in verbs reflects the historical presence of verbal inflections, now lost: *breathe*, *teethe*, *bathe*. In syntax, 'heavy,' more complex NPs are more likely but not certain to occur last in double object constructions, perhaps reflecting processing constraints. Overt pronominal subjects are disfavored, but not impossible, in the second clause of conjoined VPs with the same subject referent (e.g., *When it was*

Harry's turn to speak, he stood up and (he) launched into a diatribe about health insurance.)

Although often treated as a dichotomy, these two kinds of conditioning – the categorical and the probabilistic – are in quantitative terms, simply different points on a continuum. Categorical constraints are at the extreme ends of the probability distribution: under a given condition, the phenomenon of interest always occurs – i.e., has a probability of 1, or never occurs – a probability of 0. A variable occurrence means a probability between 0 and 1. Specific rates of occurrence of a phenomenon can be obtained empirically, but theoretical claims about categorical behavior are not often empirically validated, so some supposedly categorical properties represent the untested hypothesis of the analyst (Bresnan, Cueni, Nikitina, & Baayen, 2005; Bresnan & Ford, 2010).

The non-categorical probabilistic distributions of linguistic variables that occur in natural language are clearly perceptible to speakers. The psychological literature amply documents the human cognitive capacity to track and accurately match probabilities (e.g., Estes, 1950; Gallistel, Krishan, Liu, & Miller, 2014). Thus, probabilistic information is available and accurately processed in both perception and production. It requires something of a leap of faith to postulate that such information is nevertheless excluded from linguistic cognition – i.e., from competence and grammar.

Finally, a probabilistic component in the grammar is essential for an adequate account of language change. Linguistic change is always associated with synchronic variability; a linguistic variable marks a point in the grammar where change is possible. Like biological evolution, linguistic change depends on the occurrence of variety, and consists of the gradual expansion of one variant at the expense of another. All historical changes for which the time-course can be tracked with sufficient resolution show a gradual quantitative shift from old to new forms. An attempt to model such facts in terms of a succession of discretely but infinitesimally different categorical grammars is more faith-based than realistic. A probabilistic grammar provides an integrated account of synchronic variability and diachronic change: what changes across time is the same parameter that governs the choice between the alternant forms at a given point in time.

2. Variation in linguistic theory

Formal theories designed around categorical conditions and discrete distributions have mostly dealt with the facts of variability in three ways. One is to relegate variation to performance, and hence declare it to be irrelevant to or outside of the grammar. We will present data contradicting this position below. The second common

approach is to postulate that apparent variation is actually categorically constrained by as yet undiscovered conditions. Hence, any apparent ‘variation’ actually reflects a mixture of discretely different contexts that the analyst has not yet been able to identify, some that are postulated to categorically require variant A, and others that categorically require variant B. This tactic is inspired by historical examples of apparent indeterminacy resolved by subsequent discoveries; the prototype is Karl Verner’s (1877) discovery of a stress constraint on the ‘Grimm’s law’ sound changes in Proto-Germanic. Grimm (and others) showed that Proto-Indo-European voiceless stops mostly became voiceless fricatives in Germanic, but in some words the Germanic reflex was a voiced stop. Verner demonstrated that the latter outcome occurred word internally after unstressed syllables. The Neogrammarian school took this as evidence in support of the hypothesized ‘exceptionlessness’ of sound change; any apparent exceptions would eventually be explained in terms of categorical conditions (Jankowsky, 1972). The Grimm-Verner case is compelling, but as a systematic account of variation, this model is sorely lacking. As Otheguy (2008) dryly notes, “The supposition that, when a successful linguistic analysis is produced, variation will disappear is not well founded.” In fact, such a hypothesis is unfalsifiable, and hence it is undeserving of scientific attention.

Formal theories that engage more seriously with questions of variation take a third approach: they seek to delimit the grammatical elements involved in alternation, for example by means of parameters that specify contrasting grammatical structures. These capture typological differences between languages (e.g., Null Subject and Non-null Subject languages), but they can also be used to model language-internal variation in terms of alternating parameter settings. This is essentially the approach taken by Kroch’s grammar competition model (Kroch, 1989, 1994), which treats variation during the course of syntactic change in terms of two competing grammars, present simultaneously in the speech community and in the minds of individual speakers. One grammar generates variant A, the other generates variant B (perhaps reflecting contrasting parameter settings), and the mixture in speech reflects speakers’ varying use of each grammar. Diachronically, this model explains change in terms of a changing frequency of choosing one grammar over the other.

This approach has also been taken by scholars working in the Optimality Theory (OT) framework. The discrete but competing constraints in such models can, in principle, generate many possible surface forms, depending on their relative rankings or strengths. If rank order is fixed, surface realizations are invariant, but with partial ordering (cf., Anttila, 1997) or weighted stochastic ordering (cf., Boersma & Hayes, 2001), surface variation can be generated, and specific frequencies of occurrence can be modeled. For example, a variable deletion process could be modeled by competition between a constraint favoring deletion and a faithfulness constraint favoring retention. If the FAITH constraint has the highest ranking

say 75% of the time (or in Anttila's model, in 75% of possible orders), the surface output will show 25% deletion.

A problem that these approaches have faced is the interaction of multiple constraints on a variable process. To take one example, coda *-s* deletion in Spanish is consistently and significantly constrained by the preceding context, following context, syllabic stress, position in the word, speech rate, and morphological status (cf., Hoffman, 2004, among others). These are orthogonal, and quantitatively cumulative: i.e., a lot of weaker contexts favoring one outcome can collectively outweigh a very strong context favoring a different outcome. An account of such facts in an OT approach will attribute each of these effects to a different constraint set, and generate the surface variability by the many possible orders they can all take. It is not clear that such a grammar can adequately predict the observed patterns of surface frequencies, as it would be very difficult to compute, much less learn. Crucially, a theory that maintains the standard OT convention that the highest ranked constraint prevails, regardless of what outcomes are favored by lower-ranked constraints, cannot replicate the probabilistically cumulative distribution of variants documented in 50 years worth of studies of linguistic variation.

In response to this problem there have appeared variants of OT that question that convention and permit cumulative weighting. A recent study by Zuraw and Hayes (2017) explores approaches in Harmonic Grammar and Maximum Entropy that effectively render these models probabilistic, and, consequently, make possible a more straight-forward computation of multiple constraint effects, what they call "intersecting constraint families." Their favored mathematical model uses a logistic function, just like the variable rule model. At the extremes, categorical effects are accommodated in the model by assigning weights to such contexts that are so large as to permit only one outcome, which is a standard property of logistic models.

Such approaches mark an important step towards a linguistic theory that is empirically grounded and capable of modeling natural language. An intellectually honest appraisal of what linguistic theories need to account for must include the variable properties of language as well as the categorical ones. A probabilistic grammar has room for both, and the advantage of being learnable, computable, and accurate.

3. Probabilistic competence

Leaving aside the substantial work that speakers do in perceiving, acquiring, and producing the socially meaningful elements of linguistic variation, let us consider how the linguistic distribution of variables illuminates the structure of language and facilitates the acquisition of linguistic competence.

We begin with a phonological example. English has a general process of simplifying coda clusters by deleting coronal stops, so that utterances like *'east side'* or *'send me'* will routinely be produced without the final /t/ or /d/ in the first word. This process is sociolinguistically variable, showing the social and stylistic distribution typical of a stigmatized variant: less deletion by higher status speakers and in more careful speech styles. But, at the same time, it is subject to linguistic conditioning that shows this process to be intimately embedded in the phonology, morphology, and lexicon of English.

One constraint on coronal stop deletion (CSD) that is evident in many varieties of English is the place and manner of the preceding segment. As Guy and Boberg (1997) show (see Table 1), deletion is favored when the preceding segment is coronal (e.g., s,z > f,v; n > m,ŋ), noncontinuant (p,b > f,v; n > l), and nonsonorant (s,z > l; p,b,k,g > m,ŋ). These are the features that characterize the targets of deletion, /t/ and /d/, which are [+ cor, -cont, -son]. Hence, the deletion rates are highest after sibilants, which are coronal and nonsonorant (e.g., *most*), and stops, which are noncontinuant and nonsonorant (e.g., *act*, *apt*), and /n/, which is coronal and noncontinuant (e.g., *hand*). These segments all share two points of phonological similarity (i.e., two features) with the deletion target. By comparison, deletion rates are lowest following segments that share only one point of similarity with the deletion target, such as liquids and noncoronal fricatives (e.g., *cold*, *left*). The obvious generalization is that the probability of deletion is a function of the similarity between the context and the target of deletion: there is more deletion as the preceding segment is more similar to the final /t,d/.

The overall effect of this constraint on CSD is to avoid surface sequences of adjacent similar segments, and to enhance phonological contouring. This thus reflects the Contour Principle (Leben, 1973; Yip, 1988) – the general phonological

Table 1. Preceding segment constraint on English coronal stop deletion (from Guy & Boberg, 1997)

	N	% deleted	Factor weight ^a
Two features shared with target:			
/s,z,ʒ/ + cor, -son	276	49	.69
/p,b,k,g/ -cont, -son	136	37	.69
/n/ + cor, -cont	337	46	.73
One feature shared with target:			
/f,v/ -son	45	29	.55
/l/ + cor	182	32	.45
/m,ŋ/ -cont	9	11	.33

^a Probability estimate of the effect of a predictor, from a multivariate logistic regression with Varbrul2; 1 = categorical deletion, and 0 = categorical retention.

tendency to prefer alternations of phonological units, which can be observed in constraints on tone sequences, syllable structure (CV), metrics, etc. It operates as a categorical constraint on some processes in some languages (hence the name ‘Obligatory Contour Principle,’ or OCP). The categorical prohibition against sequences of identical segments in English is one such ‘obligatory’ case; thus the –s and –ed suffixes of English are obligatorily realized as syllabic in words with a root ending in –s or –t/d (*glasses, baited* vs. *cats, tapped*). The epenthetic vowel in the suffix creates a CVC contour, and avoids a prohibited *ss or *tt sequence.

Phonologists such as those cited above have treated the Contour Principle as part of competence, a constraint on the grammar. The probabilistic grammar argued for here incorporates both the obligatory constraints against adjacent identical elements and the probabilistic constraint against adjacent similar elements that is evident in coronal stop deletion as instantiations of one generalized constraint on the phonology. In the obligatory cases, the constraint effects have a probability of 1, while in the coronal stop deletion case, the effect has a high probability when adjacent segments are very similar and contour violations are most marked, and a declining probability as the segments are less similar and contour violations are minimal.

Coronal stop deletion is also sensitive to morphological structure. A much-replicated finding is that inflected words like *missed* and *packed* undergo significantly less deletion than uninflected words where the final cluster is part of the root, like *mist* and *pact* (cf., Guy, 1980, 1991, 1996; Santa Ana, 1992; Fruehwald, 2012). For many speakers, a third morphological category is also distinctive, the irregular past tense forms where the final stop is an affix, but other alterations to the root are also present, especially root vowel changes; e.g., *keep-kept*, *tell-told*, *lose-lost*, *leave-left*. These words undergo deletion at an intermediate rate. Table 2 gives the relevant deletion rates from three representative studies of the variable in different corpora of sociolinguistic interviews.

Table 2. Morphological class constraint on English coronal stop deletion in three studies (% deleted)

	Monomorphemes <i>mist, pact</i>	Irregular past <i>lost, kept</i>	Regular past <i>missed, packed</i>
Guy (1991)	38	34	16
Santa Ana (1992)	58	41	25
Fruehwald (2012)	49	37	22

Various explanations of these facts have been proposed. Functionalist accounts appeal to the different functional loads carried by the final stop in each class (Guy, 1996). Accounts that appeal to differences among the classes in internal boundaries (Guy, 1980) or structural organization (Fruehwald, 2012) argue that the

phonological process interacts with and is constrained by elements of morphological structure. Guy (1991) and Santa Ana (1992) propose a derivational account in the lexical phonology framework where the deletion process applies both within the lexicon and postlexically, such that the three classes differ in the number of points in their respective derivation where they are exposed to the possibility of deletion. This model predicts an exponential order among the rates of stop retention (i.e., non-deletion) of the three classes, which, it will be noted, provides an excellent fit to the data in all the studies cited in Table 2. But all of these theoretical treatments begin from the same understanding of the place of variation with respect to competence: the sensitivity of the process to the morphology of the words it affects is *prima facie* evidence that it is part of the grammar. And since the effect is quantitative – deletion is inhibited in inflections, but not categorically prohibited – this further entails that the grammar is probabilistic.

Probabilistic grammatical constraints on variation are not confined to phonological variables. Similar patterns of non-categorical but grammatically regular distribution of variants occur in syntax, involving relationships at a distance, making reference to constituency structure, sequential ordering, movement, etc. Two examples from Brazilian Portuguese illustrate the kinds of phenomena at issue. Portuguese, like many Indo-European languages, has number agreement between subject and verb, and within the noun phrase. Historically, and prescriptively in the contemporary standard variety, both of these agreement relations are presumed to be categorical: plural subjects are expected to always occur with plural verbs, and all items other than adverbs in a plural NP/DP are expected to be plural-marked. However, in popular speech number agreement is variable in both verbs and NPs. Crucially, this variation is not random, but highly constrained by syntactic structure.

Consider first the constraints on subject-verb agreement in popular Brazilian Portuguese (PBP). Prominent among them is a linear position effect. Subjects located immediately before the verb trigger the highest rates of verbal plural marking. Preverbal subjects that are distant from the verb (i.e., separated by intervening material such as adverbs or modifying phrases) are somewhat less likely to trigger plural marking, while post-verbal subjects are associated with very low rates of plural marking on the verb.

Table 3. Subject position constraint on verbal number agreement in popular Brazilian Portuguese (from Guy, 1981)

Subject position	N	% plural marked	Factor weight
Immediate preverbal	1861	74	.54
Distant preverbal	597	56	.31
Post-verbal	199	27	.18

Again, these facts are susceptible to various theoretical explanations. Coelho (2000) accounts for the low rates of agreement with post-verbal subjects in unaccusative verbs (e.g., *Sumiu os taxi* ‘The taxis disappeared_{-sg}’ as opposed to *sumiram os taxis* ‘...disappeared_{-pl}’) as a consequence of VP-internal subjects failing to raise to a pre-verbal position, and hence not undergoing feature checking. Given that post-verbal subjects are now rare in Brazilian Portuguese (as the Ns in Table 3 illustrate), it is possible that the grammar now disqualifies some of these from subjecthood. Feature matching, spreading, or percolation accounts are also possible. But any adequate account of these facts must explain why these particular syntactic structures are associated with these specific rates of occurrence of the syntactic operation that generates number agreement. Agreement is not a randomly selected option in an otherwise categorical grammar; rather it is variable operation embedded in a probabilistic syntax.

Another position constraint on agreement in PBP appears in variable nominal agreement. The observation there, systematically replicated in many studies and obvious from the most minimal inspection of colloquial speech, is that plural markers abound early in an NP, and become rarer towards the end. Relevant data from two studies with robust Ns are given in Table 4: Guy (1981) studied illiterate working class speakers in Rio de Janeiro, while Oushiro (2014) investigated a social cross section of speakers in São Paulo. Numerous studies in other regions of Brazil have found similar results (e.g., Braga, 1977; Scherre, 1988).

Table 4. Position constraint on nominal number agreement in popular Brazilian Portuguese in two studies.

Position in NP	Guy (1980) – Rio de Janeiro		Oushiro (2014) – São Paulo	
	N	% plural marked	N	% plural marked
First	5,247	95	14,246	99.7
Second	3,947	28	16,934	86
Third	552	21	2,725	86
Fourth and fifth	42	11	225	87

Plural marking in PBP is nearly categorical in the first word in an NP. In the Rio de Janeiro data, drawn from maximally vernacular speakers with no schooling and low social status, there is a huge drop (67%) in the second position, followed by progressively lower rates of plural marking in later positions. In the São Paulo data, drawn from a corpus that includes many speakers with secondary or tertiary education and middle to high social status, the decline in plural marking between the first and second positions is a more modest 15%, and there is no significant decline thereafter. In both varieties, it is almost always the case that marking never skips a

word; any unmarked word will never be followed by a marked word¹ (hence *as casas branca*, *as casa branca* ‘the white houses’ are possible, but *a casas branca*, *as casa brancas*, *a casa brancas* are unattested, and typically rejected by native speakers.)

These data are presented in terms of the linear position of words in the DP/NP, but other studies have pursued a more detailed breakdown by the structure of the phrase. By far the most common phrase structure in the data is a Determiner + Noun sequence, in which the determiner will almost always be marked and the noun will often not be marked (*os amigo*, *umas casa* ‘the friends, some houses’). But the initial position may also be occupied by an adjective (*velhos amigo* ‘old friends’), possessive (*meus amigo* ‘my friends’), quantifier (*todos amigo* ‘all friends’), or even a noun (*amigos velho* ‘old friends’), and in almost all of these structures, the first word will bear a plural marker. Scherre (1988) argues for a structural analysis that contrasts items occurring before the nominal head, the nominal head itself, and items occurring after it. However, for most of the data, this kind of analysis is coincident with one relying on the linear position.

What grammar of agreement is adequate for generating this regular pattern in PBP? Simply declaring agreement to be optional rather than obligatory is clearly inadequate. A satisfactory account needs to generate essentially obligatory marking in initial or pre-head position, more or less regardless of the word class occupying that structural slot, and declining probabilities of plural marking in subsequent positions. It must also predict that marking never skips an eligible word (i.e., there is no marked word with an eligible unmarked word to its left in the same phrase).

One model that satisfies these requirements relies on probabilistic feature spreading: the plural phi-feature is attributed to a node dominating the entire phrase, and then percolates or spreads down the tree from left to right, with a probability of recursion that is less than 1. The spreading model explains the absence of skipping: if the plural feature has not spread to a given node, it is not available for spreading to a subsequent node.

Alternative models to explain these facts that are not internally probabilistic are difficult to sustain. A grammar competition model (Kroch, 1994) might work for the São Paulo data, if it postulated alternation between a grammar with obligatory agreement and one with only initial plural marking, with the former selected about 86% of the time. But for such a model to generate the pattern found in the Rio de Janeiro data (and elsewhere in other studies) – a progressive decline in plural

1. Some exceptions to this occur in phrases with double determiners, such as *as minhas casas* ‘the my houses’ and *todas as casas* ‘all the houses.’ Such cases are occasionally produced without a plural marker on the first word: *a minhas casas*, *toda as casas*. This may indicate that the first element projects a higher node outside the core DP in which agreement operates.

marking in later positions – would require either a very complicated inventory of competing grammars, or some statement about which grammar is selected at what rate for NPs of different lengths. It would also require some stipulation to prohibit skipping. None of these models would offer a compelling alternative to a grammar with probabilistic generalizations, rules, and constraints.

4. The fuzzy lexicon

Probabilistically orderly variability is also evident in the lexicon. The lexicon is commonly conceived as the repository of lexically arbitrary information – the phonological content and semantic significance of words and morphemes, and information about derivational and inflectional class membership. In the traditional generative conception, the phonological representation of the word or morpheme is cast in terms of a unique string of phonemes, which serves as the underlying representation for any occurrence of the word in an actual utterance. When variant realizations of morphemes are found that cannot be reduced to a single underlying representation, allomorphs – alternative representations – may be recognized, but categorical theories define these as bound by categorical constraints (e.g., the *-en* participial suffix in English occurs only with a specific subset of verbs: *taken*, *frozen*, *eaten*, *fallen*, etc.). In this view, the lexical entry has no place for probabilistic information such as frequency of occurrence, or likelihood of variant realizations of segments or inflections in various contexts.

Quantitative studies of variation, however, provide substantial evidence of probabilistic lexical properties, consistent with a non-deterministic, ‘fuzzy’ model of representation. We will consider two kinds of such evidence: probabilistic representations of lexical frequency, and probabilistic indications of lexical exceptionality. We also discuss usage-based or exemplar theories of the lexicon which directly incorporate information about realizations, frequencies, and contexts.

4.1 Lexical frequency

Lexical frequency effects in variation have been much studied in recent decades. A number of variable processes have been shown to be systematically conditioned by the frequency of occurrence of a lexical item. One example is the *-ing/-in* alternation in English. The occurrence of the *-in* alternant is positively correlated with the frequency of the root with which it occurs. Figure 1 shows the results of a study by Laturus, de Vilchez, Chaves, and Guy (2016).

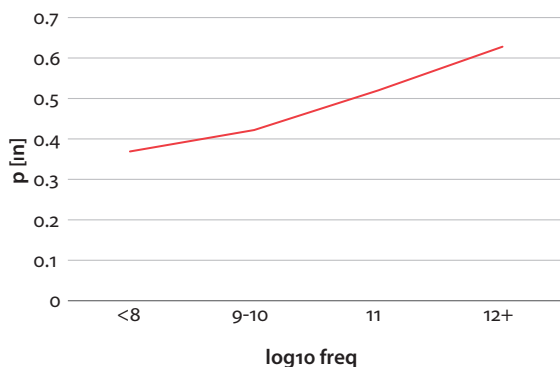


Figure 1. [ɪn] by lexical frequency in American English (from Laturus et al. 2016)

Similar facts obtain in the case of English coronal stop deletion. Figure 2 is taken from Guy, Hay, and Walker (2008), showing the positive correlation between lexical frequency and deletion.

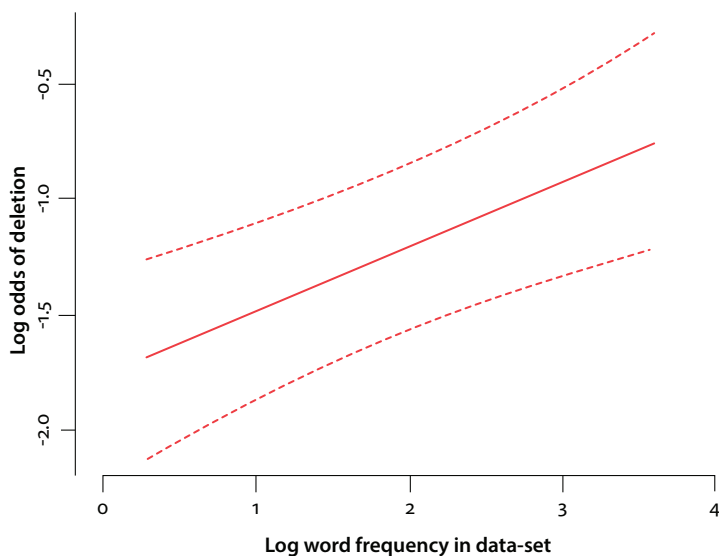


Figure 2. Coronal stop deletion increases with lexical frequency (from Guy et al., 2008)

Such findings imply that the lexicon contains information about lexical frequency, which is available to condition selection of variants or interaction with phonological processes. The quantitative evidence also illuminates the question of what lexical elements are associated with this frequency information – roots or fully derived forms?

The frequency constraint on coronal stop deletion illustrated in Figure 2 happens to interact with the morphological constraint shown in Table 2. The nature of this interaction is that the frequency effect – increasing deletion with increasing frequency – is apparent only for uninflected root forms, whereas the regular past tense forms are unaffected by lexical frequency. Figure 3, from Myers and Guy (1997), illustrates this point. The implication is that frequency information is associated only with roots, which are stored in the lexicon, and not with regularly derived forms.² This is consistent with Pinker’s (1999) ‘words and rules’ model of the lexicon.

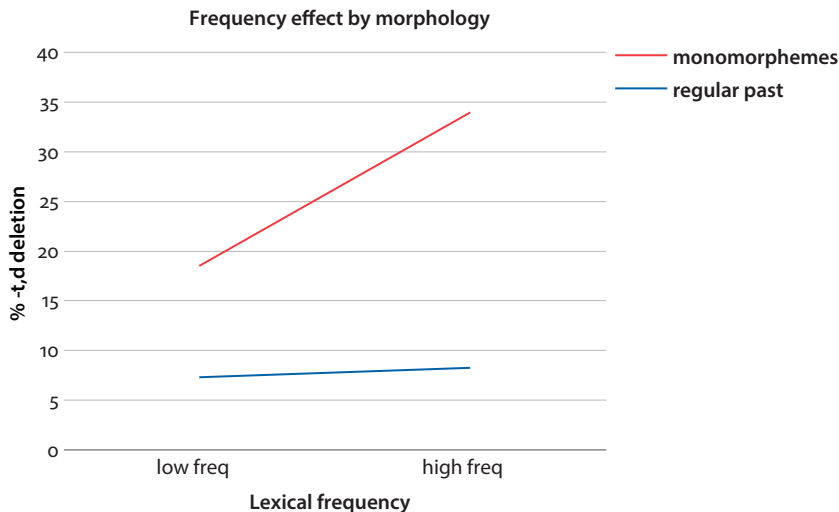


Figure 3. Coronal stop deletion: frequency interacts with morphological structure

What models of lexical representation can account for these facts? There are two contenders that appear to achieve at least observational and descriptive adequacy. One is the usage-based phonology of Joan Bybee (2001, *inter alia*), in which probabilistic properties of the lexicon are a central focus. In this approach, the lexicon is postulated to include remembered exemplars of potentially every utterance of every word the speaker has ever produced or perceived, tagged with information about the linguistic and social contexts in which the forms occurred. Hence, memory alone provides complete information about the quantitative distribution of words and variable processes, including lexical frequency, and idiosyncrasy. Bybee proposes a gradual process of reduction and assimilation through repetition to explain positive correlations with frequency such as those demonstrated above.

2. Irregular past tense forms (*left*, *kept*, etc.) also show increased deletion with lexical frequency, suggesting they are also stored as lexical entries, as Pinker argues.

A grammar that operated on these lines can accurately generate many aspects of variation and change in considerable quantitative detail, and in addition to capturing frequency effects, it is capable of accounting for entrenchment of morphologically irregular forms, lenition and assimilation processes, and even socio-stylistic variation (because exemplars are tagged with social information). This is all accomplished by incorporating probabilistic information directly into linguistic representations. However, these virtues are achieved at a considerable modeling cost: the theory requires an essentially unlimited number of predictor variables (each exemplar), and implies great demands on the processing and storage capacity of the language faculty. The theory also appears to over-predict the effect of lexical frequency and the presence of lexical idiosyncrasy (every word has its own distinctive exemplar cloud), and to under-predict productivity, as in the production and perception of novel items for which the speaker has no exemplars.

The second alternative is the ‘variable rule’ model first articulated in Cedergren and Sankoff (1974). As we have noted, this paper makes the case that grammar is itself internally probabilistic. The authors also lay out a formalism to model such probabilistic grammatical operations. They adapt a conventional generative model by allowing rules, processes, and constraints to be associated with probability weightings.

This model straightforwardly permits frequency effects and other lexically specific constraints: lexical items that are involved in variable processes may be associated with individual probabilities of undergoing those processes. As with an exemplar model, these probabilities will be updated based on experience, but the variable rule model requires saving only the probability value in memory, not the entire exemplar cloud. It is thus more cognitively economical. The variable rule model also avoids the prediction issues mentioned above. Productivity is a central design feature, as in any generative grammar. Lexical idiosyncrasies and frequency effects are not intrinsic to the model. Constraint values are generalized across the entire lexicon, and will prevail wherever significant lexically specific quantitative patterns are absent, such as in most lower-frequency words (cf., Erker & Guy, 2012).

4.2 Lexical exceptions

A conventional assumption about the nature of grammar is that phonological processes apply uniformly across the lexicon, to all words that satisfy the relevant structural description. This is the synchronic version of the Neogrammarian hypothesis of exceptionless sound change. Nevertheless, it has long been recognized that some words behave idiosyncratically. This creates a problem for theoretical models that provide no internal mechanism for lexical exceptions to sound change

or phonological alternations; it also presents a challenge for the language learner. How are lexical exceptions represented in the grammar?

In studies of variation, it is not uncommon to discover that certain words systematically fail to conform to the general distribution or rates of occurrence of variants that prevail for most of the lexicon. Such words are quantitatively exceptional. A case in point is the word *and* in English, which is uttered without its final /d/ at an extremely high rate, well in excess of phonologically comparable words like *hand*, *band*. This suggests that it is a quantitative exception to the process of CSD. In the Guy *et al.* study (2008) of early New Zealand English, *and* showed 80% final stop deletion vs. 29% deletion for all other words in the corpus.

How might such a pattern be represented in the grammar? For lexical frequency, we have just argued that high frequency lexical items are associated with individual probabilities of undergoing particular phonological processes. But this strategy is not adequate for the lexical exception cases, because of the way they interact with other phonological constraints. As shown in Table 5, following context ordinarily has a powerful effect on CSD: for the general lexicon, deletion is over five times as likely in pre-consonantal contexts than pre-vocalic. But this constraint is much weaker for *and*, where pre-consonantal contexts are associated with only a 17% increase in deletion compared with pre-vocalic contexts.

Table 5. *and* in New Zealand English: constraint effects are weaker for lexical exceptions

Following	Other words		<i>and</i>	
	N	% del	N	% del
Context:				
__C	1339	58.3	315	87.9
__V	1477	10.4	182	75.3
Range:		47.9	>	12.6

These facts are difficult to generate using a model in which *and* is simply associated with a higher probability of undergoing CSD than other words. If the following context is an independent predictor that applies equally to all words, then its effect should be constant, regardless of whether a given word is more favorable to deletion than most other words. A more straightforward model is that exceptional words have multiple lexical entries – allomorphs – in the lexicon. In addition to its full lexical entry /ænd/, English *and* must have an additional allomorph that underlyingly lacks a final stop: /æn/ or /n/. In usage, speakers vary stochastically, or perhaps stylistically, between these allomorphs. When the latter allomorph is selected, the surface realization lacks a final /d/ regardless of what the following context is. The surface corpus will therefore contain a mixture of forms, some of which were subject to the deletion process and affected by its constraints, and others for which

deletion and its constraints were irrelevant. This produces the surface attenuation of the following context effect that we observe for exceptional *and*.

Similar cases of lexical exceptions have been studied in a number of variable processes in English, Spanish, and Portuguese. Thus, Hoffman (2004) finds that the discourse markers *entonces* 'so' and *pues* 'well' in Salvadoran Spanish have exceptionally high rates of final –s deletion, and also show weaker or nonsignificant effects of the constraints on deletion that are evident in the general vocabulary, such as syllabic stress and following context. Woods (2008) shows that the pronouns *I* and *my* are realized as monophthongal at an exceptionally high rate in Southern American English, and are weakly or insignificantly affected by constraints such as speech rate and following context. All such cases involve function words that undergo some phonological process at an exceptionally high rate. They are high frequency items, but frequency alone does not explain them; they differ significantly from other high frequency forms, notably by the attenuated effect of constraints on the process. The evidence suggests that such cases reflect lexicalization: the general phonological process has been encoded in the lexical entry as an allomorph. This would certainly be a reasonable conjecture for a language learner, faced with the kind of evidence we see in Table 5, and it is especially likely for function words, which are known diachronically to behave in exceptional ways.

5. Acquisition

Linguistic competence in a language must be acquired. Knowledge of a language is not an 'app' downloaded from a single source and hence uniform across all users. Rather, it is painstakingly constructed anew by each speaker, through observation, interaction, and use, by means of processes of association, deduction, inference, and generalization. So our questions are: how do these processes work in the face of variable input, and what kind of grammar do they yield? Are the cognitive skills that enable language acquisition tuned to respond to probabilistic input? Does normal engagement in language use lead the learner to a deterministic or a probabilistic grammar?

The data every child encounters will always display variability on a considerable scale. This presents the child with an inferential conundrum: does the variability reflect idiosyncrasy everywhere – words have lexically specific phonological, morphological, and syntactic properties, and speakers are unique and inconsistent? Or alternatively, are there generalizations to be made, and if so, what are the appropriate generalizations? In particular, is it even possible for learners to perceive and acquire quantitative patterns?

Yang (2016) elaborates a ‘variational model’ of language acquisition that gives a probabilistic formulation to the learner’s grammatical development:

The child has a statistical distribution over the space of possible grammars ... and it is this distribution that changes in response to linguistic data. As learning proceeds, the child will access the target grammar with increasing probability, while the non-target but linguistically possible grammars will still be used, albeit with decreasing probabilities. (Yang, 2016: p. 6)

In Yang’s view, children derive generalizations in accord with a function that evaluates productivity and processing cost in the pursuit of an efficient grammar. Initially, every word or structure may be treated as idiosyncratic and simply memorized, but patterns that are sufficiently general, with exceptions that do not exceed a certain limit, will be formulated as rules, permitting a more compact representation and reducing the inventory of forms that require idiosyncratic, ‘exceptional’ treatments. They thus pursue generalizations, regular patterns, unmarked forms, and default settings, but do so quantitatively not categorically, tracking probabilities not asterisks.

With this work Yang makes an important contribution to recognizing probabilistic processes in the mind of the child learner in the course of language acquisition. However, in his model, the child ultimately seeks deterministic rules, albeit ones that have exceptions, in modest, memorizable numbers. Language learning is probabilistic, but for Yang, the grammars to be learned are not. We argue that the probabilistic cognitive processing Yang recognizes in acquisition is operative in the grammar itself – in the child’s linguistic competence.

The learnability of probabilistic distributions is, as we have noted, well-established in psychological research. Linguistic studies of variation in early language acquisition demonstrate that children converge their productions of linguistic variables on the quantitative patterns that they hear in the input. For example, Smith, Durham, and Fortune (2007, 2009) showed children acquiring phonological and syntactic variables and their associated probabilistic constraints at a very early age – by 3;0 – and acquiring sociolinguistic constraints soon thereafter in ways that reflect caregiver and community behavior. But this convergence is limited by the developmental state of the child’s mental grammar. One notable example occurs in the acquisition of the morphological constraint on English coronal stop deletion discussed above in Section 3. The pattern seen in Table 2 is replicated by children at a fairly early age for the monomorphemic and regular past tense words, with higher deletion in the former than in the latter. However, Guy and Boyd (1990) have shown that the intermediate deletion levels found for the irregular past tense forms are not acquired until late adolescence or young adulthood.

Consequently, the productions of young children match their parents' deletion rates very closely for monomorphemes and regular past verbs, but not for irregulars in the *lost*, *kept*, *told* class. This is illustrated in Figure 4 from Roberts's (1994) study of children and parents in Philadelphia. Guy and Boyd argue that this represents a developmental stage in the child's grammatical analysis of verbal morphology. In the early grammar, these irregular forms are treated as unanalyzed wholes, suppletive past tense forms analogous to *were* and *thought*. Hence, for CSD, they are treated as monomorphemes. But with increasing linguistic maturity speakers come to a morphological analysis in which such words are derivationally related to their roots, so the final alveolar stop acquires morphemic status, and consequently, a lower rate of deletion.

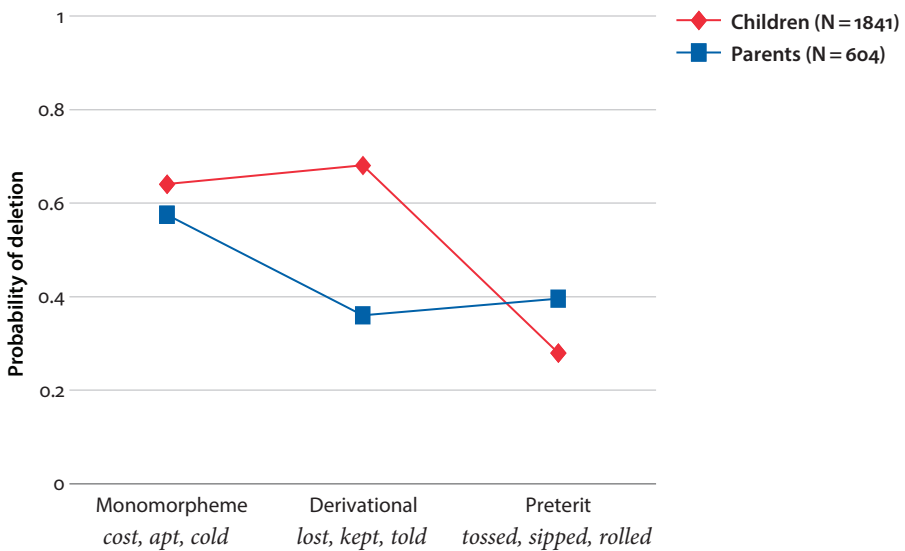


Figure 4. Probability matching of 16 children, 3–5 years old, Philadelphia. From Roberts (1994), Figure 7.4

These facts indicate that probabilistic information is intimately embedded in the grammar. Acquiring the grammar depends on attending to and acquiring the probability distributions of variants, and inducing a grammatical structure that allows the learner to reproduce them. The probability limits are 0 and 1, and these are the values that will be acquired for categorical phenomena, but learners attend to, and learn, phenomena with probabilities that lie between 0 and 1.

6. Conclusions

Variation is manifestly present in language use. Since the grammars that linguists seek to describe are the mental objects that permit speakers to produce language, they must operate so as to generate variation. A model of grammar that fails to do so also fails to achieve the most elementary level of observational adequacy. So the task for linguistics is to identify the grammatical architecture that produces linguistic variability.

This task may be approached initially from the standpoint of the language learner. Faced with great diversity in the input, the learner observes quantitative patterns and formulates statistical inferences about the shape of the grammar that can generate those patterns. The grammar is constrained: it has to be learnable, it has to be productive – capable of perceiving and generating novel utterances, and it has to be human – i.e., consistent with human cognitive capacities and with the possibilities permitted by the language faculty or Universal Grammar. The easiest patterns to learn are those that are maximally or minimally likely – occurring with probabilities of 1 or 0. But the processes of learning, and the need to arrive at a grammar that achieves convergence with the input, require the learner to attend to the whole observable range of probabilities. Indeed, all the aspects of language incorporate probabilistic properties: in the input, in perception, in inference and acquisition, in the form of the grammar and lexicon, and in production.

The construction of linguistic theories necessarily parallels what the language user does. Linguists have always encountered variable, messy data, and always sought patterns, rules, generalizations. Where theory has gone wrong is when it privileges the regularities to the neglect of the messiness. We cannot discover adequate theories of language if we ignore quantitative patterns in the input, restrict the internal workings of the grammar to categorical, deterministic processes, and generate only categorical patterns in the output. Rather, we must continue to emulate the children, by embracing linguistic variation, and incorporating it into our theories.

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Discovering structure

Person and accessibility

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We probe grammatical person differences comparing 3SG with 1SG in actual language use, utilizing subject pronoun expression in Spanish. We reconfigure the familiar constraint of accessibility to distinguish between clause linking (prosodic and syntactic connectedness) in coreferential contexts and distance from the previous mention (intervening clauses) in non-coreferential contexts. This refinement reveals that accessibility impacts 1SG earlier than 3SG, for which the pronoun rate rises more slowly with increasing distance. At the same time, for pronominal and unexpressed subjects, a greater proportion of 3SG than 1SG occurs in coreferential contexts. 3SG pronominal and unexpressed subjects thus tend to cluster more closely. By these differences in the workings of accessibility and in contextual distribution, unlike speech act participant 1SG, 3SG is a *transient person* in discourse.

Keywords: third person, accessibility, subject expression, pronouns, lexical subjects, variable context, prosody, coordination, contextual distribution, Spanish

1. Grammatical person in cross-linguistic perspective

Linguistic structure is not a given but “must be discovered through analysis,” as Ricardo Otheguy has urged (2002: p. 400). Grammatical categories espoused by linguists do not always coincide with actual usage, and though the category of person features in accounts of subject pronoun expression, its effects are not well understood. Admittedly, variable use of subject pronouns in Spanish has been so widely analyzed that we might wonder if there is anything more to be discovered about it. After all, “multiple studies ... across communities, across settings, and across the lifespan reveal the very consistent nature of structured variation” in subject pronoun expression (Carvalho, Orozco, & Shin, 2015: p. xxii).

Grammatical person is consistently found as a probabilistic constraint on subject expression, often the strongest, as in Ricardo Otheguy’s own analyses of Spanish

in New York City (Otheguy & Zentella, 2012; Otheguy, Zentella, & Livert, 2007). Person effects are reported for a range of other languages as well, for example, Arabic (Owens, Dodsworth, & Kohn, 2013: p. 268; Parkinson, 1987: p. 356), Auslan (McKee, Schembri, McKee, & Johnston, 2011: p. 388), Bislama (Meyerhoff, 2009: p. 311), Cantonese and Russian (Nagy, Aghdasi, Denis, & Motut, 2011: pp. 141–142), and Turkish (Koban, 2011: p. 362). Here we try to explain this effect.

A key distinction often made is between first and second person in contrast with (animate) third person, said to hold cross-linguistically. For example, in languages with split ergative marking based on person, the most common configuration is one in which first and second persons are treated differently from other NPs, including (animate) third person (e.g., Delancey, 1981: p. 628). In languages with switch reference marking, this is sometimes applied only on third person verbs (e.g., Haiman & Munro, 1983: p. xi). And in agreement patterns, zero marking is more likely on the third person than on first and second (e.g., Bybee, 1985: p. 53).

The status of third person follows from the “independent role that the distinction between speech act participants and third person referents plays in grammar and discourse” (Dahl, 2008: p. 143). Indeed, much cited is Benveniste’s characterization of the third person as a “non-person,” “never being reflective of the instance of discourse” (Benveniste, 1971: pp. 221–222).

For an exploration of subject person in spontaneous speech, the most pertinent difference is that between first and third person singular (1SG and 3SG) human subjects, as these make up the bulk of the data. Together, they account for approximately two thirds of most Spanish datasets (e.g., Claes, 2011: p. 199; Erker & Guy, 2012: p. 540; Lastra & Butragueño, 2015: p. 43; Otheguy et al., 2007: p. 790). In English conversational data also, 1SG and 3SG are the most frequent human subjects (Scheibman, 2001: pp. 68, 80).

The most widely reported difference between these two persons is that 1SG subjects favor pronominal expression more so than 3SG (e.g., Bayley & Pease-Alvarez, 1997: p. 363; Cameron, 1992: p. 168; Enríquez, 1984: p. 350; Morales, 1986: pp. 93–96; Orozco, 2015: p. 27; Posio, 2015: p. 72; Ranson, 1991: p. 139). The same pattern has been observed not only in the typologically similar language Brazilian Portuguese (Silveira, 2011: p. 48), but also in Mandarin Chinese (Jia & Bayley, 2002: p. 110), and even in English, despite the rarity of unexpressed subjects in this “non-null subject” language (Torres Cacoulios & Travis in press). Are subject pronoun rate differences manifestations of true differences between grammatical persons in the functions of subject forms?

Here we seek to better understand the 3SG vs. 1SG person difference by converting the question into one with a quantitative answer. Using prosodically transcribed spontaneous speech data (Section 2), we focus our attention on the role of

accessibility in subject pronoun expression. Once we break down accessibility to consider both linking across clauses in coreferential contexts and distance from previous mention in non-coreferential contexts (Section 3), differences between 3SG and 1SG emerge. One lies in the implementation of the effect (Section 4). Another is found in the distribution of 3SG vs. 1SG subjects in relation to the degree of accessibility of their referents, as for 3SG, the distribution of pronominal and unexpressed subjects is impacted by the availability of lexical forms (Section 5). Contextual distribution of the data thus contributes to the overall rate of variants. In light of both contextual effects and contextual distributions related to accessibility, we revisit the variable context for subject expression to establish the alternative grammatical ways of “saying the same thing” (cf., Labov, 1972: p. 72) (Section 6).

2. Spontaneous speech and prosodically transcribed data

To explore grammatical person differences in language use, we examine 1SG and 3SG subjects in spontaneous speech data. The generalizability of the analysis is bolstered by the examination of two corpora, representing different dialects and genres, and providing close to 8,000 tokens for observation. The New Mexico Spanish-English Bilingual corpus (NMSEB) was collected in the years 2010–2011, recording 40 *Nuevomexicano* speakers from northern New Mexico, USA (Torres Cacoullós & Travis, 2018, Chapters 2 & 3). NMSEB consists of sociolinguistic interviews conducted with extended family members and acquaintances, totaling approximately 30 hours, or 300,000 words of speech. The Corpus of Conversational Colombian Spanish (CCCS) consists of 30 conversations between close friends and intimate family, such as spouses, recorded during 1997–2004 (cf., Travis, 2005: pp. 9–25). It comprises a total of approximately 100,000 words, or nine hours of speech, from 37 speakers.

Initially extracted were all tokens of finite verbs with 1SG and 3SG (human, specific) subjects that are unexpressed or realized pronominally (*yo* ‘I,’ *él* ‘he,’ *ella* ‘she’). Outside the envelope of variation as defined are postverbal subject pronouns (Section 6). Contexts with negligible variation are also set aside (Otheguy et al., 2007: p. 776; Torres Cacoullós & Travis, 2018: pp. 119–121). This includes non-human and nonspecific human referents, because they are rarely realized as personal pronouns *él*, *ella* (in these data, never), and *wh*-interrogatives, in which subjects are either unexpressed or in postverbal position. Instances that cannot be reliably analyzed, such as truncated verbs, are also excluded.

This leaves 5,571 instances of variable pronoun expression in NMSEB, and 2,802 in CCCS.¹ A first finding, consistent with other studies, is that the subject pronoun rate is higher for 1sg than for 3sg in the two datasets, at 29% vs. 18% in NMSEB (N = 3,296 and 2,275), and 50% vs. 32% in CCCS (N = 1,389 and 1,413). Notice that, for both persons, rates are higher in CCCS than in NMSEB. It is through the analyses that we are able to test the role of genre in this blanket difference, and assess whether an overall rate difference is tantamount to a linguistically significant difference in the structure of variation patterns (cf., Travis, 2007).

We chose these corpora because they are prosodically transcribed following the same precise protocols. Prosodically based transcription sharpens the analysis of linguistic structure, as syntax tends to align with prosody. A manifestation of the syntax-prosody connection is that words in the same prosodic unit tend to have a tighter syntactic relationship than material across prosodic units (Croft, 1995: pp. 849–864). Relevant to the study of variable subject expression is that subject pronouns tend to occur in the same prosodic unit as the verb with which they occur. Prosody thus provides an objective criterion for determining the syntactic status (as bonafide subjects or not) of pronouns that are detached from a verb or that appear to be dislocated (Torres Cacoulios & Travis, 2018: p. 55).

In the transcription, the speech stream is segmented into prosodic units, here the Intonation Unit (IU) – “a stretch of speech uttered under a single, coherent intonation contour” (Du Bois, Schuetze-Coburn, Cumming, & Paolino, 1993: p. 47). Each IU is transcribed on a distinct line, and is followed by punctuation representing “transitional continuity,” i.e., the prosodic contour with which it ends. In Example (1), the commas marking the first two IUs indicate “continuing” intonation, while the period of the third IU indicates “final” intonation (a fall to low pitch) (Du Bois et al., 1993: p. 53). The three clauses presented in a sequence of IUs in (1) form a “prosodic sentence” (Chafe, 1994: pp. 139–140), and illustrate the role of prosody in the linking of clauses.

- | | |
|--|---|
| (1) Ivette: <i>Ella venía pa' mi casa,
parqueaba su carro y luego,
nos íbamos las dos con el
~Rubencito.</i> | ‘She would come to my house,
she would park her car and then,
we would both go with Ruben.’ |
| | (NMSEB 06 El Túnico, 52:42–52:47) ² |

1. The CCCS 1sg dataset is from a portion of the corpus (see Travis & Torres Cacoulios 2012: pp. 712–713); 3sg was extracted from all transcripts. While this skews the relative frequency of the 1sg and 3sg subjects, it does not affect the linguistic conditioning of the variability within each person (the contribution of linguistic context to the selection of a pronominal over an unexpressed subject).

2. The information in parentheses following examples indicates the corpus, recording number and name, and identifiers of the lines reproduced (beginning–ending time stamps for NMSEB

...*(1.0) y no la dejaba la* ...*(1.0) and Bobbie wouldn't let her.*
~Bobbie.
yo no sé qué estarían I don't know what they were doing there.
haciendo allá.
 (NMSEB 13 La Acequia, 18:16–18:28)

As seen in Figure 1, in both datasets, the subject pronoun rate is lowest in coreferential contexts (with zero intervening clauses, i.e., where the target subject is coreferential with the subject of the immediately preceding clause), and highest at ten or more clauses. In between there are step-by-step increases at one intervening clause, and then at two–nine intervening clauses.³ Note that while the pronoun rate in CCCS is consistently above that of NMSEB, the key point here is that the effect is the same in both datasets. The conclusion is that distance from the previous mention is pertinent to the Spanish subject expression, beyond binary switch vs. same reference with respect to the subject of the immediately preceding clause.

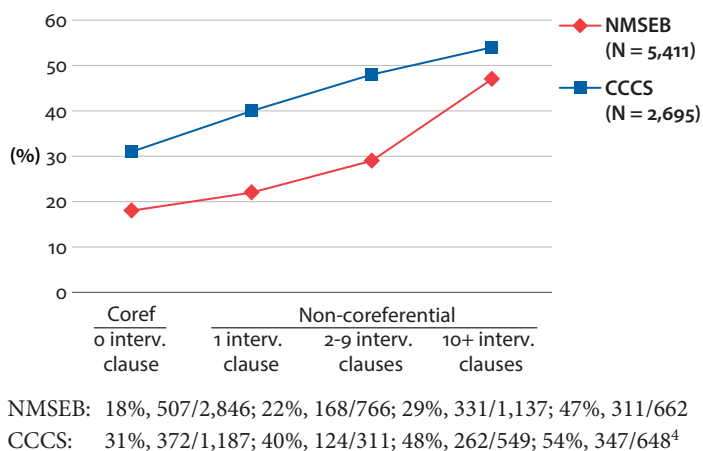


Figure 1. Rate of pronoun expression according to distance from previous mention

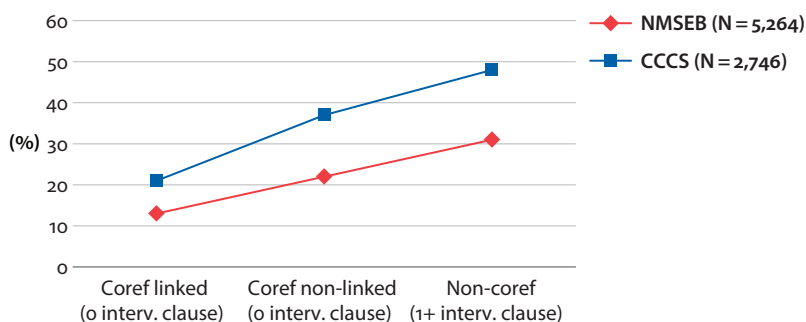
3. The difference between zero and one clause (coreferential vs. local switch reference contexts) turns out to be smaller in the absence than in the presence of intervening human subjects. How the accessibility measure of human switch reference (Travis & Torres Cacoullas 2012: p. 727–729) intersects with that of linking (see Section 3.2) remains to be investigated.

4. Due to analysis-specific exclusions, token numbers in the cross-tabulations vary; excluded here are cases where it was not possible to determine the exact number of intervening clauses, for example, due to unclear speech. The overall rise from 0 to 10 + intervening clauses is significant for both NMSEB ($F(3, 5,407) = 93.37, p < .001$) and CCCS ($F(3, 2,691) = 33.88, p < .001$). (Statistical results presented are based on Tukey's post hoc tests conducted on separate one-way ANOVAs fit to each dataset.)

- (8) Julia: ... *Entonces,* '... So,
 .. *Que Ø quería ir a trabajar* .. (he) wanted to go and work at the
al restaurante, restaurant,
entonces, so,
supuestamente, supposedly,
en un mes, in one month,
Ø iba a ganar un millón y (he) was going to earn over a million
pico, ((pesos)),
 (CCCS 03 Familia, 688–693)
- (9) Fabiola: *Ø tenía mucho cabello,* '(He) had a lot of hair,
o no? or not?'
 Molly: ... (1.0) *Ø tenía poco,* '... (1.0) (he) had little,
 .. *no no muy muchote no.* .. no not a whole lot no.'
 (NMSEB 09 La Salvia, 2:00–2:04)

In the proposed refinement, subject continuity comprises a combination of semantic and structural features, with the semantic link of coreferentiality being broken into two categories based on the presence or absence of structural linking. The hypothesis is that, within coreferential contexts, unexpressed subjects are favored more in clauses that are structurally linked – syntactically and/or prosodically – than in those that have no such linking.

Figure 2 shows that structural linking does make a difference: a pronoun is least likely if the subject is both coreferential with the preceding subject and is prosodically and/or syntactically linked to it; it is most likely in non-coreferential contexts. Merely coreferential contexts, in the absence of structural linking, are intermediate. Again, though the rate is higher overall in CCCS, this holds for both datasets.



NMSEB: 13%, 178/1,384; 22%, 270/1,238; 31%, 829/2,642

CCCS: 21%, 98/458; 37%, 261/700; 48%, 767/1,588⁵

Figure 2. Rate of pronoun expression according to structural linking between clauses – prosodic and syntactic

5. The rise from coreferential linked to non-coreferential is significant for both datasets (NMSEB $F(2, 5,261) = 90.33, p < .001$; CCCS $F(2, 2,743) = 58.17, p < .001$).

It is important to recognize that the linking effect is not a surrogate for clause type effects (whereby coordinate clauses and, in some studies, subordinate clauses disfavor pronominal subjects).⁶ First of all, there can be syntactic linking (via a conjunction) without prosodic linking, as in (7). Furthermore, prosodic linking also applies to non-coordinate main clauses (as in (5)). Indeed, one-third of main clause structurally linked tokens are linked via prosody alone (312/946 in NMSEB and 102/291 in CCCS), and the rate of pronoun expression is equally as low as that in clauses with a coordinating or subordinating conjunction (at 15% in NMSEB and 21% in CCCS).

4. The workings of accessibility for 3SG vs. 1SG

It makes sense that there would be a true difference between first and third person in relation to their accessibility. While a first person referent is thought to be always accessible as a discourse participant, the same is not so of third person referents which are typically made accessible through a mention in the discourse (Chafe, 1994: pp. 78–79; Dahl, 2000: pp. 64–66; Prince, 1981). But, in fact, both first and third person are conditioned by accessibility.

Figure 3 shows, in both datasets, the rate of expression of 1SG and 3SG subjects by linking and distance. The rate of expression is confirmed to be higher for 1SG than for 3SG at every degree of distance. Nevertheless, the similarities are striking. On the one hand, the same linking effect described in the previous section applies to both persons. This is seen in two ways. First, the subject pronoun rate increases from linked to non-linked coreferential contexts by a factor of approximately 1.5 to 2. Second, there is little difference between non-linked coreferential contexts and non-coreferential contexts with a distance of one intervening clause (increasing at most by a factor of 1.1). Thus, structural linking is relevant in the application of accessibility for both persons. On the other hand, also apparent for both persons is a distance effect, with the rate of subject expression greater when there are ten or more intervening clauses than when there is just one intervening clause from the previous mention. In sum, for both persons, the effects of structural linking and of distance are clearly apparent.

For the first person, the application of the accessibility effect (i.e., the use of pronouns for less accessible referents) has been interpreted in the sense of speakers “bringing the ideas of themselves back into the active consciousness of the listeners” (Chafe, 1994: p. 87). For the third person, it is often assumed that, as third

6. Coordinate clauses have a lower pronoun rate than non-coordinate main clauses; reports on subordinate clauses are inconsistent (see, e.g., Enriquez 1984: p. 257; Orozco 2015: p. 22; Otheguy & Zentella 2012: p. 164; Shin 2014: p. 211; Travis 2007: p. 115).

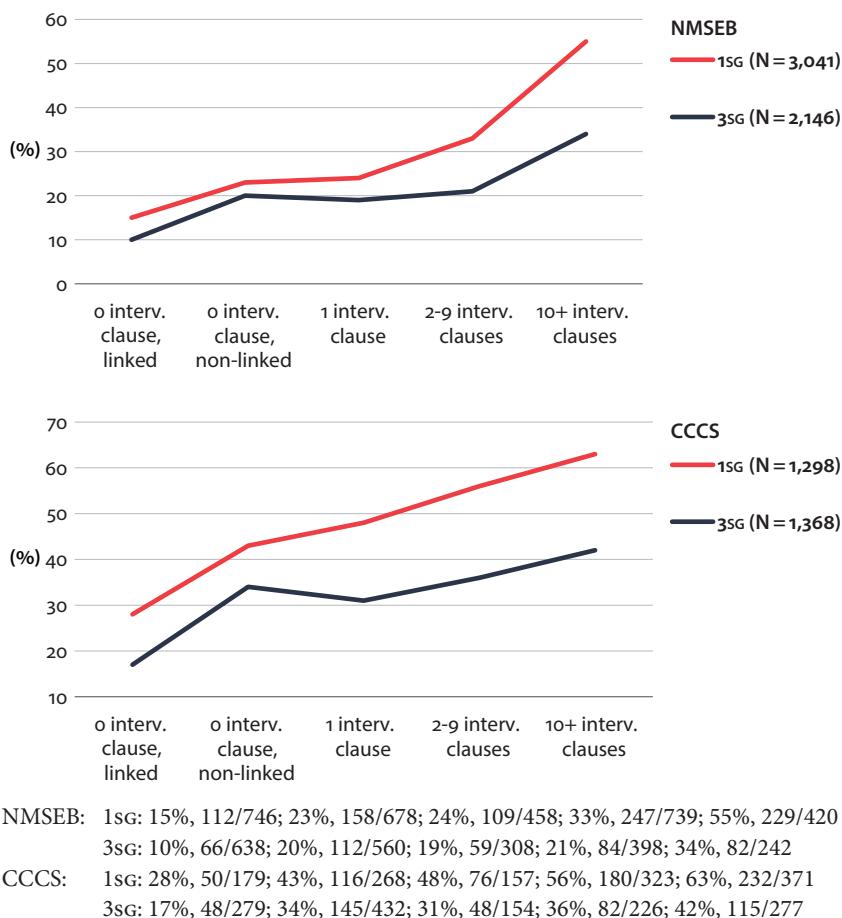


Figure 3. Rate of pronoun expression according to structural linking and distance, for 1SG vs. 3SG

person referents are outside “the instance of discourse” (as Benveniste (1971) put it) and have to be introduced, they are “by default of rather low accessibility” (Ariel, 2000: p. 218). Thus, for Spanish, it has been predicted that “contextual salience or discourse-induced accessibility should play a smaller role [for first person] than in the case of anaphoric third person” (Posio, 2013: p. 257).

Within the variable context for subject pronoun expression, though both persons are sensitive to accessibility, they do indeed differ – but not quite as predicted. Once we refine accessibility by the discerning measures of clause linking and distance, we are able to ascertain that the difference lies in the way the effect applies. The effect begins to impact pronoun rate earlier for 1SG, actually, than for 3SG. This is depicted in Figure 3, where, for 1SG, we observe a rise from one through to

Figure 4 gives the distribution of subject forms with this three-way breakdown for 3sg, corresponding to the two-way breakdown for 1sg (here, as above, for human, specific preverbal subjects).⁸ Pronominal tokens still make up proportionally twice as much of the 1sg data (29%) than of the expanded 3sg data (15%) once we add in lexical subjects (17%). However, the proportion of unexpressed subjects is now virtually the same for the two persons: 71% for 1sg, compared with 68% for 3sg. Thus, the widely reported higher rate of unexpressed 3sg subjects is at least in part due to the fact that there exists a third option that, at first blush, can be taken to compete with pronominal and unexpressed subjects (cf., Gudmestad, House & Geeslin, 2013; Posio, 2015: p. 72).



1SG: 71% unexpressed, 29% pronoun (N = 3,296)

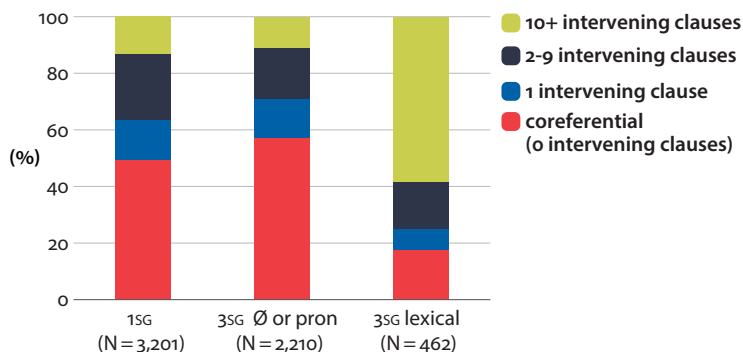
3SG: 68% unexpressed, 15% pronoun, 17% lexical (N = 2,737)

Figure 4. Distribution of 1sg and 3sg subject forms (NMSEB)

What impact might the use of lexical subjects have on the workings of accessibility in the choice between 3sg pronominal and unexpressed subjects? Observe the distribution of subject forms according to distance from the previous mention, in Figure 5. The first two bars give the breakdown for 1sg and 3sg subjects, considering pronominal and unexpressed forms; the third bar gives the breakdown for 3sg lexical subjects. Comparing the first two bars, we see that pronominal and unexpressed 3sg subjects tend to occur closer together than 1sg subjects, appearing

8. Comparison with CCCS is not available, because lexical subjects were not extracted.

proportionally more in coreferential contexts and less with a previous mention at two or more intervening clauses (at one intervening clause, the proportion is the same for both persons).⁹ 3SG lexical subjects, in turn, differ from both 3SG pronominal and unexpressed and 1sg, as they occur over half the time at a distance of ten or more intervening clauses from their previous mention.¹⁰



1sg: 49% (N = 1,584), 14% (458), 23% (739), 13% (420)

3sg Ø or pron: 57% (1,262), 14% (308), 18% (398), 11% (242)

3sg lexical: 18% (82), 7% (34), 17% (77), 58% (269)

Figure 5. Distribution of 1sg and 3sg subject forms according to distance from previous mention (Ø and pronominal vs. lexical) (NMSEB)

Full NPs are the favored 3sg form to introduce a new referent, or to return to a prior referent that hasn't been mentioned for some time (cf., Dumont, 2016: p. 84). This we confirm in Figure 6, which gives the proportion of Ø, pronominal, and lexical subjects at different degrees of distance, for 3sg. At a distance of 0 intervening clauses, unexpressed subjects predominate (80% of subjects in coreferential contexts are unexpressed). At the other end, at a distance of 10+, i.e., for referents

9. Similarly in CCCS, the distribution of Ø and pronominal subjects according to distance from previous mention indicates that 3sg subjects occur closer together than 1sg subjects (see token counts in Figure 3).

10. In comparing 3sg lexical forms vs. 3sg pronouns and Ø (third and second bars), a generalized linear regression with a logit link function demonstrates that lexical forms are significantly favored more at greater degrees of distance between mentions (0 vs. 1: $z = -2.48$, $p < .05$; 0 vs. 2-9: $z = -6.47$, $p < .001$; 0 vs. 10: $z = -19.67$, $p < .001$). In comparing 1sg pronouns and Ø vs. 3sg pronouns and Ø (first and second bar), 1sg pronouns and Ø are favored more the greater the distance (0 vs. 1: $z = -2.05$, $p = .05$; 0 vs. 2-9: $z = -5.38$, $p < .001$; 0 vs. 10: $z = -3.64$, $p < .001$).

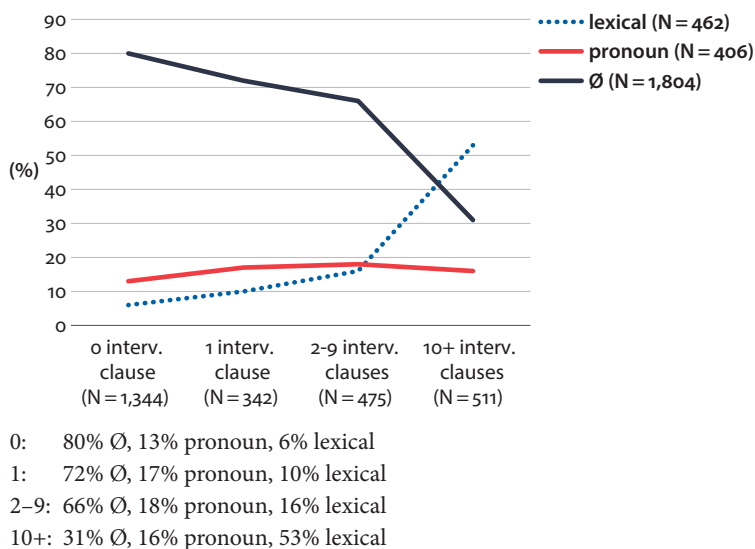


Figure 6. Proportions of 3SG subject forms according to distance (Ø vs. pronominal vs. lexical) (NMSEB)

without a previous mention in subject position within the preceding ten clauses, a lexical form is much more likely (53%) than either a pronominal (16%) or unexpressed (31%) subject.¹¹

5.2 The clustering of 3SG pronominal and unexpressed subjects

Precisely because speakers tend to use a lexical form for less accessible 3SG subjects, the occurrence of unexpressed and pronominal targets is depressed at greater degrees of distance from the previous mention. Instead, as we saw in Figure 5, 3SG *personal pronoun and unexpressed* subjects tend to *cluster* together (occur with no intervening clauses from their previous mention) more than 1SG subjects do.

The greater clustering of 3SG (unexpressed and pronominal) than 1SG subjects can be explained by the deictic properties of these subject persons: as external parties, 3SG subject referents are introduced, continue in the discourse briefly as topics of discussion, and are then abandoned (as in (10) above), while 1SG subject referents can be referred to at any time in the conversation without introduction.

11. The lexical subject proportion in NMSEB is one-tenth (11%, 36/322) at a distance of 2–4 intervening clauses between the target subject and previous mention but reaches a proportion of one-fourth (27%, 41/153) at a distance of 5–9 intervening clauses. How far back to look for a previous mention in subject expression studies remains an empirical question.

Dahl similarly finds greater “clustering” for third person pronouns than for first and second person pronouns in Swedish conversation and remarks that “once you have started talking about a third person referent, the chance that you will continue doing so also in the following clause is much higher than in the case of egophoric [1SG and 2SG] referents, other things being equal” (2000: p. 65). In other words, “the typical behavior of third person referents is to stay on the scene once they have been introduced while egophoric referents pop in and out all the time” (2000: p. 66). As Dahl (2000: p. 66) points out, 3SG clustering may be part of the explanation for zero-marked 3SG agreement (alongside frequency-based explanations, see Bickel, Witzlack-Makarevich, Zakharko, & Iemmolo, 2015: p. 47).

We have verified here that the person difference is found in the greater proportion of 3SG tokens occurring in coreferential contexts (see Figure 5). In other words, the “clustering” of 3SG is indeed the higher proportion of occurrences in adjacent clauses. Consistent with this trend is that a larger proportion of the 3SG than 1SG data occur in structurally linked contexts (see token counts in Figure 3). The clustering of 3SG is thus quite tight, and short lived in discourse. In this sense, we can characterize 3SG as a *transient* person.

The contextual distributions contribute to the higher rate of pronouns with 1SG subjects, since 3SG (unexpressed and pronominal) subjects occur proportionally more often in coreferential contexts which, by the effect of subject continuity, disfavor pronominal subjects. Contextual distribution goes a long way to explaining pronoun rate differences by grammatical number as well. One consistent finding for Spanish is that plural subjects have a lower rate of pronominal vs. unexpressed subjects (e.g., Bentivoglio, 1987: p. 36; Otheguy et al., 2007: p. 791). This result is a byproduct of the greater propensity of plural than singular subjects to occur in the context of a previous partially coreferential mention (Cameron, 1995: p. 21). Contextual distribution is typically controlled in lab-based studies and often neglected in corpus studies focused on significance of predictor effects. But the lesson here is that contextual distributions can illuminate quantitative usage patterns.

5.3 Contextual distribution and genre

The same clustering of 3SG in the NMSEB sociolinguistic interviews is evident in the CCCS conversations among family and friends. Where there is a difference between the corpora is in aggregate data distribution, with proportionally fewer CCCS subjects overall in coreferential contexts (36% and 52% for 1SG and 3SG respectively vs. 49% and 57% for NMSEB) and more at greater degrees of distance of 10 + clauses (28% and 20% for 1SG and 3SG respectively, about double the proportions in NMSEB, at 13% and 11%) (see Figure 5 and Footnote 9).

The difference is attributable to genre: more dialogic interaction involves dense switching back and forth between interlocutors.¹² There is *less subject continuity* – seen in the lower proportion of clauses in which the subject is coreferential with the immediately preceding clause – and *more shifting of topics*, seen in the greater proportion of tokens in non-coreferential contexts at precisely longer distances from previous mention (43%, 648/1,508, of all non-coreferential subjects in CCCS occur at a distance of 10 or more intervening clauses, compared with just 26% (662/2,565) in NMSEB, $p < .001$). Note that the genre difference lies in data distribution rather than in conditioning of variant choice, or distinct grammars (as shown in Travis, 2007; Travis & Lindstrom, 2016). More generally, contextual distributions of the data may give rise to aggregate rate differences that may in turn mask similarities in the conditioning of variant choice.

In sum, grammatical person differences in rates of pronominal vs. unexpressed subjects are in part attributable to the availability of 3SG full NPs. Evident are different distributions according to contexts impinging on subject expression, with 1SG subjects tending to occur more than 3SG subjects (setting aside full NPs) in environments that are propitious to subject pronouns, namely in non-coreferential contexts.¹³ In contrast, 3SG subjects (again setting aside full NPs) tend to cluster together (Figure 5). Beyond contextual distributions, accessibility via previous mention has minimal impact at low degrees of distance for 3sg, such that the rate of subject pronouns remains comparatively flat (here, up to 10 clauses from the previous mention) (Figure 3).

12. In Figure 3, there is an increase in the subject pronoun rate between non-linked coreferential contexts (0 intervening clauses) and 1 intervening clause for 1sg but not for 3sg in CCCS. This is partly attributable to immediately preceding coreferential-subject clauses produced by the interlocutor (by definition, not prosodically linked), which account for 19% and 30% of 1sg and 3sg coreferential tokens, respectively, in the more dialogic CCCS (compared with 11% and 13% only, in NMSEB) (on interlocutor-produced previous mentions, see Torres Cacoullos & Travis 2018: p. 87).

13. The second contextual distribution difference between the persons is that 1sg, but not 3sg, subjects are associated with cognition verbs (Bentivoglio 1987: p. 51; Torres Cacoullos & Travis 2018: p. 102). In Spanish, separate regression analyses by person confirm that semantic class has a significant impact for 1sg – cognition verbs favoring pronominal subjects (Travis & Torres Cacoullos 2012: p. 734–742), but is “not relevant” for 3sg (Shin 2014: p. 325).

6. Inside and outside the variable context

Contextual distributions of unexpressed, pronominal, and lexical forms with respect to distance give a first indication that lexical subjects are the odd man out: lexical subjects are a very minor player when the previous mention occurs at a distance of under ten clauses, but at ten clauses, they are a robust contender, in fact, the preferred variant (Figure 5 and Figure 6). We can say, then, that *pronominal and unexpressed subjects are grammatical means of referring to an accessible subject*. This is the definition of the variable context for subject expression. As full NPs do something else, they are outside this envelope of variation.

True, subject pronouns are taken to be semantically distinct from unexpressed subjects, being used to mark “contrast,” “emphasis” or “focus” (e.g., Chafe, 1994: p. 37; Payne, 1997: p. 43). Such functions have been widely claimed not only for Spanish (e.g., Davidson, 1996; Posio, 2013; Serrano, 2014) and other Romance languages (Mayol, 2010), but for a range of languages in which subject expression is variable, including Finnish (Helasvuo, 2014: p. 454), Japanese (Lee & Yonezawa, 2008: pp. 741–739), and Javanese (Ewing, 2014: pp. 55–56).

In treating pronominal and unexpressed subjects as competing variants of the same linguistic variable, we do not deny that meaning differences may come into play in some contexts. Studies that have operationally tested for contrast in everyday speech indeed report a favoring of pronouns in particular constructions with contrastive elements (such as *sólo* ‘only’); however, contrastive contexts arise so rarely that they only account for a minor portion of the data (e.g., Bentivoglio, 1987: pp. 46–48; Paredes Silva, 1993: pp. 41–43; Travis & Torres Cacoullós, 2012: pp. 714–723). Thus, rather than assume that potential differences in meaning drive the choice between forms in every given instance, we work on the basis of the hypothesis that differences in grammatical function can be neutralized in discourse (Sankoff, 1988: p. 153). Neutralization-in-discourse is a requirement for (most) language change, since for one form to take over from another there must be a period of overlap in use. It is also the hypothesis for stable variation as in the case of Spanish subject expression; the antithesis, that grammatical alternatives necessarily always reflect communicative differences, must also remain a hypothesis (Sankoff, 1988: p. 154).

The neutralization-in-discourse hypothesis permits us to circumscribe a variable context, or locus of variability, which in turn permits us to state differences between the variants. If linguistic forms tend to co-occur with contextual elements harmonious with their meaning, forms with different meanings should be preferred in different (sub)contexts (Aaron & Torres Cacoullós, 2005: p. 615; Poplack, 2001: p. 405). Such differences in how the variants are deployed within the variable context are manifested in the direction of effect of conditioning factors – contextual

elements probabilistically constraining the choice – favoring one variant and disfavoring the other.

Forms whose use is affected by different conditioning factors do not belong to the same variable context. Consider postverbal subject pronouns, illustrated in (11). These are properly outside the variable context for subject pronoun expression (e.g., Cameron, 1992: p. 116). The reason is that the subject pronoun position is sensitive to different contextual features from those affecting expression. Postverbal placement of the subject pronoun is strongly favored in the presence of preverbal elements (adverbs, objects) in the clause and in the prosodic unit (Benevento & Dietrich, 2015: p. 415; Silva-Corvalán, 1982: p. 113), features that are not, as far as we know, relevant to subject expression.

- (11) Mariana: *puras mujercitas tengo yo*. ^I_{-POST} have all girls’
(NMSEB 19 School Bus, 40:05–40:07)

Confirmation that lexical subjects are not part of the same envelope of variation as unexpressed and pronominal subjects is that their inclusion provokes a contrary accessibility effect. Returning to Figure 6 above, which depicts proportions of 3SG subject forms by distance, notice that the proportion of unexpressed 3SG subjects gradually diminishes from zero to nine intervening clauses from the previous mention, and then radically drops once the distance reaches ten clauses. The proportion of pronouns, however, hardly changes according to distance, presenting a flat line at all degrees of distance (hovering around 16%); the difference is that, at 0–9 intervening clauses, unexpressed subjects predominate, while at 10+, lexical forms do. The steady proportion of pronouns regardless of distance from the previous mention – when lexical subjects are counted – is consistent with the report that when 3SG lexical, pronominal, and unexpressed subject forms are all considered in a single analysis, switch reference is pertinent for the choice of full NPs, but not personal pronouns, over unexpressed subjects (Gudmestad et al., 2013: p. 287).

Figure 7 now depicts subject pronoun rates as a choice over unexpressed subjects for 1SG and for 3SG (in the solid lines, seen previously in Figure 3), and over a lexical form for 3SG pronouns (in the dotted line). In opposition to unexpressed subjects, the 3SG pronoun rate rises with distance, as it does for 1sg; in opposition to a lexical form, it declines. What Figure 7 clearly confirms, then, is that the relevant comparison across the different grammatical persons is pronoun vs. \emptyset : within this variable context, 1SG and 3SG respond similarly to accessibility. At the same time, by establishing refined measures of subject continuity, a difference has been discovered in the workings of accessibility, namely the earlier and sharper pronoun rise with increasing distance for 1SG as compared with 3SG (Section 4).

Ricardo Otheguy has called for “handling data without relying on antecedently given formal or relational structure” (2002: p. 401). What are unexpressed subjects

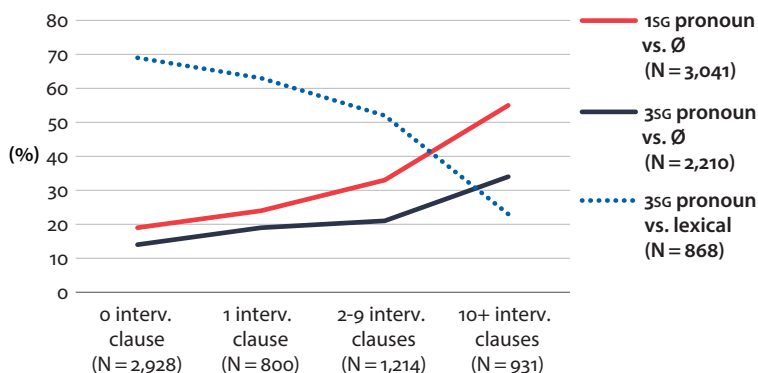


Figure 7. Rate of subject pronouns according to distance (NMSEB):
1SG pronoun vs. Ø, 3SG pronoun vs. Ø, 3SG pronoun vs. lexical

the non-expression of? Here, we have scrutinized 3SG subjects inside and outside the variable context for subject expression as it is usually (and often unreflectingly) circumscribed. Eschewing analytical reliance on a property of linguistic structure taken *a priori* as a given, we relied on actual usage to interpret the workings of referent accessibility in discourse.

The conclusion is that lexical subjects affect patterns within the variable context for subject expression but are appropriately left outside it. We began by treating the presence of a subject pronoun and the absence of any subject noun phrase, commonly termed an unexpressed or null (Ø) subject, as “alternative ways of ‘saying the same thing’” (cf., Labov, 1972: p. 72). The analyses lead us to say that the “same thing” here is grammatical means of mentioning an accessible subject. The variants are appropriately labeled in terms of *expression* or presence vs. absence of the *pronoun*, because the absence of any subject noun phrase is truly in variation with pronominal, not lexical, subjects.

This exercise confirms that linguistic meaning “is [not] the beginning of analysis; ... it is the end result” (Otheguy, 2002: p. 388). In the analysis of variation, “the definition of a linguistic variable is the first and also the last step” (Labov, 2004: p. 7).

7. Conclusion

Let us summarize the findings on contextual effects and distributions. Accessibility is operationalized in the data of discourse as clause linking – prosodic and syntactic – and distance from the previous mention, measured here by the number of intervening clauses from the previous coreferential subject. The use of lexical subjects at greater degrees of distance contributes to distinct contextual distributions of

unexpressed and pronominal subjects for the third person. As compared with 1sg, 3sg unexpressed and pronominal subjects are more likely to occur in a coreferential context. They are also less impacted by accessibility at shorter distances from the previous mention. The clustering of 3sg unexpressed and pronominal subjects means that their referents tend to be talked about in adjacent clauses.

On this basis, we offer an amendment of the characterization of the third person as a “non-person” (Benveniste, 1971). Third person is a transient person, in Spanish, in the form of clustered pronominal and, especially, unexpressed subjects. The amendment generates a hypothesis, testable in the data of language use.

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Appendix. Transcription conventions

Transcription conventions used in this chapter (cf., Du Bois et al. 1993)

Carriage return	new Intonation Unit
	(where the IU does not fit on one line, the second line is indented)
.	final intonation contour
,	continuing intonation contour
?	appeal intonation contour
..	short pause (0.2 secs)
...	medium pause (0.3–0.6 secs)
...()	timed pause (0.7 secs or longer)
-	truncated word
=	lengthened syllable
[]	overlapped speech
<@ @>	speech uttered while laughing
~	pseudonymized proper noun

The justification of grammatical categories

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What is the theoretical justification for positing such constructs as conjugation classes, declension classes, parts of speech, grammatical gender, and agreement rules? This paper argues that no grammatical category or construct should be taken as an *a priori* given; each must be justified by the demonstration that it solves a distributional problem. This is the core analytical principle upon which Columbia School linguistics rests, and it is responsible for much that is innovative in Ricardo Otheguy's grammatical and sociolinguistic research. The novel analytical consequences of this principle will be illustrated by applying it to the distributional problem of the different co-occurrence patterning of such apparent synonyms as *blanca* and *blanco* in Spanish.

Keywords: Columbia School linguistics, grammatical categories, parts of speech, gender classes, Spanish gender, agreement rules, variation

Part I

Introduction

Ricardo Otheguy is known for his skepticism towards presumed truths and for his concern with foundational questions about categories, methodology and goals, questions that go to the heart of linguistic inquiry itself. This turn of mind comes naturally to him, but it has also been significantly shaped by the iconoclastic ideas of William Diver (b.1921–d.1995). Diver was an Indo-Europeanist by training, but in mid-career he turned his focus to synchronic linguistics. In his fresh eyes, the discipline was on the wrong track and needed a new theoretical grounding. Diver's students at Columbia University were inspired by his reconceptualization of the field and following his lead have produced over the past fifty years a literature that has come to be known as Columbia School (CS). That literature has shaped Otheguy's own work in multiple ways. My aim here is two-fold: first to show how these various points of influence arise from a coherent theoretical framework; and

secondly, to show how that framework unifies two strands of Otheguy's research, his work in sociolinguistics and his work in grammatical theory. This unification will be illustrated by his research on the variable use of subject pronouns in Spanish and his reanalysis of gender agreement in Spanish. I will, however, propose an improvement to the latter, a revision that eliminates a serious empirical failure of the original. For readers who may not be familiar with Diver's thinking, this paper starts with a summary of the foundational ideas that have shaped CS research. My summary of CS will focus on the justification of linguistic categories generally, but specifically of word classes, since gender in Spanish is one of my main topics here, and gender classes are a kind of word class.

Rethinking the foundations of linguistics

Diver's rethinking of the foundations of linguistics was triggered by the rise of generative grammar in the 1960s. By that time, Chomsky was indisputably a revolutionary figure, but he was not, in Diver's estimation, a foundational thinker. The reason is evident in Chapter 2 of *Aspects of the Theory of Syntax* in which Chomsky explains the generative enterprise in simple terms. Chomsky begins by laying out three kinds of linguistic facts which he takes to be beyond dispute, illustrating each with the sequence *sincerity may frighten the boy*. The first kind of fact is that those words constitute a sentence in English with a particular phrasal structure: *frighten the boy* is a Verb Phrase consisting of the Verb *frighten* and the Noun Phrase *the boy*, and so on. The second kind of fact involves grammatical function: *sincerity* functions as the subject of the sentence and *frighten the boy* functions as the Predicate; *the boy* functions as the Object of the Verb Phrase, and so on. The third kind of fact involves parts of speech; *boy* is a Count Noun and a Common Noun, *sincerity* is an Abstract Noun; *frighten* is a Transitive Verb that takes certain kinds of subjects and objects. After this summary, Chomsky states the following:

It seems to me that the information presented [above] is, without question, substantially correct and is essential to any account of how the language is used or acquired. The main topic I should like to consider is how information of this sort can be formally presented in a structural description, and how such structural descriptions can be generated by a system of explicit rules.

(Chomsky, 1965: p. 64)

Chomsky's "substantially correct" facts struck Diver as all wrong. In his eyes their recitation by Chomsky revealed an uncritical acceptance of the grammatical categories of the Western linguistic tradition. This tradition is rooted in Aristotelian logic and reached its most developed form in the grammars of the classical languages of the 19th century. As a classicist himself, Diver knew those grammars well; and he

knew something more: those grammars did not work. In one sense, this was already known. Sapir famously said “all grammars leak,” a statement usually understood to be about the intrinsic nature of grammars: every rule has its exceptions. But Diver saw exceptions to grammatical rules in a different light: the exceptions showed that the categories and rules of the grammars were wrong. If Chomsky accepted those categories and rules as substantially correct, then generative grammar was built on a foundation of sand.

For Diver, scientific inquiry begins with observations, not with such received notions as sentence, grammaticality, subject, object, noun, and verb.¹ As Diver saw it, the Western study of language never underwent the scientific revolution that had taken place in other disciplines, and, as a consequence, still remained tethered to Greek philosophy and logic (see Diver, Davis, & Reid, 2012: pp. 430–437). In a scientific inquiry, categories, constructs, and rules are hypotheses that must be tested before they are accepted. This means that they must be defined precisely enough to yield predictions that can falsify them.

The theory of the sentence

In this spirit, Diver recast traditional grammar as a set of scientific hypotheses, which he called ‘the theory of the sentence.’ He did this so as to allow its scientific evaluation. Only when each of the categories and rules of the tradition is in the form of a hypothesis can predictions be derived that can test them. Diver’s version of traditional grammar tells us nothing new; its definitions of terms are all too familiar. His intent was to imbue the traditional categories and rules with a conceptual coherence that we may not have perceived when we learned them as grade school pupils.²

The well-known premise of ‘the theory of the sentence’ was that the structure of the sentence reflects the structure and content of a complete thought. Diver recognized that the theory had two structural levels: the parts of the sentence and the parts of speech. The parts of the simple sentence were the subject, what a complete

1. Huffman (2006) saw this as the key to Diver’s linguistic framework, his single most important idea.

2. In highlighting the internal coherence of traditional grammar, Diver was following the scholarly practice of addressing the strongest version of the thesis one is challenging. Diver’s careful reformulation and then refutation of traditional grammar was a staple of his classes, and brief versions figure in many of his conference presentations. Twelve years after Diver’s death, Joseph Davis and this author synthesized Diver’s various written versions along with our class notes to produce a comprehensive statement “Traditional Grammar and its Legacy in Twentieth Century Linguistics” (Diver, Davis, & Reid, 2012). Here, I can only offer the briefest of summaries.

thought is about, and the predicate, what is said about the subject; the predicate consisted, in turn, of a predicator and one or more complements that completed the thought. The direct object complement received the action of the predicator directly and the indirect object complement received it indirectly. Each of these parts could be modified. The parts of speech reflected the notional categories of thought. Nouns named persons, places, or things; verbs expressed actions or states; adjectives expressed qualities, and so on.

So far, we have little more than a schema of related ideas inspired by the notion of the logical proposition. Diver saw that in order to become testable, the theory of the sentence must make predictions about how those ideas relate to observable manifestations of language. Thus, the second part of the theory links its functions and categories to the morphology of a specific language, in this case Latin. According to the theory, each part of speech was expected to function syntactically as a different part of the sentence. Verbs functioned as predicators; adjectives functioned to modify nouns; adverbs functioned to modify verbs; and nouns functioned as subjects and complements as indicated by the case of the noun. The nominative inflection marked the noun as the grammatical subject; the accusative inflection marked the noun as the direct object; the dative inflection marked the indirect object; the ablative inflection marked an adverbial complement. The verbal morphology of the predicator marked the subject-predicate relation by agreeing in person and number with the noun in the nominative identifying the subject.

This theory has always been attractive aesthetically because of its elegance, rationality, and neat alignment of categories with functions. And, indeed, it has rarely been abandoned; it has consistently served as the model for all the subsequent grammars written for the European languages. It is clearly present, as we saw above, in Chomsky's "substantially correct" facts, and it still underlies today all frameworks that are sentence-based. But if the theory of the sentence is treated as a set of hypotheses from which testable predictions can be derived, every part is falsified by the facts of every language to which it has been applied, most notably *even by Latin*, the language for which it was originally designed.

I cannot do justice here to the thoroughness and rigor of Diver's demonstration of these failures, but only suggest with a few examples what he was getting at. The theory predicts that every sentence has a subject. This prediction is falsified by sentences lacking subjects, i.e., lacking a noun or pronoun in the nominative expressing what the sentence is about. (The generative convention of positing invisible subjects for subjectless sentences makes the hypothesis unfalsifiable.) Verbs are said to agree with their subjects in number, but singular verbs also occur with plural subjects and plural verbs also occur with singular subjects. Reflexive pronouns are defined as pronouns that are coreferential with the subjects of their clauses, but reflexive

pronouns are also coreferential with the subjects of different clauses. Transitive verbs appear without objects, and intransitive verbs appear with objects. According to the theory, the Latin nominative is the case of the grammatical subject, but nouns in the nominative also occur in the predicate, and subjects also occur in the accusative. The accusative was held to be the case of the direct object, and the dative the case of the indirect object; but indirect objects also occur in the accusative, and direct objects also occur in the dative. As for the subject-predicate structure of a sentence, according to the theory each constituent of a sentence is either a part of the subject or the predicate; this hypothesis is falsified by, among many others, the genitive absolute constructions of Greek and the ablative absolute constructions of Latin, which the grammars acknowledge bear no grammatical relation to either the subject or the predicate.³ For Diver, facts such as these falsify the categories, constructs, and rules of the theory of the sentence. And not just for Latin; they are symptomatic of a general failure. For if they fail to fit Greek and Latin, there is little reason to try to make them fit other languages, and certainly no reason to entertain the idea they are part of a universal grammar.

Linguists of Diver's day were unfazed by his critique because they did not regard the notions of sentence, agreement, subject, object, noun, and verb as hypotheses in need of testing; they were pre-analytical categories of observation.⁴ So they took his critique as support for Chomsky's move to abandon the original notional definitions of those terms motivated by the structure of thought in favor of formal definitions that would eventually be articulated. But this missed Diver's larger point. Chomsky dropped the notional definitions of the categories *but he retained the categories themselves*, which then became untestable primitives. For Diver, as we have seen, this was unscientific. Scientific inquiry does not *begin* with *a priori* categories; it begins with observations. But how, one may well ask, can language be observed directly? How can linguistic analysis proceed without some primitive terms to guide the observations? This was the corner that Diver knew he had painted himself into.

3. For example: *Sicilia relictā*, Ulyxes ad regnum Aeoli navigavit 'With Sicily having been left behind, Ulysses sailed to the kingdom of Aeolus.' An absolute construction occurs in the Second Amendment of the Constitution; "A well-regulated Militia, being necessary to the security of a free State, the right of the people to keep and bear Arms shall not be infringed."

4. In "Saussurean Anti-Nomenclaturism," Otheguy (2002) demonstrates why the categories of traditional grammar are not pre-analytical categories of observation. See the Introduction of this volume for further discussion.

The pre-theoretical problem: The acoustic asymmetry of vocal sound⁵

Recognizing that language cannot be observed without categories, Diver moved back a step, looking for the pre-theoretical phenomena that initiate linguistic analysis. What analytical problem exists *prior* to the introduction of any linguistic category? What gets the linguistic enterprise on the road? Diver's deceptively simple answer was strikingly different from the ones usually proposed. The starting point of linguistic analysis has to be the asymmetry of vocal sound. Vocal sound is not white noise; that is, it is not equally loud at all frequencies. Why, then, does vocal sound depart from acoustic symmetry in the way it does on a particular occasion? Why is it not white noise? Why do particular sequences of sounds recur and pattern? Why do the voices in a Moscow bar sound consistently different from the voices in a Tokyo bar, even to someone who doesn't understand a word spoken in either place?

In confronting such questions, the analyst must make an initial commitment about the nature of the phenomena, must, that is, make a reasonable assumption about the vocal sounds the speakers are producing. For the CS linguist, this initial commitment is in the form of the working hypothesis that some vocal sounds are a consequence of people attempting to communicate using a code system. The basic structure of any code, be it Morse code, traffic lights, or music notation, involves the fixed pairing of a 'signaling thing,' and a 'signaled thing'; what Saussure called a *signifiant* and a *signifié*. Diver called the signaling thing in language a *signal* and the signaled thing a *meaning*.⁶ Following Saussure, Diver called the union of these two things a *linguistic sign*. The terms *sign*, *signal*, and *meaning* are used exclusively as just defined when occurring in the CS literature.

This initial commitment transforms the pre-theoretical version of the problem into one delimited by a theoretical framework. By adopting a communicative orientation, the pre-theoretical problem of the asymmetry of all vocal sound (which includes coughs, belches, whistling, and humming) is narrowed to the asymmetry produced by the intent to communicate using a code system; in a word, speech. The acoustic asymmetry of speech can then be presumed by the linguist to be due to people pronouncing (or aiming to pronounce) signals whose meanings they have chosen because the meanings help communicate a message.

Why would Diver take such pains to point out the obvious fact that people are usually trying to communicate when they speak? Grounding linguistic theory in the fact that people make some vocal sounds for the purpose of communication was Diver's way of preventing the introduction of untested notions or unmotivated

5. The following three sections draw heavily on Diver (2012/1979: pp. 293–300).

6. Note that this makes *meaning* a technical term – the invariant semantic content of a signal – and not equivalent to a dictionary definition or to the interpretation of a sentence or form on a particular occasion.

abstractions early in the inquiry that would guarantee its failure. What Diver was guarding against, to repeat, were the categories and constructs of the linguistic tradition: categories such as sentence, subject, object, complement, noun, verb, adjective, and preposition; processes such as pronominalization, embedding, and agreement; semantic notions such as synonymy and propositional meaning; normative concerns of grammatical correctness; even the notion of distinct named languages. Diver regarded all of these as unsuitable starting points for scientific inquiry because none have any *prima facie* plausibility; they are all categories and notions internal to a discredited quasi-philosophical theory that in Diver's judgment fails to fit the facts of every language to which it has been applied. For Diver, the only way to avoid using these failed notions as the starting point of linguistic analysis was to provide an alternative starting point. Vocal sounds known to constitute efforts at communicating via signs provided for Diver the required alternative. The sign, then, was for Diver the one construct that the linguist was allowed to posit without challenge at the outset of linguistic analysis.

The theory-defined problem: The distribution of signals

Taking the linguistic sign – and not the sentence – as a working hypothesis about the basic unit of linguistic structure leads the analyst to consider minimal linguistic forms as either potential signals or parts of signals. Very often a signal turns out to be equivalent to the morpheme of the American descriptivists; but not always, because different criteria govern their postulation. Morphemic analysis was predicated on the assumption that the formal units of language could be identified *prior* to the establishment of their meanings, whereas a CS signal and its meaning constitute a single complex hypothesis: two sides of a single coin. So one doesn't know for certain whether a form is a signal until one has hypothesized and successfully tested a fixed and stable meaning for it.

In a CS analysis, the linguist selects a form as a likely signal and attempts to formulate a meaning that fits its use. By fit I mean account for, or explain, where and when the signal occurs in spoken and written discourse.⁷ This is the basic problem CS is trying to solve. Here are examples of such problems CS analyses have addressed. Why do *the* and *a* always precede the word they modify and on what basis do speakers choose between them? Why does *an* sometimes occur where *a* is expected? Why does the *-s* of *explodes* occur when it does; why, in 'the dog chased the cat' does *dog* precede *chased* and *cat* follow *chased*; why do *look*, *see*, and *watch* occur on different occasions for messages of visual perception? Why does Spanish

7. If this attempt fails, it could be because the proposed meaning is wrong; the form is not a signal, only part of a larger signal; or it is the manifestation of two or more homophonous signals.

pegar ‘to hit’ and *ver* ‘to see’ occur sometimes with clitic *le* (*le pegué* ‘I hit him,’ *le vi* ‘I saw him’) and sometimes with clitic *lo* (*lo pegué* ‘I stuck it,’ *lo vi* ‘I saw him’); why does Spanish *viejo* ‘old’ sometimes precede *amigo* ‘friend’ (*viejo amigo*) and elsewhere follow (*amigo viejo*); why does *yo* ‘I’ co-occur with *canto* ‘sing’ but seldom or never with *canta* ‘sings’; why does *yo* sometimes fail to occur where it might be expected, e.g., *yo tengo veinte años* ‘I’m twenty years old,’ *tengo veinte años* ‘I’m twenty years old.’ Questions of this kind are about what CS calls the *distribution* of signals. CS analyses are ultimately about the distribution of signals – nothing more – because signals most directly shape the acoustic stream of speech.⁸

Maintaining the direct link to observation embodied by the questions above means that the justification of all theoretical constructs, categories, and distinctions proposed by the linguist must be their contribution to explaining the distribution of signals. If they don’t do that, they are not justified. Such familiar categories and constructs as subject, agreement, object, reflexive, sentence, noun, pronoun, gender, relative clause, etc. are only justified, and can only be posited, if they help explain the distribution of signals. This CS position on linguistic categories thus differs from the mainstream one and can be put in terms of burden of proof. For many linguists, the burden of proof lies with the person who denies the legitimacy of a category, whereas for the CS linguist, the burden of proof lies with the person who asserts the legitimacy of a category. Put another way, for many linguists, all the categories of the linguistic tradition are on the table for possible use until one is removed for cause; whereas for the CS linguist, no categories are on the table at the outset; a category gets on the table as a result of having helped to account for the distribution of one or more signals.

This difference has a practical consequence. If, as in all forms of traditional and generative analyses, all categories are on the table, then they can themselves be legitimate objects of inquiry. One might investigate how best to define the notion ‘subject’ or ‘ergativity’; whether noun or verb exist as categories in Mandarin; how Finnish communicates the notion of ‘agency’ or ‘certainty’; how gender agreement works in Spanish. A CS linguist, on the contrary, would never ask, nor attempt to answer, such questions. Categories and constructs such as ‘subject,’ ‘ergativity,’ and ‘agreement’ would only be posited in the course of solving distributional problems; prior to that they are of no interest in and of themselves.⁹

8. For a quantitative demonstration of how the grammatical signs encoding number meanings in English help account for features of the acoustic asymmetry of a spoken text, see Reid (1991, Chapter 10).

9. The CS insistence that theoretical constructs connect in some way to observations at the pre-theoretical level should not be mistaken for empiricism. Rather, it is simply the requirement that distinguishes inquiry about the natural world from inquiry about notions, concepts, and ideas; in short, the difference between science and philosophy.

Description and explanation in linguistic analysis

I said above that one doesn't know for certain whether a form is a signal until one has hypothesized and successfully tested a meaning for it. This means that the analyst is solving for two unknowns simultaneously, an unknown signal and an unknown meaning. Since a CS analysis is accounting for the distribution of signals in terms of their meanings, analysts don't know for certain precisely what distributional problem they will end up solving until they have solved it. This fact has an important consequence for understanding the connection between description and explanation, and the nature of linguistic analysis more generally. The American descriptivists saw the enterprise as purely descriptive and eschewed explanation. Chomsky reintroduced the goal of explanation (in the form of Universal Grammar) but saw the path as proceeding stepwise from description to explanation. The problem with the stepwise procedure is the choice of the initial descriptive terms. Things can be described in multiple ways – ways that can be incommensurate – and the wrong terms could make later explanation impossible. (An example of this point will be offered in the concluding section of this paper.)

That danger of starting from units that later turn out to block the possibility of explanation would be negligible in a science whose structural units are readily apparent. In such cases, description can precede explanation. Diver drew an example from astronomy; the major kinds of celestial objects – the sun, moon, stars, planets, and comets – were identified and their apparent motion described on the basis of pure observation long before people had an understanding of what they were or why they moved as they did. With observable units, description can precede explanation.

The problem confronting the linguist is that the structural units of language are all *unobservable*. Examining a spectrographic representation of speech, one sees only what Martin Joos (1957: p. 115) called “the gooey continuity of phenomena” not units. If it seems that one observes sentences, subjects, complements, verb phrases, relative clauses, words, or phonemes, it is only because one has been searching for them from the start; that is, one thinks one observes them only if those units are already taken as *givens*. Potential analytical units in linguistics, argued Diver, must therefore be evaluated for their explanatory power *at the outset*. Initial descriptive fit alone does not establish the legitimacy of a linguistic term. This means that the stepwise procedure of moving from description to explanation is not feasible. The move has to be in the opposite direction: units and categories are posited with an eye for their explanatory value, and then used as theoretically motivated categories of description.¹⁰

10. Here is Diver's (1979/2012: p. 297) summary of this point: “Observational units can be described before they can be understood. Description can precede explanation. Unobservable units cannot be described until they have been understood. Description cannot precede explanation.”

If units and categories are to be evaluated for their explanatory value at the outset, one must have a clear conception of what counts as explanation. For Diver, scientific explanation involves the demonstration that the phenomena under study are manifestations of principles or facts about which we have independent knowledge. So, for example, the motion of the moon across the sky can be explained in a two-step procedure: first, by the hypothesis that the moon is moving in an elliptical orbit around the Earth; and second, by the demonstration that its orbital motion is a consequence of gravitational attraction in conjunction with the laws of motion. This counts as explanation because we know about gravity and the Newtonian laws of motion quite independent of the movement of the moon.

Similarly, we know many things quite independent of language about sound, communication and human psychology that might help us explain the acoustic asymmetry of speech. For instance, we understand the physics of sound; we know, as mentioned above, that people want to communicate; we know how a code system is structured; we know the physiology of the vocal tract; we know about the Fletcher–Munson curve of auditory sensitivity; we know that differential attention (i.e., the figure/ground distinction) plays an important role in consciousness; we know that many things people do are purposeful, i.e., *goal-directed*; we know people develop routines for frequently performed tasks and also occasionally depart from them; we know they sometimes use tools in ways the tools were not designed for if a reason arises to do so; we know people can infer things from circumstantial evidence and make associations; we know they prefer easier ways of doing things and often take shortcuts. A linguistic sign has explanatory power to the extent it helps to demonstrate that the distribution of its signal is in some way a manifestation of these many other things about which we have independent knowledge. Approaching speech as a phenomenon that may share commonalities with other aspects of human behavior is quite at odds with the assumption that has dominated the field of linguistics, namely that speech is a manifestation of a unique, autonomous, logic-based, computational system governed by mathematical rules for the concatenation of symbols lacking notional content.

Part II

Introduction

All of Otheguy's scholarly work has been shaped by a concern with the theoretical starting points for linguistic analysis. The CS principle that theoretical units, constructs, and categories are justified only when they help explain the distribution of signals has played a consistent role in his thinking; but he has not always articulated this role explicitly. Part II of this paper will therefore illustrate how this principle applies to the solutions of several analytical problems, some of them studied by Otheguy, by ruling out some categories, and justifying others. Most importantly, we will see that the principle that categories must be explanatory responses to true distributional problems leads us to posit features of formal linguistic structure *beyond* the linguistic sign. The sign, to recall, is motivated theoretically by the notion of communication. By contrast, the new structural features that we justify here have no *prima facie* plausibility – no deductive motivation – in terms of the communicative function of language. Nevertheless, they gain entrance to the theory because, whether or not they are part of the tradition, they are links in an explanatory chain that shows that aspects of the asymmetry of vocal sound are a consequence of people attempting to communicate using a code system.

The first two examples that I offer presently show how the principle legitimates two categories found in the traditional grammars of Spanish and Latin: conjugation classes, and declension classes. The third example, on the other hand, shows that the principle does not justify the part of speech categories *noun* and *verb* found in grammars of English. The fourth example, based on Otheguy's own work, shows a more complex outcome. The principle supports the traditional category of gender for Spanish, but it does not support the part of speech distinction between nouns and adjectives. Moreover, while it justifies the positing of gender classes, the classes differ in their makeup from the traditional ones. This analysis thus provides a clear example of the difference between a CS and the traditional-generative treatment of a major feature of linguistic structure. One striking difference will be that the CS analysis does not posit a formal rule of gender agreement in Spanish. But the CS framework does not involve a simple-minded wholesale rejection of the traditional categories; what it involves is the demand that any proposed category or construct help explain the distribution of signals. We will see that, in some cases, the explanatory solution may involve the postulation of brand new categories, and, in other cases, categories that resemble those already envisaged in the tradition.

First example: Conjugation classes in Spanish

Consider the four words *canta* ‘sings,’ *cante* ‘might sing,’ *come* ‘eats,’ and *coma* ‘might eat.’ The final vowels are traditionally analyzed as morphological indications of the distinction between what are called the indicative and subjunctive mood, a distinction that has to do with the possibility of an event. (See Diver, 2012a and Goldberg, 1991, 1995 for CS analyses of this distinction.)

	‘sing’	‘eat’
(Third person, singular, present) indicative	<i>canta</i>	<i>come</i>
(Third person, singular, present) subjunctive	<i>cante</i>	<i>coma</i>

The suffixes *-a* and *-e* are morphemes but neither is a signal because no invariant meaning can be assigned to either of them; neither consistently indicates either the indicative or the subjunctive. Rather, *-a* suffixed to *cant-* consistently signals the indicative, and *-a* suffixed to *com-* consistently signals the subjunctive. The same, *mutatis mutandis*, for *-e*. This is true of many other Spanish words. The analytical solution familiar to any student of a Romance language is to group certain words into formal classes called *conjugation classes*. The members of each class co-occur with a different set of suffixes; together each stem and suffix makes multiple semantic distinctions. These distinctions involve the time of the event, the likelihood of the event, the thematic status of the event, the number of entities at the center of attention with respect to the event, and the relation of that entity to the speech situation.

Talking about verb endings this way reflects a feature of the CS framework not yet mentioned. All grammatical morphology is analyzed as signals of meanings that, in one way or another, facilitate communication. Typically, grammatical meanings are grouped into systems that make distinctions within a single semantic substance such as time, number, location, degree of importance, and degree of control of an event. The signals of grammatical meanings are usually morphological; but they can also be features of word order. For example, *viejo* ‘old’ before and after *amigo* ‘friend’ (*viejo amigo* ‘longstanding friend,’ *amigo viejo* ‘elderly friend’) signal a meaning distinction having to do with the kind of semantic relation *viejo* bears to *amigo* (see Klein-Andreu, 1983).¹¹

11. For an example of a CS analysis of a grammatical morpheme universally regarded as meaningless, see Reid (2011). Reid analyzes the *-s* of ‘he talks’ as a signal and posits a meaning for it that accounts for its distribution without positing in addition a formal rule of subject-verb agreement.

Returning now to conjugation classes, since there seems to be no ready semantic or phonological rationale for why each lexical class takes the suffixes it does, those facts must be treated as arbitrary formal features of linguistic structure. The most straightforward way to do this within the CS framework is to treat the conjugation class of the lexical stem as an integral part of each *signal* (rather than the environment of the signal); in other words, the signal has two parts, a suffix and the conjugation class of the lexical stem to which the suffix is attached. For instance, the lexical stems *cant-* ‘sing’ and *com-* ‘eat’ belong to different conjugation classes, and each class employs different suffixes for the points involving the indicative/subjunctive contrast; there are thus two different signals for the indicative and two different signals for the subjunctive (in the third person, singular, present) as shown below.¹² Both components of these signals are part of the arbitrary linguistic code.

signals	the indicative/subjunctive contrast
class I stem + <i>a</i>	indicative
class II stem + <i>e</i>	indicative
class I stem + <i>e</i>	subjunctive
class II stem + <i>a</i>	subjunctive

Conjugation classes are justified because they enable the statement of signals for which stable meanings can be posited, and signals and meanings are links in an explanatory chain that connects speech sounds to things about which we have independent knowledge, in this case the structure of a code. Note, however, that positing conjugation classes of stems does not legitimate *verb* as a formal word category. Spanish has conjugation classes and suffixes associated with certain grammatical distinctions that, for lack of a better name, could be called verbal suffixes; but so far at least no case has been made for the category verb. The rationale is that positing conjugation classes enables the statement of signals as testable hypotheses, but positing, in addition, the union of those classes as a formal structural class in its own right has not (yet) been shown to help explain the distribution of signals. Conjugation classes, then, represent a clear example of how the CS principle of limiting categories to those required as links in an explanatory chain leads to the postulation, not only of linguistic signs, but of other constructs as well.

12. Spanish actually requires the positing of three conjugation classes for reasons that parallel the case made here for two.

Second example: Declension classes in Latin

Latin provides another example of when it is necessary to posit formal lexical classes in a CS analysis. Like Spanish, Latin has conjugation classes, and in addition, it has classes which would be called in the tradition *nouns*. For readers who do not know Latin, a word of explanation. All grammars of Latin analyze certain words as occurring with suffixes that indicate five morphological categories called *cases*: nominative, ablative, dative, accusative, and genitive. Diver accepted this morphological analysis and made four of these cases signals in a grammatical system whose meanings indicate the relative degree of control each participant exercises over an event, one usually described by an associated word with verbal suffixes (Diver & Davis, 2012). Each case is identified by a suffix, but (as in Spanish) the suffix varies with the lexical stem. The problem is how to state the suffixes for each case. The traditional analytical solution is to divide these stems into five formal classes called *declension classes*. Each class takes a different set of case suffixes. Figure 1 shows the suffixes for the nominative singular of the five declensions.

Declension class I	<i>puella-0</i> (fem.)	'girl'	(alternate analysis: <i>puell-a</i>)
	<i>agricola-0</i> (masc.)	'farmer'	(alternate analysis: <i>agricol-a</i>)
Declension class IIa	<i>serv-us</i> (masc.)	'servant'	<i>pinus</i> (fem.) 'pine tree'
Declension class IIb	<i>puer-0</i> (masc.)	'boy'	
Declension class IIc	<i>templ-um</i> (neuter)	'temple'	
Declension class IIIa	<i>civ-is</i> (masc.)	'citizen'	<i>civitas</i> (fem.) 'citizenship'
Declension class IIIb	<i>rex</i> [= <i>rek-s</i>] (masc.)	'king'	
Declension class IIIc	<i>anser-0</i> (masc.)	'goose'	
Declension class IV	<i>cas-us</i> (masc.)	'(a) fall'	<i>man-us</i> (fem.) 'hand'
Declension class IV	<i>gen-ū</i> (neuter)	'knee'	
Declension class V	<i>re-s</i> (fem.)	'thing'	<i>diē-s</i> (masc.) 'day'

Figure 1. Nominatives in different declension classes in Latin¹³

This traditional solution is, I believe, the right one. And like the Spanish signals for the indicative and subjunctive, the declension class of the lexical stem should be treated as an integral part of the Latin signals for the case distinctions. Declension

13. It may appear that Declension class IV masculine and Declension class II could be collapsed, since the nominative suffix for both is /us/. This cannot be done however because the suffixes for the other cases are not the same, e.g., the genitive of *manus* is *manūs*, while the genitive of *servus* is *servi*. Note too, that although the tradition posits five declension classes, there are actually at least ten because some declensions are subdivided.

classes are thus legitimate features of the morphological structure of Latin because they enable the statement of signals and meanings, links in an explanatory chain that connects the sound of speech to the structure of a code system.

Third example: Nouns and verbs

The CS principle that a category or construct is only posited as a part of explaining the distribution of signals can rule out a possible category as well as legitimate it. In the passage below, Diver argues that positing a part of speech distinction between nouns and verbs is not justified for English, because there is no distributional problem that this step would solve. Referring to existing CS analyses, Diver points out that several grammatical systems have meanings applicable to entities (e.g., meanings having to do with the identification of a referent and number of things described), and other systems have meanings applicable to events and states (e.g., meanings having to do with an event's temporal location and its likelihood). Following the tradition, one could call the signals in the former *nominal* morphology and the signals in the latter *verbal* morphology. For example:

Nominal morphology (<i>italicized</i>)	Verbal morphology (<i>italicized</i>)
<i>The cold water-0</i> is refreshing Lexical stem	He <i>has water-ed</i> the grass. Lexical stem
He gave <i>many long talk-s</i> . Lexical Stem	He always <i>talk-s</i> a lot. Lexical Stem

Diver then writes:

It is not then that there is a part of speech classification existing independently in the lexicon, such that a particular class "takes" certain endings, but that that part of speech is an effect in the message produced by the meanings of the grammatical systems associated with the lexical item in the particular instance. [italics WR]

(Diver, 2012/1995: pp. 502–503)

Diver is making the familiar point that most of the common English words that occur with nominal morphology also occur with verbal morphology. As shown above, *talk* and *water* occur with both morphologies. This means that the list of putative nouns in English would look very much like the list of putative verbs. For example, *look, talk, walk, throw, take, mend, burn, run, flower, water, up, sneeze*, and countless others all function as both. This being the case, argued Diver, there is no

distributional problem that would be solved by establishing nouns and verbs as separate lexical classes; no problem, at any rate, having to do with which words occur with nominal morphology and which occur with verbal morphology.¹⁴ Readers should note that the justification for positing conjugation classes for Spanish and declension classes for Latin is precisely what Diver says is lacking in English to justify nouns and verbs, namely two classes of lexical stems each occurring with different endings (i.e., one that would occur only with nominal morphology and the other occurring only with verbal morphology).

Nouns and verbs as semantic categories

One might counter that in English not all words occur with both nominal and verbal morphologies; for instance, *explode* almost always occurs with verbal morphology, and *explosion* with nominal morphology. Do not the distributional restrictions of such words constitute the sort of problem that would justify positing noun and verb as lexical categories for English? The answer hinges on whether doing so would have independent explanatory power. Would those categories help account for any distributional facts not already accounted for by the meanings of the words themselves?

Answering that question necessitates a consideration of lexical meaning. What kind of meanings might one posit for words that occur with both nominal and verbal morphologies such as the ones listed above? One possibility would be a meaning that is notionally mid-way between entity and event. The CS analysis in Sabar (2018) posits just such a meaning for *look*: VISUAL ATTENTION. This meaning makes *look* equally coherent notionally with the meanings signaled by both nominal and verbal morphologies – *she looked at him, she gave him a dirty look* – thus supporting Diver’s position that there is no need to posit separate lexical classes.

For other words such conceptual neutrality would be hard to achieve if the meaning, as required by CS linguistics, is to be a testable hypothesis. So it is likely *water* should be defined as a substance and *talk* as an activity. Such definitions do not, however, necessarily constrain speakers to use *water* only with nominal morphology and *talk* only with verbal morphology, i.e., such definitions do not in themselves force the postulation of lexical classes. Speakers can exploit the grammatical meanings of nominal and verbal morphology to shape the interpretation of lexical signs in a direction *contrary* to their inherent lexical meanings. So when *water* occurs with an *-ed* suffix, the meaning of *-ed* ‘Event located in the PAST’ says in effect ‘interpret *water* as an activity (involving the substance H₂O) taking place in the past,’ e.g., *he watered the wine*. By the same token, when *talk* occurs with a

14. This does not preclude the possibility that there might be some other distributional problem in English whose solution would call for positing nouns and verbs as lexical classes.

determiner, a word functioning adjectivally, a number inflection, and is followed by a word with an *-ed* suffix, their meanings collectively say, in effect ‘interpret *talk* as a thing that could play a role in controlling an event’ (see Stern, this volume), e.g., *his long talks bored the audience*.¹⁵

The extent to which speakers exploit the expressive potential of nominal and verbal morphology to shape the interpretation of words in ways contrary to their inherent meanings depends on the other expressive options available. If the only difference in meaning (by hypothesis) between *explode* and *explosion* is event vs. entity, that will be the basis on which speakers choose between them, and speakers will then always use verbal morphology with *explode* and use nominal morphology with *explosion*. On the other hand, the expressive alternatives to *water* whose inherent meanings are more explicitly event-like involve other differences as well, forcing the language user to do more than simply cast the liquid as an event. *Irrigate* is Latinate and more precise, suggesting an agricultural context; while *dilute with water* is Latinate and wordy. So on some occasions using *water* with verbal morphology is the best expressive option in light of the alternatives.

It would seem, then, that positing two classes of English words, one expressing things (e.g., *explosion*, *water*) and a second expressing events (e.g., *explode*, *talk*), would not constitute a solution to any real distributional problem; put another way, it would not increase the explanatory power of the grammar beyond what it already had by virtue of having posited such meanings for those signals individually, (or by virtue of recognizing that the meanings of suffixes can sometimes shape interpretations in directions contrary to lexical meanings, as is the case with *water* and *talk*). If one follows the principle that all theoretical categories and constructs must be part of an explanatory chain that is set in motion to solve a true distributional problem,

15. Using a sign in such a loose way would be impermissible in a formal system governed by rules of logic. But if a language is conceived as a set of communicative tools, then meanings are tested in terms of their contribution to the message the speaker is trying to communicate, not by logical criteria. This is the difference between seeing language use as *goal-directed* rather than rule-governed. In the two cases described above, the meanings of *talk* and *water* are still operative: *he watered the wine* does involve the substance H₂O, and the phrase *his long talks* does describe a durative event. The meanings of the two words help communicate the messages via an interaction with the meanings of the associated nominal and verbal morphology. Nevertheless, meaning hypotheses such as these can still be falsified. The meaning for *bear* that covers ‘the floor can *bear* one ton,’ ‘he *bears* his sorrow silently,’ and perhaps ‘I cannot *bear* his presence’ would not cover the *bears* in the Goldilocks story. If the Goldilocks *bear* were counted as the same sign as the first three, it would falsify the meaning hypothesis. The analyst would then conclude that either the hypothesized meaning that covers the first three instances of *bear* is wrong, or that the Goldilocks *bear* is a different sign. See Sabar (2018) for a CS evaluation of the several meaning hypotheses that have been proposed for *look* and the empirical basis for choosing one over the others.

one would conclude that English is a language that lacks nouns and verbs. For the CS linguist, the assertion that English has nouns and verbs as semantic classes is descriptively tenable but theoretically unjustified; or, to put it more provocatively, accurate but not true.

Fourth example: Gender classes

Even under the stringent Diverian standard adopted by CS, the facts associated with what in Spanish and other languages is usually called grammatical gender is another place that legitimately calls for positing lexical classes.¹⁶ I will argue, then, that Spanish grammatical gender is like Spanish conjugations and Latin declensions, but unlike English parts of speech; English nouns and verbs are not justifiable categories, while Spanish conjugational and Latin declension classes are. But, under the Diverian standard, the classes I will posit for Spanish do not match those posited by the tradition, or by the generative analyses that rely on it. That is, the analysis that I'm about to offer keeps the familiar feature of gender classes but is in fact radically different from the traditional-generative one.

To see why, we must ask what problem gender classes help solve in Spanish and languages like it. I suspect most linguists would answer 'agreement phenomena.' But recall that Diver foreclosed the introduction of traditional notions that might well guarantee its failure, and the idea of grammatical agreement certainly has that potential. The problem with agreement is that the term is not the name of an objectively observable phenomenon, but the name of a hypothesis – a proposed solution to a problem. One doesn't observe in Spanish *blanca* and *la* agreeing with *casa* in the phrase *la casa blanca* 'the white house'; one only observes the frequent co-occurrence of those three words, and that they make up a sequence that is easily understood. By contrast, *el casa blanco* 'the white house(?)' rarely if ever occurs, seems very odd, and it's hard to imagine why one might say it. Grammatical agreement is a proposed explanation for these facts. Better then to begin, not with agreement, gender, and rules as if they were facts, but with the actual distributional facts that have given rise to the notions of gender classes and agreement rules. Consider these facts of Spanish.

Spanish has two words that both seem to mean 'old': *vieja* and *viejo*. But each co-occurs with different words when used in attributive constructions. To express 'old house,' one says *casa vieja* (or *vieja casa*), not *casa viejo*; for 'old tree,' one says *árbol viejo* or *viejo árbol*, but rarely if ever *árbol vieja* or *vieja árbol*. There seems

16. Diver (2012b: pp. 251–5) accepted the existence of gender classes in Latin and developed their communicative function in facilitating pronoun reference; Zubin & Köpcke (1981) do the same for German. Contini-Morava (2002) accepts the existence of gender-like noun classes in Swahili and addresses the question of the meaning of noun class markers.

to be no apparent semantic or phonological explanation for these distributional facts. Other words, however, show no such asymmetrical pairing when used attributively. *Grande* ‘big/large/great’ co-occurs with both *casa* ‘house’ and *árbol* ‘tree’: *casa grande*; *árbol grande*; *estudiante* ‘student’ co-occurs with both *viejo* and *vieja*: *estudiante viejo*; *estudiante vieja*. Stepping back and beginning with these distributional problems keeps one’s analytical options open; positing a rule of noun–adjective gender agreement should be just one possibility to consider, not an *a priori* commitment. The analysis I am about to propose is another possibility.

The analysis of Otheguy and Stern (2000)

My analysis is based on Otheguy and Stern (O & S) “The Acategorical lexicon and the pairing strategies: a critical account of inherent gender in Spanish” (2000). The majority of their article is a critique of the traditional analysis of Spanish gender. Their critique follows the same line of argumentation as Diver’s critique of traditional grammar. But while their thinking is that of Diver’s, they do not spell out its underlying logic, and it is worthwhile to do so now.¹⁷ What follows is the logic of hypothesis testing as applied to such categories as noun, adjective, and gender classes, and to constructs such as agreement rules. It is the logic that O & S borrowed from Diver and that they applied, correctly, in their rejection of Spanish nouns, adjectives, and agreement; and that they applied, incorrectly, to their rejection of Spanish gender classes.

If one were going to posit a set of explanatory linguistic categories and constructs such as the ones mentioned above (noun, adjective, gender classes, agreement rules), each category must be testable, which means open to falsification. The ideal categories would have the following properties:

- There would be a category suitable for every item being categorized;
- Each category would be defined so that testable predictions can be drawn from it;
- If the definitions of two categories lead to different predictions, those definitions should not allow the same item to belong to both categories.

This last requirement calls for elaboration because most linguistic analyses using the categories of sentence grammar violate it. In sentence-based analyses, the same word is allowed to be both a noun and a verb (in the case of English *up*, a noun, verb, adjective, adverb, and preposition); the same verb can be both transitive and intransitive (*sing*); the same word can be both a count noun and a mass noun (*chicken*); the same noun can be both singular and plural (*deer*). This poses no

17. Diver did not spell his logic out in his writing as will be done here, but his critique of traditional grammar is clearly based on the logic of hypothesis testing and the notion of falsification.

problem if one accepts the legitimacy of these grammatical categories from the start. But if one regards the categories as hypotheses, then each such instance of dual-category membership weakens the testability of the constructs involved. To illustrate why, consider the sentence-based analysis that posits a rule of subject-verb agreement, and *horse* as a singular noun and *horses* as a plural noun. The agreement rule predicts (1) and (2).

- (1) The horse grazes.
- (2) The horses graze.

The regular occurrence of (1) and (2) would be confirmations of the predictions of the agreement rule that the verb agrees in number with its subject. On the other hand, if (3) and (4) occurred, they would be disconfirmations of the predictions.

- (3) The horse graze.
- (4) The horses grazes.

If one decided that an instance of (3) or (4) was a legitimate test of the prediction (i.e., not a 'performance error'), that *single* example would falsify the agreement hypothesis regardless of the frequency of (1) and (2). In short, confirmations of predictions count for little in hypothesis testing; what counts is the absence of disconfirmations, for they hold the potential of falsifying the hypothesis.¹⁸ Put another way, hypotheses are supported by escaping falsification on occasions they could be falsified.

So far, so good. Consider now the hypothesis that *antelope* is both a singular noun and an irregular plural noun. The agreement rule then predicts the regular co-occurrence of singular *antelope* with singular *grazes* and plural *antelope* with plural *graze*.

- (5) The antelope grazes.
- (6) The antelope graze.

Disconfirmation of the prediction drawn from the agreement rule would be – note carefully now – instances of plural *antelope* co-occurring with singular *grazes* and singular *antelope* co-occurring with plural *graze* as in (7) and (8).

- (7) The antelope grazes.
- (8) The antelope graze.

As can be seen, when *antelope* can be both a singular and a plural, confirmations of the predictions of the agreement rule are indistinguishable from disconfirmations.

18. Disconfirmation of a prediction falsifies the hypothesis being tested if the prediction follows necessarily from the hypothesis.

This means that (5) and (6) do not truly test the agreement rule, because they are not occasions when falsification is possible. Mistaking (5) and (6) as supporting the rule is an instance of confirmation bias, a natural human tendency that the scientific method is designed to thwart.¹⁹

Summarizing Otheguy and Stern's critique of the traditional analysis of Spanish gender in terms of the considerations above, the traditional analysis posits noun and adjective as lexical categories, but then says that nearly forty percent of the adjectives can function as nouns, e.g., *viejo*. The traditional analysis also posits a distinction between words with inherent gender and words with variable gender (i.e., between nouns and adjectives), but then adds that many words that have variable gender also have inherent gender (again *viejo*). It posits a distinction between masculine words and feminine words, but then says some words are in effect both masculine and feminine, e.g., *pobre*. In truth, the traditional analysis is more in the way of an interpretive framework than a set of testable hypotheses. And, in fact, its most straightforwardly testable feature – the rule that masculine nouns take the masculine articles, *el, los, un, unos* and feminine nouns take the feminine articles *la, las, una, unas* is falsified by examples (to come) where the reverse occurs.

Following the CS principle that categories and constructs must be explanatory solutions to distributional problems, O & S do not posit a formal rule of noun–adjective agreement, nor a distinction between nouns and adjectives, nor between words with inherent and variable gender. In their place, they posit what they call two *pairing strategies* which, we will see shortly, are justified because they do help solve distributional problems. O & S's first pairing strategy (to come) accounts for the fact that *el* regularly co-occurs with some words, while *la* regularly co-occurs with others; for example, that *el* pairs with *árbol* and *libro*, and *la* pairs with *casa* and *mano*. But their strategy by itself doesn't account for **which** words pair with *el* and **which** with *la*: why does *el* regularly pair with *árbol* and not *casa*? Why does *la* regularly pair with *mano* and not *libro*? This is a major analytical failure. O & S cannot account for these facts because they have not posited gender classes. My purpose here is to rectify this shortcoming of O & S so that their analysis successfully solves the distributional problem described above of the asymmetrical co-occurrence patterning of some Spanish words.²⁰

19. For a CS analysis of grammatical number in which *antelope* is only a singular, see Reid (1991, Chapter 4).

20. O & S mistakenly assumed that gender classes and gender agreement were a single hypothesis because they cannot be tested independently, and that if there were no rule of gender agreement there could be no gender classes (personal communication). But, in fact, they are two separate hypotheses; a language can have gender classes without having a rule of gender agreement as is the case in the analysis being presented.

Gender classes in Spanish

As already indicated, I posit for Spanish the same kind of formal construct as does the linguistic tradition: gender classes. (There might be some other solution, but I cannot think of one.) I assign many Spanish words to one of two gender classes, which I call Class A and Class B. I use these terms rather than the traditional masculine and feminine because, as the analysis proceeds, it will prove important to underscore the well-known semantic arbitrariness of the class assignment. To be sure, the gender classes have something to do tangentially with sex; but they are clearly not semantically based, as the majority of the words in both classes describe inanimate things and abstract concepts; and many words describing animates can refer to both males and females, e.g., *persona*, *víctima*, *foca*, *tiburón* ('person,' 'victim,' 'seal,' 'shark'). Figure 2 shows examples of words assigned in my analysis to the two classes.

Class A		Class B	
<i>hija</i>	__'daughter'	<i>hijo</i>	__'son'
<i>vieja</i>	'old'	<i>viejo</i>	'old'
<i>casa</i>	'house'	<i>mapa</i>	'map'
<i>mano</i>	'hand'	<i>libro</i>	'book'
<i>buena</i>	'good'	<i>bueno</i>	'good'
<i>la</i>	'the'	<i>el</i>	'the'
<i>mesa</i>	'table'	<i>día</i>	'day'
<i>víctima</i>	'victim'	<i>árbol</i>	'tree'
<i>blanca</i>	'white'	<i>alto</i>	'high'
<i>amarilla</i>	'yellow'	<i>violín</i>	'violin'
<i>alta</i>	'high'	<i>amarillo</i>	'yellow'
<i>moto</i>	'motorcycle'	<i>blanco</i>	'white'
<i>persona</i>	'person'	<i>gabinete</i>	'cabinet'
<i>una</i>	'a'	<i>un</i>	'a'
<i>uno</i>	'one'	<i>uno</i>	'one'
<i>primera</i>	'first'	<i>planeta</i>	'planet'
<i>multitud</i>	'crowd'	<i>los</i>	'the' pl.
<i>foca</i>	'seal'	<i>tiburón</i>	'shark'
<i>policía</i>	'police'	<i>primer</i>	'first'
<i>esa</i>	'that'	<i>ese</i>	'that'
<i>-ada</i>	meaning undetermined	<i>-ado</i>	meaning undetermined
<i>-ida</i>	meaning undetermined	<i>-ido</i>	meaning undetermined

Figure 2. Some examples of Class A and Class B

Readers will immediately note my gender classes diverge from the traditional ones in two striking ways:

- *vieja* and *viejo* (and *bueno* and *bueno*) are treated as separate lexemes with the same meaning, not two forms of the same lexeme that appear, now one, now the other, depending on agreement (the same treatment is given also to *una* and *un*, *la* and *el*, *esa* and *eso*);
- as a consequence, there is no distinction between lexemes with what the tradition calls inherent gender (so-called nouns *casa*, *árbol*) and lexemes with variable gender (so-called adjectives *viejo* ~ *vieja*, *bueno* ~ *bueno*). All forms assigned to a gender class have inherent gender; thus both *casa* and *bueno* are inherently Class A, and *árbol* and *bueno* are inherently Class B.

Both these differences stem from the fact that, following O & S, I did not begin the analysis with the uncritical adoption of the categories noun and adjective. O & S did not posit that category distinction in Spanish for the same reason Diver did not posit nouns and verbs in English: doing so is not a part of the solution to the distributional problem being addressed. As is well known, most putative Spanish adjectives regularly function syntactically as nouns, for example, *el hombre viejo* ‘the old man,’ *el viejo* ‘the old man/the old one.’²¹ I also did not begin with the category ‘word.’ My Classes A and B are classes of signs, not words, which is why they contain both words and grammatical suffixes (-*ada*, -*ado*); both are signs. This also explains my conclusion about the synchronic status of the final vowels of *vieja* and *viejo*, namely that they are part of a single lexical sign. I explored the possibility of analyzing them as signs in their own right; if successful, then *viejo* and *vieja* would be instances of the same sign stem *viej-* with different sign suffixes. But that was not feasible because, unlike the case with -*ado* and -*ada*, I can see no possibility of

21. Otheguy (1976) proposed to unite the forms often called headless relatives with the forms called articles, regarding them as uses of the same sign. In the tradition, in a headless relative like *Leí el que me recomendaste* ‘I read the one you recommended’ no specification of the noun is offered, as it is in *Leí el libro que me recomendaste* ‘I read the book you recommended.’ For Otheguy, this has things backward. The word that introduces the entity in the message (the true ‘noun’ if you will) is the l-form, the *el* in this case, which can appear by itself, or modified by a word, such as *libro*, the true ‘adjective.’ Relying on and extending the analysis by Flora Klein-Andreu, Otheguy says that *el libro* is a sequence of Characterized plus Characterizing (Zed + Zing) every bit as *libro nuevo* ‘new book.’ Just as *libro* can appear unmodified by *nuevo*, so can *el* appear, in the traditional headless relatives, unmodified by *libro*. A lexical item like *libro* is thus neither a noun nor an adjective, inasmuch as it can be Zed (*libro nuevo*) or Zing (*el libro*). Only the *el*, as clearly seen in the uses as headless relative, consistently expresses an entity; it is the only true ‘noun’ among these forms.

positing meanings for those two vowels. Saying that *-a* means ‘Gender A’ and *-o* means ‘Gender B’ without defining A and B semantically strikes me as vitiating the CS notion of meaning as semantic substance that contributes to the communication of a message.²² Moreover, treating those vowels as grammatical suffixes is in fact part and parcel of maintaining the distinction between nouns and adjectives which is not the line of analysis being pursued. I will, however, continue to use the familiar term ‘word’ instead of ‘sign’ when the distinction is not relevant.

Readers may have noted that words such as *grande* and *estudiante* are not assigned to either Class A or Class B. They are what could be called *genderless words*, just like *comer*, *para*, *yo*, and *tú*. The reason I do not assign these words to a gender class is that doing so would not solve any distributional problem. As pointed out above, *grande* and *estudiante* exhibit no constraints on their distribution, co-occurring with both Class A and Class B words (*casa grande*, *árbol grande*); so assigning these words to a gender class would violate the CS principle that analytical moves must constitute explanatory solutions to distributional problems, not reactions to the *a priori* notional distinction between entities and qualities, a distinction rooted in a philosophical tradition. Assigning words like *grande* and *estudiante* to a gender class would be a solution in search of a problem.

Genderless words, shown in Figure 3, do not actually form a true structural class on a par with Classes A and B. Figure 3 was created to draw attention to the difference between my gender classes and the traditional ones, which sometimes do include *joven*, *grande*, *pobre*, and *estudiante*.²³

22. This illustrates again the difference between a morpheme and a signal; see discussion in Part I. While the morphological status of the final *a* of *casa* and *buena* and the final *o* of *libro* and *bueno* in modern Spanish could well be different from their status in Latin, it should nevertheless be noted that those vowels were never in fact analyzable as indications of the gender of a noun or adjective. Most of those *a*'s go back to the Latin accusative suffix *-am* of the first declension; and most of those *o*'s go back to the accusative suffix *-um* of the second and fourth declensions; but declension class and gender class were separate intersecting categories in Latin. While almost all first declension nouns are “feminine,” a dozen common ones are “masculine” (e.g., *agricola* ‘farmer,’ *nauta* ‘sailor’); and while most second declension nouns are “masculine,” not all (see Figure 1). Spanish *mano*, Class A for most speakers, goes back to fourth declension “feminine” *manum*. The fact that the final vowels of *casa*, *buena*, *libro*, and *bueno* are not signals does not, however, mean they are synchronically irrelevant. The strong statistical tendency for Class A signs to end in /a/ and Class B signs to end in /o/ functions as a heuristic in acquisition, allowing children to guess the gender of the words and create neologisms on the basis of the vowel pattern.

23. The reason Figure 4 includes *para*, *comer*, *tú*, and *siempre* is again to underscore the fact I have not posited the categories noun and adjective which would have then limited the list of genderless words to nouns and adjectives.

<i>grande</i>	'big, great'	<i>treinta</i>	'thirty'	<i>tú</i>	'you'
<i>estudiante</i>	'student'	<i>cantante</i>	'singer'	<i>dulce</i>	'sweet'
<i>yo</i>	'I'	<i>pobre</i>	'poor'	<i>fiel</i>	'faithful'
<i>mejor</i>	'best/better'	<i>brillante</i>	'brilliant'	<i>valiente</i>	'valiant'
<i>joven</i>	'young/youth'	<i>siempre</i>	'always'	<i>uno</i>	'one'
<i>para</i>	'by, for'	<i>comer</i>	'to eat'		

Figure 3. Genderless words not included in Class A or Class B

Figure 3 helps to clarify the specific distributional problem being addressed by the postulation of gender classes. The choice of many signs on the two lists can be accounted for in terms of their meanings; their gender plays no role. So, for example, the choice between *hija* and *hijo* to describe a family member is accounted for by their meanings of 'daughter' and 'son.' Similarly, the choice between *blanca* and *amarilla* to describe, say, the color of a house is accounted for by their meanings 'white' and 'yellow.' But the meanings of signs like *blanca* and *blanco* or *vieja* and *viejo* do not account for the choice between them because the two signs are synonyms. This is the problem being addressed: the distribution of gendered signs that are synonyms. As will be seen, assigning *hijo* and *hija* to a gender class is done as part of the solution to the choice between *viejo* 'old' and *vieja* 'old' when those words are used to describe *hijo* or *hija*.

The need to posit a communicative strategy

The assignment of signs to gender classes does *not* however solve any distributional problem all by itself; something else is needed. That is, the gender classes alone do not mandate any particular co-occurrence patterning; they cannot explain why the sequence *casa blanca* often occurs and *casa blanco* rarely does; why *árbol blanco* often occurs and *árbol blanca* rarely does. Still lacking is a statement of the principle of choice between words with the same apparent meanings but different gender class assignments: pairs such as *blanca* and *blanco*, *vieja* and *viejo*, *una* and *un*, *buena* and *bueno*, *alta* and *alto*, *la* and *el*, *linda* and *lindo*, *pequeña* and *pequeño*, *esa* and *eso*. For a reason to be explained shortly, I call such a principle of choice a *communicative strategy*. Two principles of choice, or strategies, are posited, to be discussed shortly. They are essentially equivalent to the two that O & S posit.

STRATEGY 1: In constructing a discourse, choose all gendered signs that describe the same thing (i.e., that have the same conceptual 'referent') from the same gender class.

Strategy 1 helps hearers perceive which words go together conceptually in the message, and it is illustrated by the signs in bold in example (9) below. Note that Strategy 1 applies both to the choice (in traditional terms) of a noun and a modifying adjective (e.g., *sonrisa campechana*) and to the choice of a noun and its article (e.g., *la sonrisa*, see footnote 21).

- (9) ¿Hay **una historia**? Si hay **una historia** empieza hace tres años. En abril de 1976, cuando se publica mi **primer libro**, él me manda **una carta**. Con **la carta** viene **una foto** donde me tiene en brazos: desnudo, estoy sonriendo, tengo tres meses y parezco **una rana**. A él, en cambio, se lo ve favorecido en **esa fotografía: traje cruzado**, sombrero de **ala fina**, **la sonrisa campechana: un hombre** de treinta años que mira **el mundo** de frente. Al fondo, **borrosa** y casi fuera de foco, aparece mi **madre**, tan joven que al principio me costó reconocerla.

Piglia, Ricardo. 1980/2001. *Respiración artificial*.

Barcelona: Editorial Anagrama

‘Is there a story? If there’s a story [it] begins three years ago. In April of 1976, when my first book is published, he sends me a letter. With the letter comes a picture where [he] has me in his arms: naked, [I] am smiling, [I] am three months old and [I] look like a frog. He, in contrast, is favored in the picture: double-breasted suit, a hat with a thin brim, the smile pleasant: a man of thirty who looks at the world straight on. In the back, blurred and almost out of focus there is my mother, so young that at first it was hard for me to recognize her.’

Una and *historia* each help describe the same thing and both are Class A signs following Strategy 1. *Primer* and *libro* describe the same thing and both are Class B signs. *Una* and *carta* describe the same thing and are both Class A signs, and the same for *la* and *carta*. The same too for *una* and *foto*, *una* and *rana*, *esa* and *fotografía*, *ala* and *fina*, and *la*, *sonrisa* and *campechana*. As mentioned above, it seems likely that a meaning could be assigned to *-ado*, so I am tentatively treating it as a Class B sign. It was chosen as a suffix to *cruz-* rather than *-ada* in example (9) because *cruzado* describes the same thing as does the Class B sign *traje*. (The choice of *desnudo* will be dealt with later.)

Nearly all instances of Strategy 1 in example (9) involve adjacent words, but two involve words in different clauses: *Al fondo*, *borrosa y casi fuera de foco*, *aparece mi madre*, *tan joven que al principio me costó reconocerla*. Class A *borrosa* and *-la* rather than Class B *borroso* and *-lo* were chosen because Class A *madre*, *borrosa* and *-la* all describe the same person.

I chose to illustrate Strategy 1 with a passage from a novel because, to recall, CS analyses are accounting for the distribution of signals in spoken and written texts, not speakers’ introspective judgments. More pointedly, this analysis is accounting

for actually observable facts of distribution, such as the fact that *una historia* occurs frequently in speech and writing when describing a single thing but *un historia* rarely if ever does; the analysis is not accounting for the fact that ¿*Hay una historia?* is regarded, in some analytical traditions, as a grammatical sentence and ¿*Hay un historia?* as ungrammatical. Grammaticality, like agreement, is one of the notions Diver set aside in establishing a scientific foundation for linguistics.

Strategy or rule?²⁴

Readers may well wonder if Strategy 1 is simply an agreement rule by another name. What's the difference between a rule and a strategy? The theoretical difference is that an agreement rule (in a generative grammar at any rate) is a formal construct of mental grammar, with the same status as the constructs of a CS mental grammar, like signs, grammatical systems, and gender classes. By contrast, a strategy is not part of propositional knowledge of grammar *per se*; it is procedural knowledge of its use. A strategy is thus a regular way speakers deploy features of grammar in communication. This is why I expressed Strategy 1 as an imperative addressed to the speaker.

In my analysis, the two gender classes are hypotheses about mental grammar, while Strategy 1 is a hypothesis about how speakers regularly exploit the existence of these classes. It thus has the same theoretical status as one of Grice's Maxims, a practice that facilitates successful communication. In this case, Strategy 1 helps hearers recognize that two or more signs (e.g., an article and its noun, or a pronoun and its antecedent) should be construed together as describing the same thing.

This theoretical difference has an important consequence with respect to falsification. A traditional agreement rule is a formal hypothesis that is tested by repeatedly exposing it to opportunities for falsification. Positive data count for little; the rule is supported by escaping falsification on occasions it could be falsified. A single negative datum does not, however, falsify a proposed strategy; it simply shows that other strategies of use are being employed. Nevertheless, a (hypothesized) strategy is still open to falsification because it gives rise to quantitative predictions that if unconfirmed would falsify it. For instance, Strategy 1 predicts that, in any spoken

24. Communicative strategies were introduced by Diver as early as 1970 in a classroom handout (Diver 2012b: pp. 257–260) and figure, either explicitly or implicitly, in many CS analyses (e.g., García, 1975: p. 61; García & Otheguy, 1983; Diver, 1995: pp. 104–110; Reid, 1991, Chapter 4; Huffman, 1997: p. 214; Otheguy & Stern 2000). However, their theoretical status has never been clear. The position expressed in this section is the author's proposal for clarifying their status.

or written text when two gendered signs describe the same thing, the number of such pairs belonging to the same gender class will exceed the number of such pairs belonging to different gender classes at a greater than chance level. This prediction was tested on the first 50 tokens of such pairs in the text from which Example 9 was drawn and it was confirmed by 100% of the tokens to which it applied. This result supports both the hypothesized gender classes and Strategy 1.

The need to posit a second communicative strategy

We have seen that Strategy 1 is a hypothesis about what speakers *do*. But speakers do not always follow Strategy 1. Consider example (10), from a baseball instructional manual.

(10) ...se sigue la misma técnica que utiliza *el primera base* ...

‘...the technique that is used is the same as that of *the first baseman* ...’

Ealo de la Herrán, Juan. *Béisbol*. Editorial Pueblo y Educación, 1984

The word *base* is a Class A word, and Class A *primera* is chosen according to Strategy 1. But *el*, a Class B word, is also chosen, and this choice does not follow the principle laid out in Strategy 1. (Strategy 1 would have yielded *la primera base*.) If Strategy 1 were recast as a hypothesized component of mental grammar, example (10) would falsify it. As a strategy, however, the example simply shows that speakers sometimes choose between synonymous signs on some other basis.

We know (independently of example (10)) that speakers cannot always be operating on Strategy 1 in choosing gendered words because Strategy 1 is not always applicable. In the examples below, *un* is a Class B word and *una* is a Class A word but *joven* is ungendered. Speakers cannot be following Strategy 1 because Strategy 1 applies to the word choices in sequences of two or more gendered words, not to instances of single words, as is the case in examples (11) (Class B *un* and ungendered *joven*) and (12) (Class A *una* and ungendered *joven*).

(11) *un joven* ‘a youth’ (male)

(12) *una joven* ‘a youth’ (female)

What would make a speaker choose *un* in (11) and *una* in (12)? The message difference (a male vs. female youth) is the clue, but that difference of sex cannot be due to systemically encoded meaning because, to recall, *un* and *una* do not (by hypothesis) differ in meaning, only gender class.

Earlier I said that the make-up of the two gender classes was semantically arbitrary, but this is not completely true because, as is well known, there are pockets of semantic motivation. All the words whose meanings explicitly include the notional

Class A		Class B	
<i>hembra</i>	'female'	<i>varón</i>	'male'
<i>mujer</i>	'woman'	<i>hombre</i>	'man'
<i>madre</i>	'mother'	<i>padre</i>	'father'
<i>hija</i>	'daughter'	<i>hijo</i>	'son'
<i>esposa</i>	'wife'	<i>esposo</i>	'husband'
<i>niña</i>	'girl'	<i>niño</i>	'boy'
<i>azafata</i>	'stewardess'	<i>sobrecargo</i>	'steward'
<i>hermana</i>	'sister'	<i>hermano</i>	'brother'
<i>nuera</i>	'daughter-in-law'	<i>yerno</i>	'son-in-law'
<i>profesora</i>	'female professor'	<i>maestro</i>	'male teacher'
<i>ministra</i>	'female minister'	<i>ministro</i>	'male minister'
<i>regenta</i>	'female regent'	<i>regent</i>	'male regent'
<i>alcaldesa</i>	'female mayor'	<i>alcalde</i>	'male mayor'
<i>hombria</i>	'manliness'	<i>macho</i>	'male, macho'
<i>caballerosidad</i>	'gentlemanliness'		

Figure 4. Gender class of some words whose meanings specify male or female

component 'female' are in Class A and almost all words whose meanings explicitly include the notional component 'male' are in Class B.²⁵

This semantic consistency in the make-up of the two gender classes establishes associations in speakers' minds. Specifically, the regular pairing (following Strategy 1) of *hombre*, *hijo*, and *padre* with *el*, *un*, and *eso* has contaminated the latter with the idea of a male referent. The notion 'male' is a component of the meanings of *hombre*, *hijo*, and *padre* and other sexed words but absent in the meanings of *el*, *un*, and *eso*. Likewise, the regular pairing of *mujer*, *hija*, and *madre* with *la*, *una*, and *esa* has contaminated *la*, *una*, and *esa* with the idea of a female referent. These sex connotations then affect the choice between *el* and *la*, *un* and *una*, *eso* and *esa* when sex is relevant to the message and when Strategy 1 is not applicable.²⁶ This influence will be called Strategy 2 for choosing gendered words.

25. I say 'almost all' because *hombria* ('manliness') and *caballerosidad* ('gentlemanliness') are in Class A, not B. The impression among people without linguistic training that the Spanish gender classes are based on a distinction of sex (a misapprehension perpetuated by the misleading terms *masculine* and *feminine*) would seem to come from the fact that this small island of semantic motivation based on sex includes a dozen words of extremely high frequency: *mujer*, *hombre*, *madre*, *padre*, *hija*, *hijo*, *niña*, *niño*, *hermana*, *hermano*, *esposa*, *esposo*.

26. My thinking about the connotations of words due to their gender class has been heavily influenced by Ellen Contini-Morava's work (2002) on the far more complex gender system of Swahili. Contini-Morava reviews the pros and cons of positing a meaning for each of the Swahili noun classes, positing a semantic network for each, or allowing the notional associations among the

STRATEGY 2: In constructing a discourse, choose between signs with the same meanings but in different gender classes on the basis of their connotations of male or female reference.

In (11), *un* implies the *joven* ‘youth’ is male because *joven* describes a young human being and *un* regularly co-occurs with signs whose meanings specify ‘male’; and in (12), *una* implies the *joven* is female because *una* regularly co-occurs with signs whose meanings specify ‘female.’²⁷

Strategy 2 is not restricted to ungendered words like *joven* and *estudiante*; it is used with gendered words as well. Returning now to example (10) (*el primera base*), it would appear that the writer is following Strategy 2 in choosing *el* rather than *la* to pair with *primera base*; *el* implies that *primera base* describes a male. To make this case, however, requires an examination of the expressive potential of the word *base*. *Base* has a meaning which in a baseball context can cover the white pads on the field, the game positions that those pads mark, or the person (male or female) playing the positions. This latitude of interpretation is illustrated in (13) below; the first appearance of *la tercera base* is interpreted as the base on the ground (or the position), and the second appearance of *la tercera base* is interpreted as the player, ‘the third baseman.’ Note that on both occasions, the writer is following Strategy 1 not Strategy 2 in choosing *la* and *tercera*.²⁸

- (13) Usualmente el jugador que defiende **la tercera base** es el que está más cercano al bateador de turno en el *infield*, por lo que se le conoce como *antesalista*. **La tercera base** tiene que esperar que la pelota del bateador venga.

‘Usually the player who defends [the] **third base** is the one in the infield nearest the hitter taking his turn, and for that reason is known as the foyer player. **The third baseman** has to wait until the hitter’s ball comes ...’ Wikipedia

words of the classes to reside in speakers’ experience with the classes rather than made formally explicit in their mental grammar. In the end, she chooses the third option as I do here.

27. The tendency to use *los* rather than *las* with an ungendered word like *joven* for reference to a mixed-sex plurality would seem to be a case of cultural sexism: treating males as the norm. But note this sexism is less pernicious than when some English speakers use *he*, *his*, and *him* for mixed-sex reference, e.g., ‘everyone should keep *his* passport with *him*.’ The systemic meanings of *he*, *his*, and *him* certainly appear to include the notion of ‘male,’ whereas the meanings of *el* and *los* do not. Putting it another way, the sexism in English involves the denotation of words (i.e., their inherent meanings), while the sexism in Spanish involves their connotation.

28. Since tokens of Strategy 1 far outnumber tokens of Strategy 2 in running text, hearers assume speakers are operating on Strategy 1 unless there is reason to think otherwise. Thus, they do not interpret the *la* of *la tercera base* as chosen (following Strategy 2) because the player is female, because there is no reason for a writer to be following Strategy 2 in a generic description of the rules of baseball where there is no specific real-world referent.

The author of example (14), on the other hand, alternates between Strategy 1 and Strategy 2 with the word *base* depending on whether the word refers to the (male) player or the inanimate base.

- (14) La colocación **del** [de+el] **tercera base** depende fundamentalmente de las características del bateador que ocupa su turno al bate ...

‘the positioning of **the third baseman** depends fundamentally on the characteristics of the batter taking his turn at bat ...’

En situaciones de toque de bola, con **la primera base** ocupada, se coloca sobre el borde de la hierba y cuando observa que el bateador ha dejado caer el bate, ... **el tercera base** tiene que conocer la habilidad del lanzador ...

‘In bunt situations, with **first base** occupied, [he] goes to the edge of the grass and when he observes that the batter drops the bat, ...**the third baseman** has to know the skills of the pitcher ...’

Ealo de la Herrán, Juan. *Beisbol*. Editorial Pueblo y Educación, 1984

Examples (13) and (14) might be cases of inter-speaker variation; Ealo de la Herrán uses Strategy 2 with *base* when referring to a player and the Wikipedia writer of example (13) uses Strategy 1. But other writers alternate in the same text between Strategy 1 and 2 with *base* when referring to a player.

- (15) Es solo en esta acción que tiene ventaja **el primera base** que juega de derecha. ‘It is only in this move that the right-handed **first baseman** has an advantage.’

La regla más antigua del béisbol es la que dice: “tener el ojo sobre la pelota.” En la recogida efectuada por **la primera base** esta regla es de la mayor importancia. Debe tener muñecas flexibles, manos hábiles y un perfecto sentido de toque y de tiempo.

‘The oldest rule in baseball is “keep your eye on the ball.” This rule is of great importance for the pickups realized by **the first baseman**. He must have flexible wrists, skillful hands ...’

Andrés de Avila, *Beisbolexplicado.com*

Strategy 2 is not constrained to the single word *base* in baseball discourse. Example (16) is from a rule book on bullfighting. *Espada* ‘sword’ is a Class A word but is paired with Class B *el* and *los* to imply that *espada* is describing a male, and hence a living being, not an inanimate object.

- (16) **Los espadas** matarán el número de toros que les corresponda, en conformidad con lo anunciado en los programas y en caso de accidente **el espada** más antiguo de los que queden en el redondel estoqueará los que le correspondían al compañero herido.

‘**The swordsmen** will kill the number of bulls that corresponds to them, according what is announced in the program. In case of an accident, **the swordsman** with the most seniority left in the ring will fight those [bulls] that belonged to the injured man.’

Ahumada, Miguel. *Reglamento para corridas de toros y novilladas*. Jalisco, México, 1908: p. 13

Example (17), from a novel, shows the same thing, Class A *espada* ‘sword’ paired with Class B *el*.

- (17) Alardeaba de ser el mejor amante y **el mejor espada** de toda la monarquía de Su Majestad don Felipe y aun de la Francia y la Inglaterra.
 Segarra Berenguer, Manuel, *Cruces de seda*. 2006: p. 84
 ‘He boasted of being the best lover and **the best swordsman** of His Majesty’s entire kingdom, and even of France and England.’

Again, Strategy 2 (selecting words because of their sex connotation) is used here to help the reader interpret *espada* as ‘swordsman’ rather than ‘sword.’

In (18), Class B *primer* ‘first’ is chosen to pair with Class B *violín* ‘violin’ following Strategy 1. But Class A *la* is chosen to pair with *primer violín* following Strategy 2 to imply the violinist is female.

- (18) En cuanto los integrantes del cuarteto pisaron el escenario, todas las miradas se dirigieron a **la primer violín**, bella y rubia, que con un maquillaje algo espeso en la cara y unos labios pintados de rojo oscuro lucía un vestido largo y negro ...
 Alfonso Rey, *El escándalo de Julia*, 2005: p. 137
 ‘The moment the members of the quartet stepped onto the stage, all eyes were on **the first violin**, blonde and beautiful, who with somewhat thick make-up and dark lipstick was wearing a long black dress ...’

But in (19), both Class A *la* and *primera* are chosen to pair with Class B *violin* following Strategy 2.

- (19) Desde su puesto en el extremo superior izquierdo de la quinta fila de la orquesta, observaba con admiración las brillantes ejecuciones de **la primera violín**, una mujer elegante y pelirroja que se mecía con voluptuosa cadencia al ritmo de los vaivenes de su arco ...
 Juan José Isac, *El hombre que sólo podía quedarse un ratito*. 2012: p. 95
 ‘From his seat in the top left of the fifth orchestra row, [he] observed with admiration the brilliant executions of **the first violin**, an elegant redhead woman who swung with voluptuous cadences following the movements of her bow.’

Examples (18) and (19) show that the sex connotations of Class A and B articles and deictics have marginally affected other lexical signs in the two classes. In (18), Class B *primer* was chosen over Class A *primera* because both *primer* and Class B *violin* describe the same person, a violinist. In (19), on the other hand, *primera* was chosen (along with *la*) to imply that the violinist is a woman following Strategy 2. A passage that we saw in example (9) shows the same thing.

- (9') Con la carta viene una foto donde me tiene en brazos: **desnudo**, estoy sonriendo, tengo tres meses y parezco una rana.

'With the letter came a picture where [he] has me in his arms: *naked*, [I] am smiling, [I] am three months old and [I] look like a frog.'

Class B *desnudo* was chosen over Class A *desnuda* to imply that the narrator, writing in the first person (*estoy*, *tengo*), is a man. Note that Strategy 1 does not apply here because even though *desnudo* and the meaning SPEAKER of *estoy* and *tengo* describe the same person, neither *estoy* nor *tengo* is gendered, and Strategy 1 only applies to the choice of two or more gendered signs.

Hypotheses about mental grammar

Strategy 2 allows us to better understand the rationale for casting the principles applicable to synonymous signs (e.g., *el*, *la*; *blanco*, *blanca*) as communicative strategies rather than as grammatical rules. The two strategies mandate different grammatical choices; following Strategy 2 as in example (19) (*la primera violín*) means not following Strategy 1. A formal mental grammar (of the generative kind at any rate) is like the multiplication tables in that it has no temporal dimension. As such, it must be internally coherent; so positing components that contradict each other would amount to saying it is (simultaneously) in two different states. If, then, Strategies 1 and 2 were to count as grammatical rules, each would have to be constrained to apply in different contexts so that they do not make conflicting predictions. In my opinion, this is not feasible because those contexts would have to be defined in purely formal, structural terms. No appeal could be made to real-world reference, interpretation, or speaker intent because a mental grammar (of either the generative or CS kind) is a linguistic object idealized as existing independent of the human being in whose brain it is lodged.

Communicative strategies, on the other hand, are hypotheses about principles reflected in people's choice of signs, a kind of behavior; and people behave differently on different occasions without being guilty of self-contradiction. There is no reason for assuming prior to analysis that all of people's linguistic behavior is rule-governed; better to expect that some aspects of language use will prove to be rule-governed, while others will prove to be goal-directed; and one cannot know in advance which is which.²⁹

29. For a development of this distinction, see Reid 1991: pp. 8–9, 26, 30, 93–6, 346, 358, 368.

Analytical indeterminacy

It is not always possible, however, to determine which strategy a speaker is following on a given occasion because a good deal of idiosyncrasy exists in the gender status of words such as *médico* ‘doctor,’ *ministro* ‘minister,’ *juez* ‘judge,’ etc. I have not done a case study of *médico*, but I speculate its history could have gone like this. Originally, *médico* was a Class B word for all Spanish speakers, and the *el* of *el médico* ‘the doctor’ was chosen following Strategy 1. At this point, ‘male’ was presumably not a semantic component of the meaning of *médico* as *el médico* was used even for the rare female doctor. With the appearance of more female doctors, some speakers began to differentiate the males from the females by applying now Strategy 2 not Strategy 1: *el médico*, *la médica*. For these innovators, *médico* was still a Class B word without a sex specification. Other speakers responded to the new social situation with a Class A neologism: *médica*, with ‘female’ now an explicit component of its meaning. This had the effect of changing the meaning of *médico* (for these speakers), making ‘male’ an explicit component of its meaning as well. When these speakers choose *la* with *médica* and *el* with *medico* they are presumably back to applying Strategy 1. Clearly, then, in order to say with certainty what an individual speaker is doing in choosing *el* with *médico* on a particular occasion, one would need to know a lot more about his or her use of that word; whether that speaker chooses *la* on other occasions; and whether or not the speaker’s active lexicon includes *médica* (also *ministra*, *jueza*, etc.).

Summary

Part II of this paper has allayed the suspicion that CS, despite its critique of traditional *a priori*, might harbor an *a priori* assumption of its own; namely, that the only theoretical construct in language is the linguistic sign. As I have shown here, the criterion for allowing a construct into CS theory is not that the construct be a sign, but that it be an explanatory link in the demonstration that aspects of the acoustic asymmetry of vocal sound are manifestations of people attempting to communicate by means of a code system. In Part II, we’ve seen examples of constructs other than signs that must be rejected (e.g., nouns, verbs, adjectives, and gender agreement in English or Spanish) as well as constructs other than signs that must be incorporated into CS theory (e.g., conjugation classes, declension classes, gender classes, and communicative strategies).

The puzzle of grammatical gender for a functionalist

In the long-running debate about the innateness of grammar, one of the reasons cited for the supposed innateness of certain linguistic categories and structures is that they are not necessary for communication. A prime example is the gender classes of the Romance, Germanic, Hellenic, and Slavic languages. No one would design a language with semantically arbitrary gender classes if beginning from scratch. The puzzle of grammatical gender in the various European languages is not, however, as great as it might first appear. They are not cases of independent development; they all evolved from a single gender system in Indo-European. Still, one may well ask why even *one* gender system would arise by natural processes if, as CS claims, language is essentially structured for communication.

Answering this question goes far beyond the scope of this paper; but I'm tempted to offer a suggestion. It seems to me that the question arises from a misunderstanding of evolutionary theory, one especially prevalent among people wanting to reconcile evolutionary theory with the desire to see purpose in nature. A key feature of Darwin's theory of biological evolution is that it is purposeless. Nothing develops because of need or design. But things are gradually eliminated if they are detrimental to the organism's survival and reproduction, and perpetuated if they facilitate survival and reproduction. This means, for instance, that animals did not evolve eyes because they needed to see; quite the reverse. Animals (now) need to see because they have evolved eyes. That is, because they began to develop a sensitivity to light gradually leading to rudimentary eyes, they concomitantly developed modes of survival and enhanced reproductive opportunity increasingly dependent on sight. The same logic holds for linguistic evolution. English didn't acquire the phoneme /ŋ/ in the 16th century because speakers needed a third nasal phoneme; but once they had it (due to blind sound change), they relied on it to distinguish words, e.g., *sin* from *sing*.³⁰ Similarly, the meaning of the English word *silly* did not evolve from 'saintly' to 'amusingly inept' because speakers needed a synonym for *foolish*; but once they had it (due to children's misanalysis), it proved useful in distinguishing different kinds of ineptness.

The legitimate question to be asked regarding the gender classes in the Indo-European languages is why those classes have persisted for six thousand years when other structural features that would seem to have greater communicative

30. /ŋ/ was originally an allophone of /n/ preceding /k/ and /g/. It became a phoneme when word-final /g/ following /n/ weakened and finally disappeared, putting [ŋ] into contrastive distribution with [n] (Barber, 1993: p. 195).

value have disappeared.³¹ The point above about a new phoneme and a new meaning suggests an answer. No language needs gender classes; but once it has them, they give rise to communicative strategies that make the classes functionally useful in small ways, which then perpetuates them. The various synchronic functional CS accounts of gender that Contini-Morava (2002), Diver (2012b), Zubin and Köpcke (1981), Otheguy and Stern (2000), and Otheguy and Lapidus (2005) have developed do not explain why Latin, Spanish, German, and Swahili have gender classes; rather, they explain why those languages *still* have gender classes: why they survive; why new generations of children bother to learn and maintain them. Children continue to learn them because they have been integrated into the pragmatics of language use in ways that facilitate communication.

The unification of Otheguy's two strands of research

Edward Elgar's 1901 orchestral composition *The Enigma Variations* is a set of variations on a theme, each variation portraying a different friend of the composer's. But the theme itself is not stated musically, and Elgar never revealed its identity, a mystery that has intrigued musicologists for a century.³² As in that piece, a theme runs through Otheguy's published work that is not explicitly stated. It arises from an anomaly in the field of sociolinguistics, Otheguy's research focus. The anomaly is this. Sociolinguistics studies patterns of language use in speech communities, and it uses transcribed corpora of naturally occurring speech as its primary database. Yet the nascent field's early pioneers adopted for their theoretical foundation a linguistic theory that excluded facts of language use as outside its purview and irrelevant to its theory making. They hitched their wagon to a theory whose goal was to create a formal mechanism that would produce structural descriptions of grammatical sentences, a theory that by declaration and design had nothing to say about observed language use. Much ink and effort has since been spent over the succeeding decades to repair that initial mistake by amending the model with such things as variable rules and constraint hierarchies. But the amendments, while

31. Treating the evolution of a gender system and its preservation as two separate problems follows from Saussure's distinction between diachronic and synchronic linguistics. They are two separate problems because they call for different kinds of explanation: (causal) historical explanation, and (non-causal) functional explanation.

32. Normally, musical variations follow a statement of the theme on which the variations are based, e.g., Beethoven's variations on 'God Save the King,' Brahms 'Variations on a Theme by Haydn,' and Schubert's 'Trout Quintet,' variations on the melody of Schubert's song 'The Trout.'

allowing research to proceed, rendered the field's theoretical framework conceptually incoherent.³³

Otheguy targets this theoretical incoherence in a critique of several studies of perseveration (Otheguy, 2015), arguing that a theory of mental grammar designed to account for language introspection cannot also account for language use. His conclusion is that a field defined as the study of language use should be operating with a linguistic theory that takes language use as its object of explanation. However, he does not mention any theory by name. The theme I see Otheguy hinting at here is this: the goals of sociolinguistics would be better served by the sign-based theory of linguistic structure offered by CS than by the Chomskyan sentence-based theory.³⁴

Such a shift would, however, require a reconceptualization of the object of study. The usual answer to the question 'what does sociolinguistics study' is 'variable phenomena.' But the sociolinguistic notion of variation is entirely created by the generative model of grammar the field has adopted; variation covers those aspects of language use that are not determined by the model's categorical rules. This formulation of variation, however, often makes it difficult to offer grammatical explanations of variation that are rooted in grammatical structure (since the variation lies 'outside' it). By contrast, if variation is defined as speakers choosing (or not choosing) signs from among those whose meanings make them viable

33. For instance, the early introduction of variable rules to the generative model made as little sense as would revising the multiplication tables by adding coefficients reflecting the frequency with which people 'use' each equation. Variable rules seemed plausible at the time because people took a competence grammar as a model of mental processes. But they were being misled by its powerful metaphors. They were interpreting such terms as generate, transform, embed, move, leave a trace, and delete literally as naming mental events, when in fact the terms are a typology of equivalence relations holding between structural descriptions at different levels of abstraction; equivalences that are non-directional, and *always* true (like those in the multiplication tables). Chomsky repeatedly warned against mistaking a generative grammar for a process model, describing such an interpretation as "strange" and "absurd"; it would, he said, be mistaking a competence model for a performance model (see Chomsky, 1965: pp. 139–141). On the other hand, it was this metaphorical interpretation of the model that has made generative grammar so appealing, and hence a misinterpretation generative linguists rarely bother to correct.

34. Many sociolinguists may not think of themselves as operating on a generative model. To be sure, the version of the model underlying sociolinguistics is from a much earlier time, a time when the research agenda was to write actual competence grammars of individual languages. But no alternative theory of linguistic structure has been provided to give a theoretical foundation for such terms still regularly occurring in the sociolinguistic literature as sentence, grammar, grammaticality, grammatical rule, rule-governed, underlying structure, gender agreement, subject-verb agreement, categorical, variable, subject, delete, null pronoun, dative alternation, and the sociolinguistic bias against functional explanation.

expressive options in certain contexts, a grammatical explanation of variation is always a strong possibility.³⁵

A striking example of the possibility of explaining sign-based variation that is foreclosed by the generative model is to be found in the sociolinguistic literature on the use of subject pronouns in Spanish. Variation as that term is defined in generatively based sociolinguistics requires two variants that are grammatically equivalent. Thus, the distribution of subject pronouns in the written transcripts of conversations is described as the alternation between the *overt* and the *null* forms of the same pronoun.³⁶ But as Otheguy and Zentella (2012: p. 9) point out, describing the variation that way precludes a true theoretically grounded explanation. The terms *overt* and *null* portray it as a kind of allomorphic alternation, whereas the relevant grammatical distinction is a difference in signaled information: more information is signaled when the pronoun is present than when it is absent. Otheguy (2015) found in his data that so-called overt pronouns favor a verb whose subject is referentially different from the subject of the previous verb (a situation called *switch reference*); in such cases, the hearer's task of comprehension would be made easier with more signaled information to go on.

To be sure, many analysts prior to Otheguy had recognized that the favoring of overt pronouns for switch reference is amenable to functional explanation (e.g., Cameron, 1995). But that functional explanation, despite its eminent reasonableness, has no grounding in generative theory. Specifically, the notion of 'more signaled information' has no theoretical grounding, because the structural description of a sentence with a null pronoun looks practically identical to the structural description of a sentence with its overt variant; there is no difference in 'information.' Moreover, a competence model does not posit meanings for grammatical morphology such as pronouns to signal; so appealing to the communicative utility of a pronoun's meaning has no theoretical support. The idea of 'information signaled by the speaker to the hearer' only makes theoretical sense in the context of what might be called, following Chomsky (1965: pp. 10, 140), a 'performance model'; a grammar designed to account for speech.³⁷

35. For an example, see Nancy Stern, "Ditransitives and the English System of Degree of Control" in this volume, a treatment of so-called dative alternation.

36. Positing null subject pronouns fulfills the requirement that all sentences have subjects, which is, in turn, needed so as to maintain the hypothesis of subject-verb agreement.

37. Chomsky envisioned a performance grammar as incorporating a competence grammar as one component, but did not specify how exactly it would contribute to an account of speech. It would seem that the issue of its inclusion would be best resolved by first trying to formulate a performance grammar along the lines of a CS grammar. That would reveal what aspects of speech (or language use) are left unaccounted for in the absence of a competence grammar.

Critical for such a grammar is a place for speakers. Speakers must be recognized at the theoretical level as active players freely making choices offered by their grammars. This is possible for a CS version of mental grammar but difficult for the generative version. In Otheguy's analysis choosing whether to use a subject pronoun is the prerogative of the speaker not the grammar. By contrast, a generatively based treatment must incorporate that decision into the grammar itself because such a grammar must produce structural descriptions of sentences in a purely mechanical way without the intrusion of a speaker; a generative grammar is designed for an automaton not for a person.

For a CS analyst, the so-called variable use of a sign is part and parcel of the same problem as its so-called obligatory use, and it yields to the same explanatory factors. Thus, in Otheguy's analysis, the choice to use or not use *yo* in contexts where its meaning fits the message (e.g., *yo canto* ~ *canto* 'I sing') is accounted for in the same way as the choice of *yo* over *tú* (e.g., *yo canto* 'I sing' ~ *tú cantas* 'you sing'). Both are dealt with in terms of the communicative utility of the meanings of the pronouns involved. This makes sense because both are really the same choice, just considered from different perspectives.

We can now see that what is blocking this unified account in sociolinguistics is the pernicious notion of *grammaticality*. The centrality of grammaticality in defining the objectives of the generative competence model mandates that the fact that *yo* rarely if ever directly precedes *cantas* be given a different kind of account (namely, a formal rule of subject-verb agreement) than its appearance or absence preceding *canto*. Once the concern with grammaticality is set aside however, the two facts are recognized as different aspects of the same distributional problem: the distribution of *yo*.

Abandoning grammaticality as the theoretical foundation of linguistic theory allows, as argued above, for the integration of so-called variable aspects of language use into the main body of facts traditionally open to systemic linguistic explanation: the so-called obligatory facts of distribution. This move also does the reverse. It reveals that the facts thought of as obligatory can be reconceived as facts of variability. Consider the analysis of gender presented in this paper. By all rights, the differing distributions of the words *blanca* and *blanco*, *el* and *la* should count as problems of variation. *Blanca* and *blanco* are two signs in speakers' mental grammars of equivalent expressive value (i.e., two variants with the same meanings) whose alternation in discourse – *la casa blanca*, *el libro blanco* – poses a distributional problem. It is only the theoretical primacy accorded to the introspective judgment that *la casa blanco* and *el libro blanca* are ungrammatical that obliges the generative sociolinguist to set aside the alternation of *blanca* and *blanco* (in *la casa blanca* and *el libro blanco*) as instances of obligatory usage rather than variable usage. For the CS linguist aiming to account for language use, not grammaticality

judgments, the puzzling alternation in discourse between *blanca* and *blanco* counts as a problem of variation every bit as much as the puzzling presence and absence of *yo* (*canto, yo canto*).

The solution to the problem above involved gender classes within the grammar and two communicative strategies that despite lying outside the grammar itself do appeal to features within the grammar, namely the differing gender assignment of synonymous signs. Similarly, in Otheguy's 2015 account of the distribution of subject pronouns, speakers choose whether to use a subject pronoun (a unit within the grammar) depending on the utility to the hearer of the (hypothesized) information that the pronoun signals. This, too, amounts to a communicative strategy outside the grammar. So these two distributional problems, one traditionally handled by a gender agreement rule and the other traditionally handled by variable constraints, can now be solved in the same way.

Otheguy's innovative work gives promise that one day the distinction now made between so-called categorical phenomena and variable phenomena might melt away, and with it the divide between the mental-grammar theorist and the field sociolinguist. The two specialists might one day unite in a common endeavor of accounting in a uniform and comprehensive way for the distribution of linguistic morphology. We have at hand a scholar eminently qualified to show us that way. Let us hope that in his years of retirement Ricardo does not follow Elgar and take his important theme to the grave.

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Spooky grammatical effects

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Linguistic constructs often correspond to nothing concrete: Descriptivists' *zero morpheme*, generativists' *trace*, variationists' *null instantiation*, and Columbia School's *null signal*. These represent structural relations with no phonetic substance. Columbia School has posited, moreover, three types of relatively insubstantial semantic structure: The *residual member* and the *including member* are semantic value oppositions within a grammatical system, each defined by the system's other members. In the *opposition of substance*, in my own work, two signals share a value from one semantic substance, but only one of them bears a meaning from an additional semantic substance. All of these constructs are justified by distributional facts within theoretical paradigms. It is possible, too, to distinguish between all of the above and *absence*.

Keywords: zero, null, Columbia School, residual member, including member, opposition of substance

The idea in linguistics that the absence of an overt structural element can have real consequences – that *nothing* can be *something* – may perhaps forever and rightly be analytically suspect. But it does have a long pedigree and a successful track record. For instance, various null elements have been proposed in phonology, morphology, and grammar. And, in addition to discrete null elements, there are broader types of structural distinctions involving the absence of overt structure. Evidence continues to support the position that structural absence has a role to play in linguistic theory. And linguistics is far from unique in this respect; absence plays roles in such other realms of human experience as physics, mathematics, sport, music, and anthropology.

Any misgivings in linguistics about the absence of substance have august precedent. Albert Einstein's discomfort with the idea in quantum mechanics that measurement of a particle in one place can have an effect on a particle in a completely different place was famously expressed in his phrase "spooky action at a distance." Yet experimental evidence has long since backed up the notion of such

“entanglement” of particles. Similarly, in linguistics, decades of work have supported the position that nothingness is a force to be reckoned with.

There is no need now, therefore, for a paper laying out a general theoretical consideration of nullity in linguistics, and certainly no need to justify the practical uses that have been made of various nulls. This paper, instead, will trace a development from the very early days of the field to the most recent developments in Columbia School and variationist linguistics. This is the path that leads to the work of Ricardo Otheguy and that stands to inform the work of linguists who will continue to benefit from his influence. This path runs from the American Descriptivist null or zero element in phonological and morphological paradigms, through the empty categories of later formal syntax and – contemporaneously but not compatibly – the organization of Columbia School’s grammatical systems, extending then to more recent work that expands Columbia School theory and to Otheguy’s own constructive critique of variationist linguistics. In terms of forebears to Otheguy, this treatment will touch upon, among others, Saussure, Bloomfield, Harris, Chomsky, Diver, Labov, and García. Throughout the paper, analogies will be made with other, nonlinguistic human behaviors, in keeping with the view that human language, far from being modular, is instead “entirely consistent with the way any other form of everyday human activity is carried out” (Diver, 1995/2012: p. 485).

1. The null in mathematics

While quantum mechanics represents human efforts to understand physical phenomena, the null – or empty, or zero – element has played an important role too in fields that deal with human mental concepts. So mathematics, for instance, has its *empty set*. Consider the three simple equations and the sets of solutions each has in real numbers in Diagram 1.

	I	II	III
Equation:	$x^2 = 9$	$2x = 0$	$x^2 = -1$
Solutions:	$\{-3, +3\}$	$\{0\}$	$\{\}$
Size of set:	2 solutions	1 solution	0 solutions

Diagram 1. Null in mathematics

Equation I has two solutions, -3 and $+3$; that is, the set of solutions to Equation I has two members. Equation II has one solution, the real number 0; the set of solutions to Equation II has one member. Equation III has no solutions on the real number line; the set of solutions to Equation III has no members. Mathematics

calls $\{\}$ the *empty set*. Another symbol for the same concept is \emptyset . Of course, mathematics is not linguistics, and empty sets in mathematical set theory do not relate to sets of real numbers in the same ways that null elements in linguistics relate to overt elements in linguistics. Still, mathematics does offer another realm of human experience in addition to linguistics where a full understanding requires the postulation of a kind of emptiness. Besides, in “ \emptyset ,” mathematics furnishes a handy symbol for linguists to use.

2. The null in semiotics

Conceptual uses of the empty structural element include not only the mathematical but also the semiotic. Consider first baseball, and then language.

In baseball, it is the job of the umpire to judge whether each pitch that comes towards the batter is a good pitch or a bad pitch. Those are the only two possibilities. A good pitch is called a *strike*; a bad pitch is called a *ball*. A strike, or a good pitch, is a pitch that the batter should reasonably try to hit. If the batter does not swing at a good pitch, the umpire calls a strike and thereby penalizes the batter’s team. A ball, or a bad pitch, is a pitch that the batter should not be expected to try to hit. If the batter does not swing at a bad pitch, the umpire calls a ball and thereby penalizes the pitcher’s team. To convey to the assembled crowd of spectators his judgment of each pitch, the umpire employs visual *signals* – to use the semiotic term – for strike and ball: To signal “strike,” the umpire visibly moves one arm, typically extending it at an upward angle, and to signal “ball,” the umpire does nothing, see Diagram 2.

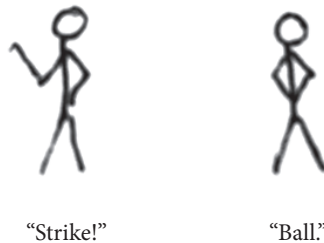


Diagram 2. Null in baseball

In the closed semiotic system that is shared by the umpire, the players, and the spectators, that absence of movement by the umpire is significant; it conveys a *meaning*. Baseball uses a null element.

2.1 Linguistics

Linguistic theory has made extensive use of nothingness. This has been the case even though it has long been recognized that language-users' pragmatic interpretation in discourse goes "beyond what sentences actually say" (Li & Thompson, 1979 p. 312), even to the point that, according to Ono and Thompson (1997: p. 489), if inference in communication were properly taken into account, then the syntactic "notion of 'zero'" that they have in mind "would play no role." That is, linguistic theory under-represents the messages for whose communication humans use language. This view (that grammar falls short of accounting for communication) is essentially compatible with Columbia School's distinction between signaled *meaning* and inferred *message* (Diver, 1974/2012: p. 31, 1975/2012: pp. 48–54).

Nevertheless, the null in linguistics has been heavily relied upon.

Typically, in lexicon, phonological distinctiveness is crucial for keeping lexical items apart. So *bear* needs to be pronounced distinctly from *beer*, if miscommunication is not to ensue. Nevertheless, homonymy – the absence of a phonological distinction – is commonplace in lexicon, as in "bear the burden," "trap a bear," and "bare one's soul" – plus "the undertaker needs another bier." As evidenced by such speech communities as the French and the Mandarin, the human capacity for dealing with homonymy in speech is vast. Homonymy may well be the strongest evidence for what Diver (1975/2012: pp. 53–56) referred to as a "human factor" in language: the fact that human intelligence is what allows language to function as well as it does in spite of the semiotic imperfections built into its structure.

In orthography, a writer may use an alphabet to represent meaningful units such as words. To that end, English orthography typically makes use of twenty-six letters, A–Z. Omission of letters, however, may be indicated by an apostrophe, as in *isn't* (*is not*) or *fo'c'sle* (*forecastle*). The apostrophe, then, is in a sense sometimes an orthographic null element.

2.1.1 Saussure to Bloomfield to Chomsky and beyond

Linguistics has a long tradition of the use of the null element. Saussure (1878, interpreted in Diver, 1974/2012: pp. 27–30) proposed for historical Greek a zero alternation with /e/ and /o/ (e.g., *leip-* / *loip-* / *lip-*) to account for attested patterns of vowels in Indo-European languages. Bloomfield (1926), in his "Set of Postulates for the Science of Language," decreed: "Absence of sound may be a phonetic or formal alternant [§43] ... Such an alternant is a zero element [§ 44]." Bloomfield gave empirical justification: "The postulation of zero elements is necessary for Sanskrit ..., for Primitive Indo-European ..., and probably economical for English." For the last, Bloomfield cited *book* "with affix zero, as opposed to *book-s*." Here Bloomfield was treating together "phonetic alternation," involving

phonemes, and “formal alternation,” involving morphemes. Zero as a phoneme was taken up by Hockett (1942: § 7.8) and achieved a fairly secure place, as phoneme or allophone, in American Descriptive linguistics. Zero as a morpheme was enshrined by Harris (1942: § 2.1).

The null element in grammar – though certainly *grammar* is a term of uncertain denotation, depending on the grammarian – rests upon the postulation of a null element in morphology. For instance, once the linguist, with Harris (1946), moves “From Morpheme to Utterance,” the postulation of a zero in morphology leads inexorably to the concept of a zero in syntax (§ 7.3). Thus, \emptyset enters into formal syntax essentially as a morpheme. For instance, in *Syntactic Structures* (Chomsky, 1957: p. 39), \emptyset is an option alongside other verbal affixes denoted *past*, *S*, *en*, and *ing* in a rewriting rule. Then, in *Aspects of the Theory of Syntax*, come the *dummy element* and the *null feature* (Chomsky, 1965: pp. 103, 155). These last are purely formal elements of syntax with no phonological or morphological content. For instance, one “dummy element” serves for “signifying” that the rule of the passive transformation of a sentence is obligatory, and one “null feature” specifies part of the syntactic environment for a selectional rule involving adjectives that can describe humans or not. The *raison d’être* of such constructs is to represent a syntactic property, something to do with the structure of sentences, not with the structure of morphemes. Likewise, the principle of “recoverability of deletion” (Chomsky, 1965: pp. 179, 182) in transformational syntax leads naturally to the creation of some formalism – a *trace* – to preserve the element that is deleted. For instance, the adjective *clever* would hypothetically have been deleted – leaving a trace – from its application to the noun *Mary* in the sentence *These men are more clever than Mary*. A syntactic slot, furthermore, is free to be occupied by an *empty category* such as PRO in a subject slot: *It is unclear what PRO to do* (Chomsky, 1982: p. 64).

Outside of the realm of formal syntax, too, the recognition of significant absence is longstanding, even if the theoretical basis for the recognition has remained largely unquestioned. In both the variationist and the grammaticalization frameworks – not that these are always separate – significant absence (e.g., null, zero, or null instantiation) is supported by some sort of structural paradigm, be it communicative (e.g., rhetorical), semantic (e.g., conceptual), traditional (e.g., the paradigm of grammatical person), or still syntactic.

Taking a feature of discourse – quotation – into account, D’Arcy (2012), in a variationist, diachronic study of English, sees a “null form” as a “strategy” of introducing quotation, alongside such overt lexical material as *say*, *think*, *go*, and (forms of) *be like*, to which list D’Arcy appends “Other.” If lexical items such as these – as opposed to grammatical elements – are members of an “open list” (Diver, 1990/2012: p. 69), then such a “null form,” rather than constituting a structurally

defined element as above, really amounts to the *absence* of an overt form (see below) in a communicative rhetorical context that is researcher-defined.

As regards the diachronic dimension, Bickel, Witzlack-Makarevich, Zakharko, and Iemmolo (2015) assume the “structure of agreement paradigms” to frame their cross-linguistic investigation testing a diachronic universal statistical principle involving the development, through grammaticalization, of “zero forms in the third rather than in the first and second person” (p. 30). Here, obviously, the paradigm of grammatical person provides the frame in which a zero form can be posited or assumed.

Bybee (1994), treating “The Grammaticization of Zero,” proposes that, through usage, something conceptual that is identifiable only in the “universal conceptual space surrounding the communicative context of language use” (p. 251), but not in the linguistic system itself, can develop into a linguistic element that has no phonetic substance but “true semantic content that is equivalent in many ways to” other linguistic elements (p. 242).¹ Here, not universal syntactic structure but “universal conceptual space” is guiding the postulation of zero.

In a similar vein, but adding a variationist approach too, Torres Cacoullós and Walker (2009) identify “overt indication of temporal distance” – cf. *no* overt indication – as a conditioning factor in “expression of future time in English.” Zero remains even here a creature of hypothetical structure, semantic even if not morphological or syntactic.

In variationist linguistics, some version of null is much studied, but typically it is assumed as the realization of a syntactic slot, not fundamentally proposed or questioned as a theoretical entity. For instance, Schwenter (2006), assuming, as did Chomsky, the syntactic framework of sentence structure, treats “null direct object” as an “observation” that is empirically “VARIABLE” in Spanish.² Similarly, Tippetts (2011) assumes a syntactic framework within which to identify direct objects in Spanish. Within the “envelope of variation” of verbs that occur with “*a*-marked” direct objects, Tippetts compares these tokens with “un-marked or *a*-less tokens” (excluding other uses of *a* ‘to’).³

Subject of the sentence, too, counts as a syntactic slot. Within variationist linguistics, the problem of “the variable *absence* and *presence* of subject personal pronouns in Spanish” – in the careful words of Ricardo Otheguy (2015, emphasis added

1. Already García and Putte (1989) had proposed frequency of usage as the mechanism that drives the diachronic development of an opposition between a nothing and a something.

2. See Otheguy (2002) for a critique of the commonplace view in linguistics that syntactic categories constitute observations.

3. On Spanish *a* with direct objects, see also García and Putte (1989).

jd) – has an extensive literature. It is an apparently uncontroversial statement that “In Spanish, as with other so-called pro-drop languages, subject personal pronouns (SPPs) are often omitted ... without changing the basic meaning of the utterance” (Carvalho, Orozco, & Shin, 2015: p. xiii). Leaving aside other theoretical obstacles (or “boulders,” to use Otheguy’s term), the view that a subject personal pronoun is “omitted” can ultimately be traced back, perhaps, all the way to the pioneering variationist study of the “deletion” of copula in English by Labov (1969). Though much that is practical – involving, say, bilingualism, contact, and language acquisition – has been learned through quantitative studies of the phenomenon (as seen in papers in Carvalho, Orozco, & Shin, 2015), few are those scholars who have questioned, as Otheguy has, whether the *absence* – or omission or deletion – of a form (such as *él* ‘he’) is the same thing, theoretically speaking, as the *presence* of a null form (Ø).⁴ As Otheguy points out, the distinction becomes crucial when, for instance, a researcher is concerned, on the one hand, with syntactic factors such as tense and, on the other, with extra-sentential discourse factors such as continuity of reference (or “switch reference”).

The theme in this intellectual history is the power of postulated structure to compel the postulation of null elements to prop that structure up. So if it is postulated that there exist meaningful forms (morphemes) made up of phonological elements (phonemes), then if these entities sometimes turn up (in alternation or in historical development) without those phonological elements, a null alternate of the physically absent sound will serve the purpose of preserving the postulated morphological structure. And if it is postulated that there exist sentences arranged in rule-governed patterns, then if these sentences sometimes turn up without those patterns (e.g., an infinitive clause without an overt subject), an empty category will serve the purpose of preserving the postulated sentence structure. Or if, instead of syntax, semantics is assumed to be universal, then alternation between the overt and the covert can still be deemed to have been “observed.” When such statements are made, theory – explicit or not – is driving analysis.

2.1.2 *William Diver and the Columbia School*

Not to say that analysis ever should or could be purely bottom-up or ad-hoc. Even the iconoclast William Diver (1993/2012, 1995/2012) – who, like Saussure before him, renounced the nomenclaturism of syntax (Otheguy, 2002) in developing what we now know as Columbia School linguistics and who insisted that “theory be guided by analysis, rather than the other way around” (1995/2012: p. 445) – Even Diver measured analytical success by the goodness of fit of his hypotheses to the

4. García and Putte (1989), in proposing a mechanism for the diachronic development of zero, had at least implicitly distinguished between absence (or “nothing”) and zero as a *signifié*.

data he had chosen, and he explicitly recognized the theoretical *orientations* that held the hypotheses together plausibly and coherently. That is, some overarching consideration always justifies the postulation of a null element. That is true both in what precedes and in what follows.

Diver's thought, while certainly influenced by his predecessors, is distinct from the paradigms of formal linguistics, grammaticalization, and variationist linguistics.⁵

2.1.2.1 *Diver and null*

Diver adopted the American Descriptivist construct of null. He discussed it in Diver (1990/2012) and incorporated it right into his grammar, where it was a *signal* of a *meaning* (cf. Saussure's *signifiant* and *signifié*), for instance, the meaning ONE in the English system of Number, as in *cat-Ø* as opposed to *cat-s*, with *-s* being the signal of the opposing meaning OTHER THAN ONE. Thus, Diver continued the practice of positing a null element to support the postulation of structure, in this case by using null to complete the exhaustive categorization of the semantic substance of Number.

This is not to say that it is always easy to decide whether or not to posit a zero signal, but in principle the decision is guided by oppositional structure, as long recognized (e.g., García & Putte, 1989). Contini-Morava (2006) wrestles with the question of "The Difference Between Zero and Nothing" in the context of a Swahili problem. Certain Swahili noun classes, unlike most of the eleven or so noun classes in that language, lack any identifying overt prefix in certain morphophonemic contexts. Only one of these noun classes, according to Contini-Morava, should be analyzed as having a zero prefix; the others "simply lack a prefix." This is an analytical decision, not a given. In Contini-Morava's words (p. 221): "a zero, or significant absence, can be most easily recognized (and therefore can reliably convey its meaning [in Diver's sense of that term]) within a closed set of oppositions in which all other alternatives are overt marks of some kind."

But the present paper is not a disquisition on just *null*; it is instead, one might say, a broader *Much Ado About Nothing*. This paper is an overview of the ways in which linguistic structure can be analytically relevant even when there is no overt sign of it at a certain point in discourse. Among those ways, zero, or the null element, is just one; there are other ways.

5. Huffman (2001) and Huffman (2012) offer good, accessible introductions to Diver's thought. See Davis (2004) for one take on Diver's debt to Ferdinand de Saussure.

2.1.2.2 *Diver and homonymy in grammar*

Another way for linguistic structure to be absent but relevant, seen already here in lexicon, is homonymy, in which a posited structural distinction is not maintained. Diver had homonymy in grammar too. This in itself is not unusual when one thinks of the homonymy of the English plural noun *-s* mentioned just above, as in *the cat-s*, and the singular verb *-s*, as in *It sits*. These hypotheses regarding the homonymy of *-s* in English are developed in Reid (1991) and further in Reid (2011).

Nor is such homonymy unusual in another of Diver's languages of interest, Latin. There, for instance, the suffix of the nominative plural of the first declension is identical to the suffix of the dative singular of that declension, so *agricolae* could be 'farmer-nom-pl' or 'farmer-dat-sg.' (In other declensions, the nominative plural and the dative singular are phonologically distinct.) For Diver (in Diver & Davis, 2012: pp. 218–219), those cases were signals of meanings in a grammatical system he called Degree of Control. Diagram 3 shows the system in an *interlock* with the system of Number and illustrated with a lexical item of the first declension (with length indicated by colon), with the two instances of the homonymous *agricolae* highlighted in italic type:

Meanings	Signals	Illustration (ONE / OTHER)
MOST	nominative	agricola / <i>agricolae</i>
MORE	ablative	agricola: / agricolis:
LESS	dative	<i>agricolae</i> / agricolis:
LEAST	accusative	agricolam / agricolas:

Diagram 3. Diver's system of Degree of Control in Latin (simplified)

It is not too difficult to imagine, in light of Diver's "human factor" (Diver, 1975/2012: pp. 53–56 *et passim*), how intelligent human beings manage to distinguish one *agricolae* from the other: In a given context, it will often be true that one knows whether one is dealing with one farmer or more, or whether one is dealing with a man (*nominative*) who, say, is selling corn, or with someone lower down on the scale of responsibility, such as a man (*dative*) who is sold corn. This must pretty much be the way modern Spanish speakers decide whether an instance of, say, *canto* is 'song' or 'I sing': by an intelligent use of context.

The postulation of homonymy in cases such as *agricolae* is fairly straightforward. A more interesting positing of homonymy in Diver's grammar is represented by *agricolis* in Diagram 3. Diver would have two signals *agricolis*. These are signals of the two distinct meanings MORE and LESS Degrees of Control "exercised by a participant over some activity, usually that indicated by the verb" (p. 215). But here, the two putative signals are adjacent on the scale. Moreover, the ablative plural and the

dative plural are always – without exception! – phonologically identical. One might well wonder how language-users manage to distinguish the two signals and thus the two meanings. Obviously, it was the structure of the Control-Number interlock that guided Diver’s decision to posit two signals here. Diver (1995/2012: p. 493) justified the decision regarding Latin ablative and dative plural the same way he did “the loss of a singular –plural distinction [in modern English *you*] which was maintained elsewhere in the system”: “the distinction made, precisely, in the system as a whole [i.e., *I/we; me/us; he, she, it / they; him, her, it / them*] is used as a reference point for setting up the possibilities from among which to choose where the signaling is imprecise [i.e., *you*].”

Evidently, in the phrase “as a reference point for setting up the possibilities,” Diver was not referring (just) to the analyst but to the language-user:

For the reader of the Latin text, the imprecise plural case form [e.g., *agricoli:s*] provides only the information that the word is to be regarded as not nominative, not accusative, not genitive, and what is left in doubt is only the distinction between dative and ablative. The reader, knowing [thanks to the singular, presumably; jd] the ways in which the dative and the ablative are used, can then decide which of the two is the more appropriate to infer. The need for the application of an intelligent appraisal is evident. (Diver, 1995/2012: p. 493)

Regardless of one’s confidence in Diver’s speculation about the psychological processes of the (proficient) reader of Latin, it is clear that the analyst, in setting up two signals for ablative plural and dative plural, is being guided by structure that is posited elsewhere and deemed to be relevant.

The thinking brings to mind the conception of linguistic structure traceable to Saussure (if through Meillet): a system – *un tout en soi* (Saussure, 1916/1972: p. 25) – in which *tout se tient* ‘the whole thing hangs together.’ One part of the grammar is related to every other part of the grammar.

2.1.2.3 *Diver’s residual member*

That interrelatedness of grammatical elements is particularly striking in what Diver (1978/2012: p. 125 *et passim*) called the *residual member* of a grammatical system, one whose “semantic substance is defined *entirely* by its opposition to the other members.” A somewhat trivial illustration of a residual member, seen already, is the English signal –s of the meaning OTHER THAN ONE in the system of Number (e.g., *cat-s*). A perhaps better illustration that the residual member means essentially NONE OF THE ABOVE would be Diver’s (p. 122) hypothesis for the meaning of the Greek genitive case in his system of Relation to a Place, Diagram 4:

Meanings	Signals
AT A SPECIFIED PLACE	dative
WELL-ORDERED WITH RESPECT TO A SPECIFIED PLACE	accusative
OTHER PLACE RELATIONS	genitive

Diagram 4. Diver's system of Relation to a Place in Greek

Basically, the Greek dative, says Diver, is used for a point-like location, the accusative for neat relations such as lines and circles, and the genitive for messier place relations such as the missing of a target or the meandering of a vine around a cave. The meaning of the genitive, then, is essentially NOT one of the other meanings of the system. In the Greek Place system, the genitive is a null kind of thing, in a way: an absence of something more structurally well-defined.

2.1.2.4 *Diver's opposition of inclusion*

Another variation on these system-internal relations is represented by Diver's *opposition of inclusion*. To understand this kind of structural relationship, it is necessary first to understand those seen, for instance, in Diagrams 3 and 4 as *oppositions of exclusion*: each meaning of the system excludes all the other members of the system. Such oppositions of exclusion, where one *value* excludes all the other values, are the norm in Diver. Oppositions of inclusion are far less common. One is represented by the system of Number in Greek (Diver, 1987/2012). To make the point, Diagram 5 contrasts the Number system of Greek – with its opposition of inclusion – with those of Latin and Sanskrit – which have only the more routine oppositions of exclusion.

Latin	Sanskrit	Greek	
ONE	ONE	ONE	}
	TWO		
OTHER	OTHER	OTHER	

Diagram 5. Three systems of Number

Latin has the familiar set-up: a signal (the singular) for the meaning ONE and a signal (the plural) for everything else. That is like English or Spanish. Sanskrit has something a bit more unusual but still just straightforward oppositions of exclusion: a signal (the singular) for the meaning ONE, a signal (the dual) for the meaning TWO, and a signal (the “plural”) for everything else, such as three, four, or seventy. But Greek has an opposition of inclusion. Greek has a dedicated signal (the singular) for the meaning ONE and a dedicated signal (the dual) for the meaning TWO, but its signal for numbers such as three, four, and seventy (its “plural”) can be used too when there are only two of something. The Greek meaning OTHER *includes* the meaning TWO. (This is indicated by the curly bracket.) Diver was fascinated by how the Greek writer, Homer, employed this Number system in accordance with an apparent interest in being precise or not, using the *included* signal of the meaning TWO for things that were of special interest to him and the *including* member, the meaning OTHER, for things that were of less interest to him. The point for us, however, is merely that, here again, an element of structure – a precise Number meaning – can be dispensed with. Put another way, a certain element of linguistic structure – the meaning TWO – remains relevant even when it is not signaled, even when the poet opts out of signaling that precise number.

All the structural relations seen so far might be called *oppositions of value*. In them, a given semantic substance – e.g., Number or Relation to a Place – is exhaustively divided up, by signals, into relative values. One value is defined by its opposition to the others, i.e., by being *not* another value in the same semantic substance. We have seen four types of hypotheses in Diver where an element of structure may, at a certain point in the text, be relevantly *not* present: the null signal, homonymy, the residual member in a system, and the including member in a system.

2.1.3 *The opposition of substance*

Another type of structural relation illustrates too, in its own way, the relevance of an absence of structure at a certain point in the text. This is the *opposition of substance*. As defined by Davis (1992: p. 287, summarized in Davis, 1995), an opposition of substance is “a relationship in which two signals have certain meanings in common but differ in that one signal entirely lacks meanings from some semantic substance to which the other signal belongs.”⁶ This structural relation was defined in order to account for the distribution in texts of two pronouns in modern literary Italian, *egli* and *lui*, both often glossed ‘he.’ The meanings that *egli* and *lui* have in common – and so establish a basis upon which the two pronouns can be related – are:

6. García (1983) had accounted for the distribution of the Spanish disjunctive pronouns in a way that looked forward to the opposition of substance, though she did not use the term and she insisted, unlike Davis (1992), that the forms in question had no meaning in common.

Number ONE, Sex MALE, Referent OTHER THAN SPEAKER OR HEARER (i.e., third person), and Attention LOW (as opposed to more highly demonstrative forms). Where they differ – their *opposition of substance* – is in that *egli*, but not *lui*, also signals a meaning from an additional substance: the meaning CENTRAL in a system of Focus on participants in events. Essentially, *egli* is restricted to being the subject (not the oblique) of a particular verb, while *lui* is much more of a free-floater. Consequently, the relevance of *egli* is tied to a particular event in the narrative, while *lui* may conceptually relate to something in addition to – or even instead of – an event in the narrative. For instance, *lui* may suggest a contrast between one man (*lui*) doing one thing and another man mentioned elsewhere in the context. Such a token of *lui* would be relevant both to its own verb and to some noun somewhere else in the context.

As can be imagined, the contrast between *egli* and *lui* is subtle and requires careful validation. Other oppositions of substance, however, are more readily obvious (given knowledge of the morphology). Davis (2002) analyzes the three Italian third-person disjunctive pronouns *ess* + (where ‘+’ indicates a slot for a gender and number suffix –*a*, –*o*, –*e*, –*i*), *loro*, and *sé* in terms of oppositions of substance. So *essi* ‘they / them’ is explicitly plural in number and masculine in gender. *Loro* ‘they / them / each other’ is explicitly plural in number but indifferent to grammatical gender. And *sé* ‘themselves / himself / herself / itself’ is indifferent to both number and gender. The three forms thus illustrate a one-step-at-a-time reduction in the relative semantic weights that they bear. *Ess* + provides the most information: person, number, and gender. *Loro* provides just person and number. And *sé* signals only person. The analysis shows how these oppositions of substance account for the observed distributions of the three forms in texts, including examples traditionally classed, respectively, as demonstrative, reciprocal, and reflexive: ‘they talk in the midst of them (*fra essi*)’; ‘they talk among themselves (*fra loro*)’; ‘they talk to themselves (*fra sé*)’.⁷ The distribution of *loro* is accounted for by a language-user’s *opting out* of the substance that corresponds to grammatical gender. The distribution of *sé* is accounted for by an *opting out* of substances having to do with number and grammatical gender. The relevance of that analysis to the present thesis is, again, that the systematic *absence* of a certain element of structure can be relevant in accounting for the observed presence of a form at a certain point in a text.

The idea of the opposition of substance is carried out more fully in Davis (2017b).⁷ There, the Italian clitic *si*, traditionally classed as the impersonal and reflexive pronoun of the third person (‘one,’ ‘himself / herself / itself / themselves’),

7. For another treatment of a modern reflex of Classical Latin *sē* in terms of an opposition of substance, see Gorup (2006) on Serbo-Croatian *se*. For earlier analyses, not positing an opposition of substance, see Garcia (1975), Diver (1986/2012), and Diver (1992/2012).

is analyzed in terms of oppositions of substance. Most of the other clitics (datives *gli/le/loro*, accusatives *lo/la/li/le*, plus the freestanding *egli*) signal meanings from systems of Number, grammatical gender or Sex, and a system called Degree of Control (traditionally, case), which (as above, for Diver) has to do with a participant's level of responsibility for an event. *Si*, by contrast, represents an *opting out* of all of those substances. The presence of *si* at a certain point in the text is a result of a writer's avoidance of those semantic substances. Sometimes they are irrelevant, sometimes they are superfluous, and sometimes they are too categorical and so get "neutralized" by *si*. *Si* signals very little: just that some third person is participating somehow in an event. Just who and just how, is left to inference. Crucial to the thesis here: The distribution of *si* can be accounted for only by an analytical appeal to those very semantic substances in the network of systems of which *si* is a part, which are *not* signaled by *si*. *Si* is present at a point in a text because of what *si* is not. *Si* is a mere specter of a pronoun, hardly a substantial pronoun at all – if one believes in pronouns.

The oppositions of substance that are posited for *si* account not only for individual tokens of *si* but also for statistical patterns across large stretches of text. For instance, in an authentic text, a chapter about people "Becoming Part of the Roman World" contains more tokens of signals of Degree of Control relative to *si*, while a chapter about "Italic Alphabets and Dialects" contains fewer tokens of signals of Degree of Control relative to *si*.⁸ This observed difference can be attributed to the *human factor*: People are typically held, by language-users, to bear considerable responsibility for events, while alphabets and dialects have no will of their own, see Table 1.

Table 1. *Si-* and the Neutralization of Control*

	Ch. VI	Ch. XI
	'Italic Alphabets and Dialects'	'Becoming Part of the Roman World'
<i>si</i> (no Control meaning)	122	196
<i>l</i> + (a Control meaning)	9	35
	Ratio 14:1	Ratio 6:1
		OR > 2.4

* Source: Giacomo Devoto (1951). *Gli antichi italici* (2nd ed.). Firenze: Vallecchi.

In this text, the odds of observing *si*, as opposed to *l*+, in a Chapter (VI) devoted to alphabets and dialects are over twice as high as the odds of observing *si* in a Chapter (XI) devoted to people. Thus, an element of linguistic structure that is present

8. This result is also reported in Davis (2016) and in Davis (2017b).

here and absent there in a text has very real, measurable effects on the structure of discourse. Or, at least, the opposition of substance provides an account of such correlations in discourse.

The opposition of substance is one more development in a series of linguistic treatments that point to the relevance of the insubstantial in observable phenomena.

3. Unsignaled structure in music

Much as the relevance of the opposition of substance finds support in language, so too the relevance of unsignaled structure finds support in semiotics outside of language: in music. This extra-linguistic support is relevant if, as indicated in the introduction to this paper, the facts of linguistic structure resemble importantly, through and through, aspects of other types of human behavior.

Consider modern Western musical notation. Analogize a linguistic element (e.g., *si*) to a certain triad of notes (e.g., GBD) in a musical score; analogize a phrase in a piece of discourse to a musical phrase; and analogize a language-user's grammar (a system of systems) to a whole musical composition. The 18th- to 19th-century convention in music was to indicate the key signature of an entire composition just once, at the very beginning of the piece; it is assumed then that that key signature prevails until it is explicitly changed.⁹ As a result, accidentals – sharps and flats – do not need to be indicated for each note of the perhaps several pages of a Western classical or romantic musical composition, but only once. Wherever the accidentals are not explicitly indicated, they are, actually, missing structures. In terms of performance, this principle of organization entails that a pianist's fingers, for example, will alight on a black or a white key in response to structure that is *not signaled* at that point in the musical text, perhaps not even on that page of text. For instance, consider the triad in Diagram 6:

9. This is where the analogy, like all analogies, is less than perfect. In music, there is one conventionalized place to indicate key structure, while in discourse, there is no particular conventionalized place to indicate grammatical structure. Musical key structure is typically made explicit by the composer at the beginning of the piece; grammatical structure is typically made explicit only by the linguist, not by the language-user – though language-users do occasionally speak somewhat explicitly about their grammatical structure: “OK, I’m gonna speak English now, not Spanish” or “I’ve probably never heard *egli* in Italian speech, only *lui*.” Anyway, much as an attested segment of a piece of music may lack any indication of still-relevant key signature, so an attested segment of discourse (e.g., a stretch with Italian *si*) may lack any indication of the still-relevant grammatical system (e.g., Degree of Control) being opted out of.

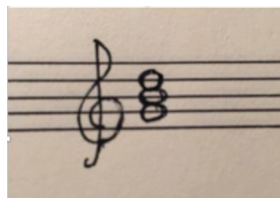


Diagram 6. Opposition of substance in musical notation

Three notes – making one chord – are indicated, but exactly what those three notes are depends on the key signature that was established at the very beginning of the composition. There are six different combinations of actual notes potentially played – the observations that a viewer-listener would make – depending on the overall key of the composition, see Table 2:

Table 2. Musical triads instantiating multiple compositional keys

Notes played	Key of the composition
• G – B – D	C maj., a min., G maj., e min., D maj., b min.
• G [#] -B – D	A maj., f [#] min.
• G [#] -B – D [#]	E maj., c [#] min., B maj., g [#] min., F [#] maj., d [#] min.
• G – B ^b – D	F maj., d min., B ^b maj., g min., E ^b maj., c min.
• G – B ^b – D ^b	A ^b maj., f min.
• G ^b – B ^b – D ^b	D ^b maj., b ^b min., G ^b maj., e ^b min.

For the benefit of those who know only that a piano has black keys and white keys: The number of those colors played might be 0, 1, 2, or 3, depending. Now, of course, a proficient pianist playing a Chopin *étude* typically will not pause to calculate all this, but the performance – the observable distribution of the movement of the fingers, to put it crudely – gives evidence of his or her implicit knowledge of the semiotic system.

Human beings are capable of operating systematically even when relevant structure is not explicitly signaled.

4. When there's no *there* there

In all the linguistic situations surveyed thus far, a posited structure provides a framework within which to posit a theoretically significant absence: homonymy, the null morpheme, the residual member or the including member in an opposition of value, and the opposition of substance. For Chomsky and his followers, that framework is sentence structure; for Diver and his followers, that framework is a

grammatical system (e.g., Number, Degree of Control, Focus, Relation to a Place) and the interlocks into which that system enters with other grammatical systems. This structural framework serves as a kind of analytical control over what gets posited; no linguist would posit a million zeroes all over the place.

That granted, however, it might be worth asking: Can anything interesting be said about situations when a structural element is present versus when it is *absent*, when it is simply *not there*, and when nothing is simply nothing? This is the question that Contini-Morava (2006) skirts but rightly avoids. Her chosen problem is, how can we tell a significant null from just nothing? Her chosen problem is *not*, what can we say when there is simply nothing? To address that question would require that we sacrifice the tight analytical control of a posited structural framework. The fear of that analytical precipice can be expressed with the truism that, throughout a body of discourse, there is an infinite number of absences of whatever structural element one might choose to study (for instance, the absence of *whom* in this paper).

Still, it might be possible to gain some understanding of what is accomplished by interjecting a structural element at a certain point in discourse as opposed to leaving it out at that point altogether. To make the task as manageable as possible, we would need to hold constant some element in the context. Then, we could at least get a sense of what effect is achieved by a language-user's introducing our hypothesized semantic substance versus not introducing it at that point in the discourse.¹⁰

Again, an extra-linguistic analogy is not hard to identify. For instance, a serious anthropologist might wish to study presence versus absence of open umbrella. Clearly, the anthropologist would want to hold certain variables constant; for instance, there might be no reason to study open umbrellas (or their absence) in the hands of persons lying in bed, nor open umbrellas carried (or not) by dogs, nor open umbrellas on cloudless days, nor open umbrellas on Antarctica. One could hold variables constant by limiting the study to, say, human pedestrians during rainfall. One could limit the geographical range of the study to, say, Northampton County, North Carolina; the borough of Manhattan, New York; and the city of Venice, Italy. Conducting such a study might reveal genuinely interesting facts about issues such as: sartorial fashion across generations, gender stereotypes across cultures, the attitudes towards rain among participants in agricultural versus urban cultures, and the design of thoroughfares across jurisdictions.¹¹

10. See also Tippets (2011). An enlightening treatment that takes a different analytical approach is the examination in Huffman (1997: pp. 293–315) of the system of Degree of Control signaled by the French clitics *lui* and *le/la/les* versus prepositional phrases with *à*.

11. For instance, there are streets in Venice that are too narrow at some points for pedestrians to carry open umbrellas, while this is not a factor in Manhattan, where streets are at least forty feet wide, or in Northampton County, where roads run extensively between peanut fields.

It is indeed feasible to study the presence versus the absence of a thing.

To conduct that as a study in Columbia School linguistics, one would have to ask: Under what circumstances is a given semantic substance not signaled at all?

4.1 Absence studied from a Columbia School linguistic perspective

Consider the distribution in modern literary Italian of *vi* and *ci*, adverbial clitics to the verb, both typically glossed ‘there’ and sometimes incorrectly viewed as “fully synonymous” (Russi, 2008: p. 57). Based on a survey of their distribution relative to each other, one might hypothesize that they are signals with relative values in a system of Restrictedness of Space, with *vi* signaling the meaning RESTRICTED and *ci* signaling the meaning UNRESTRICTED (Davis, 2017a). Such a survey would include examples that are locative in a straightforward way, such as *vi si annida* ‘is hiding out *there* (in Rome)’ versus *non ci torno* ‘I’m not going back *there* (abroad)’ (Silone, *Pane e vino*). But the survey would also include examples that the tradition classifies as existential, where the communicative effect is, putatively, merely to assert existence, as in Examples (1) (Rigoni Stern) and (2) (Calvino), below.

- (1) *vi* sono due pecore e un maiale
there are two sheep and a pig
 ‘there are two sheep and a pig’
- (2) *C’* era una farfalla morta
there was a dead butterfly
 ‘There was a dead butterfly’

Examination of the actual contexts of those examples reveals that, in (1), the sheep and the pig are *conveniently confined* in a stall where they can easily be slaughtered by hungry soldiers far from home, and that, in (2), the dead butterfly is found on the threshold of a house, one of several signs left *here and there around the countryside* by an evil viscount as omens of his ill intent towards his people. Thus, the precise space in which the sheep and pig exist is relevant: *vi* = RESTRICTED, while the precise space where the dead butterfly happens to be found is happenstance: *ci* = UNRESTRICTED. What the grammar of Italian needs to say, then, is not merely that certain examples are existential, but that sometimes the existence of something is asserted in a RESTRICTED Space, and sometimes the existence of something is asserted in an UNRESTRICTED Space.

This understanding lays the groundwork for a survey of examples asserting the existence of something, some with *vi* or *ci*, and some with neither; that is, some signaling Restrictedness of Space, and some not: some examples with a grammatical *something*, and some examples with grammatically *nothing*, some with

presence and some with *absence*. To make the survey manageable, one could limit it to examples with forms of the copula. Among such examples, one would find Example (3) (Silone):

- (3) sulla groppa dell' asino è allungato il cadavere d'un lupo
 on-the rump of-the donkey is stretched-out the body of a wolf
 'on the donkey's rump is stretched out the body of a wolf'

Examination of the context reveals that, in (3), a dead wolf is being displayed to a gaggle of villagers. One of them has shot the wolf and has brought it to show to them, in order to warn them of the existence of danger in the region. The point here is indeed simply that the wolf exists; there is no communicative need to restrict to a greater (*vi*) or lesser (*ci*) degree the space in which the wolf exists. This example, therefore, contains no signal of Restrictedness of Space. In the words of Gertrude Stein, "There is no there there."

If we in linguistics ever manage to develop a good understanding of structural absence – homonymy, the null morpheme, the residual member or the including member in an opposition of value, the opposition of substance – then we will be in a better position to understand the absence of structure. That is, understanding when nothing is *something* would help us to understand when nothing is simply *nothing*.

4.2 Absence studied in variationist linguistics

An essentially comparable approach is taken by Otheguy and Zentella (2012) in their full-length study of the presence versus the absence of subject pronouns in Spanish in New York City. Variationist linguistics concerns primarily the differences in output among individuals and groups of individuals. Now different individuals may have different mental grammars, especially if they are identified with different social groups (such as countries of origin). Then, the question of presence versus absence of an element is worth asking only if both individuals possess the element in question and also exhibit the possibility of its absence. For instance, both a Spanish speaker from Mexico and a Spanish speaker from Cuba might exhibit both *Él come* and *Come* 'He eats,' and a variationist might well study how the two speakers compare in terms of presence versus absence of *él*. The variationist might investigate whether, in general, speakers from Mexico and speakers from Cuba differ in regard to presence versus absence of *él*, and if so then how so. By contrast, it would hardly be worthwhile to study the presence versus the absence of the partitive clitic *ne* in the output of an Italian speaker from Italy compared to that of a Spanish speaker from Cuba; only the former would exhibit this *ne* at all.

To make their work analytically feasible – i.e., so that they can manageably compare presence and absence of pronoun – what Otheguy and Zentella (2012: p. 48) hold constant – the way they define their “envelope of variation” – is the presence of a bare finite verb with an “ascertainable” animate subject.¹² If they did not do this, they could claim that there are absences of overt pronoun all over the place.

It is worth considering why the problem of *Él come* and *Come* in the output of the Spanish speaker from Mexico and the Spanish speaker from Cuba is a problem of *presence* versus *absence* rather than a problem of *él* versus *null*. While there are certainly empirical reasons related to their study for this decision, Otheguy and Zentella (2012: p. 9) give a theoretical rationale as well: “The notion of a null pronoun reflects a conceptualization that is integral” in one’s theoretical framework. “The postulation of nulls ... is required by certain analytical claims that would otherwise be difficult to support” (cf. *supra*).¹³ This is the question of whether we have to do across the board – in both presence and absence – with structure or not. In the review of linguistics traced so far in this paper, a phonetic null was posited by Harris, by Chomsky, and by Diver as a structural element in its own right, the occupier of a slot in a morphological paradigm, in a sentence, or in a grammatical system comprised of meaningful signals. The opposition of substance too has to do essentially with structure: the systematic opposition between an element that bears a meaning from some semantic structure (e.g., Italian *l* + above, bearing a meaning of Degree of Control) and another element that does not (*si*). By contrast, at this point in this paper, the question is, instead, how to treat the overt presence of a structural element versus the mere absence of that element, when the absence of that element is not itself a structural element. There’s simply nothing *there*, much as when one compares an utterance like *This is a really muggy night* versus *This is a muggy night*. So, it might be argued, the theoretical reason why Otheguy and Zentella treat utterances such as *Come* as the absence of *él* rather than as the presence of a null subject is because they are analyzing not sentence structure but attested speech. Without the assumption of the framework of sentence structure, the utterance *Come* is just the utterance *Come*, and it contains no *él*.¹⁴

12. See pp. 48–55 of their volume for a full statement of their criteria.

13. Otheguy and Zentella (2012: p. 9) actually apply this statement only to formal linguistics, but, as seen above, it in fact applies more broadly.

14. This is not at all to dispute or dismiss their own carefully thought-out reasons for speaking of “absence” rather than “null subject,” but rather to give my own twist to the question, in the service of the point being made in this paper.

Adopting this position of presence versus absence, Otheguy and Zentella (2012) do discover interesting facts about Spanish as spoken in New York by members of various social groups. The facts of variation result from differential motivations by the members of the groups to insert into discourse the functional content that a certain pronoun (e.g., *él*) contributes. That insight is possible only if the question is framed in terms of presence versus absence. If “null subject” had its own value – either different from or the same as “overt subject” – then *that value* – a positive thing – would be competing, as it were, with the value of “overt subject,” much as the value of, say *él* ‘he’ competes with the value of *ella* ‘she’ or with the value of *ellos* ‘they.’

In linguistics, absence is not necessarily the same thing as null. (As, in mathematics, empty set is not the same thing as the real number zero.)

5. Conclusion

In linguistics, it can be worthwhile to truck in nothingness. This has been shown to be true in structural linguistics, in formal linguistics, in Columbia School linguistics, and in variationist linguistics and in grammaticalization. Moreover, as Diver (1995/2012: pp. 446–447) would have it, language is in some respects like other aspects of intelligent human behavior. So nothingness, if it is important outside linguistics (in mathematics, in sport, and so forth), may be important in linguistics too.

It is a well-known trait of human beings to seek out pattern and even to impute significance where there is none: seeing crabs and bulls in the constellations, finding good luck in a four-leaf clover, or believing in a divine promise on account of a rainbow. This general trait is no doubt an extra-linguistic manifestation of Diver’s “human factor” in linguistics. In a finite semiotic system such as grammar, where all the parts of the system interrelate, it is human nature to behave in ways that are consistent with that system, even when overt signaling of elements of the system is abandoned. The semantic side of language does not cease to exist when the phonetic side falls silent. If this is indeed the way human beings behave when we speak and write, then it will be unavoidable for the linguist sometimes to formulate hypotheses of such insubstantial realities as null signals, homonyms, residual meanings, oppositions of inclusion, oppositions of substance, and indeed to reckon with *absence* itself.

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Ditransitives and the English System of Degree of Control

A Columbia School analysis

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The English System of Degree of Control (Diver, 1984) is a Columbia School hypothesis that posits invariant meanings for word order signals in what are traditionally called transitive and ditransitive sentences. In this paper, the Control System is shown to account for speakers' choices between two constructions that seem, on introspection, to be equivalent: *push the wall* and *give the wall a push*. The Control meanings do not only describe a set of uses. Instead, by distinguishing between the linguistic system, on the one hand, and its use, on the other, the meanings of the Control System provide an explanation for the distribution of forms and the choices that speakers make in order to meet their communicative goals.

Keywords: ditransitive, English, Columbia School, Control System, grammar

Linguists working within functionalist approaches share the view that meaning is central to an understanding of language, an idea traced to Saussure (1916) and found today among current schools such as Cognitive Grammar (Langacker, 1988), Cognitive Construction Grammar (Goldberg, 1995, 2006), Radical Construction Grammar (Croft, 2001), and others (see Butler & González-García, 2014). Squarely in this functionalist perspective, this paper brings to bear a Columbia School (Diver, 1995 [2012]) analysis to explain some word order distributions in English, particularly around what are traditionally called transitive and ditransitive sentences. Columbia School analyses (see also papers in this volume by Reid and Davis) can be described as sign-based, as they aim to account for the distribution of linguistic forms in individual languages, generally by positing signals and meanings, and by distinguishing sharply between the linguistic system itself and its use in acts of communication.

The English System of Degree of Control (Diver 1984, Huffman 2009, Reid 2011) is a Columbia School hypothesis that accounts, in terms of semantics, for the

placement of what are traditionally called subjects and objects. In this paper, I will provide support for that analysis, by applying it to a very specific pair of seemingly equivalent utterances:

- (1) a. The girl pushed the wall
- b. The girl gave the wall a push

In each case, a photo of the activity would show the same scene: a girl and a wall, with the girl pushing the wall. This raises the question of why a speaker would choose between the (a) and (b) versions. To put the question another way, when there are two entities on the scene (a person and a wall) why would a speaker use a ditransitive construction (1b) when a monotransitive (a) is also available?

A survey of the treatment of ditransitives in functionalist theories is beyond the scope of this work. Instead, this paper will focus on the Columbia School approach, with some comparisons to Goldberg's Cognitive Construction Grammar account. After a brief summary of Goldberg's (1995) analysis, I will lay out the Columbia School (CS) hypothesis that accounts for the linear placement of verb arguments (the names of participants before and after verbs in English), and finally, will turn to an examination of data that provides support for the CS account as an explanation for the choice of utterances like (1b) in attested usage.

The phrase *give the wall a push* raises interesting questions for both Columbia School (CS) and Cognitive Construction Grammar (CCG), described below.

Ditransitives in Construction Grammar

Goldberg (1995) analyzes the syntactic frame of the ditransitive construction as [subj [v obj obj2]], and she describes the ditransitive construction's central meaning as X CAUSES Y TO RECEIVE Z, or 'transfer'.¹ She adds that, in the central sense, the transfer is successful, and pertains to "concrete rather than metaphorical or abstract ... transfer" (p. 33). Goldberg notes that "the ditransitive form is associated with a set of systematically related senses" (p. 33), and describes five "extensions" of this central sense, each of which is associated with a set of verbs. These extensions, along with a small sample of verbs that Goldberg lists for each one, are shown in Figure 1:

Goldberg also describes what she calls semantic constraints, and notes, citing both Partee (1965: p. 60) and Green (1974: p. 103), that the first object must be animate, and furthermore, that it "must be understood to be a beneficiary, or a

1. Goldberg uses the term 'meaning' consistent with the sense of the term in Cognitive Grammar generally; differences between this sense and that of Columbia School will be described in the next section.

- Central sense (Transfer)
(*give, pass, hand, serve, toss, bring, take*)
- Conditions of Satisfaction imply that the agent causes recipient to receive patient
(*guarantee, promise, owe*)
- Agent causes recipient not to receive patient
(*refuse, deny*)
- Agent acts to cause recipient to receive patient at some future point in time
(*leave, bequeath, allocate*)
- Agent enables recipient to receive patient
(*permit, allow*)
- Agent intends to cause recipient to receive patient
(*bake, make, build, get, grab*)

Figure 1. Goldberg's (1995: p. 38) extensions of the central sense of the ditransitive construction

willing recipient" (italics in the original: p. 146). Our *give the wall a push* examples violate this animacy constraint, and it is not clear in what way an inanimate entity like a wall can be a willing participant.

Another problem for Goldberg's analysis is that the central sense of the ditransitive construction, 'transfer,' appears to be operative in the transitive case of *pushed the wall*. Here too, the wall receives the action of pushing, no less than in the ditransitive *give the wall a push*.

While Goldberg does not consider *give the wall a push* as a particular exception to the general sense of the ditransitive construction as "successful transfer between a volitional agent and a willing recipient" and the animacy/willingness constraint, she does address other types of what she calls "systematic metaphors that license extensions from the basic sense" to account for examples such as "I'll grant you that much of your argument" (p. 151), in which the sense of transfer is not apparent. So this avenue – identifying metaphoric extensions – remains an open possibility to extend the proposed prototypical meaning of the construction to cover *give the wall a push* examples as well.

Theoretical preliminaries: Columbia School linguistics

Columbia School linguistics views language essentially as a device of communication (Diver, 1969 [2012]).² This axiom justifies positing linguistic signs – signal-meaning pairs – as the basic structural unit in language, and it follows then that speakers (and

2. By contrast, Geeraerts and Cuyckens (2007: 5), in an introduction to and overview of Cognitive Linguistics, state that "the primary function of language is categorization."

writers) choose lexical and grammatical signs that help communicate the messages they wish to express. Columbia School (CS) linguists ask on what basis speakers choose the forms they use. And the only way to make that determination is to examine instances of use in context, because it is only there that one can find evidence of speakers' communicative intentions. However, because in the CS framework, grammatical categories are not assumed and are not taken as observables prior to analysis, analyses must identify not only the meanings of forms, but also the identity of the signals themselves (see Reid, this volume; Otheguy, 2002).

Signals and meanings

Goldberg (1995: p. 1) describes constructions as 'form-meaning correspondences,' Langacker (2007: pp. 421–422) similarly calls them 'form-meaning pairings,' and CS linguists refer to 'signal-meaning pairs'; all of these are comparable to what Saussure called a linguistic sign. However, as CS hypotheses generally describe grammatical formatives and word order phenomena without reference to traditional categories, the identity of CS signals is not determined in advance of analysis. Specifically, CS does not take as a given the ditransitive construction. Rather, it begins with utterances such as *the dog chased the cat*, *the cat chased the dog*, *Alex taught biology*, *Alex taught the class*, and *Alex taught the class biology*, and recognizes that they all pose the distributional problem of accounting for the position of arguments with respect to their associated verbs. The CS solution to be presented here is two signal-meaning pairs (one corresponding to transitives, and the other to ditransitives), though neither is described in terms of familiar syntactic or semantic categories. In the case of what are called transitives and ditransitives, CS posits sets of relational meanings for the entity that appears before the verb as well as for each of the two positions after the verb. Thus, for tokens like *Alex taught the class biology*, *he passed me the potatoes*, and *she gave the wall a push*, the analysis posits a signal (stated below) that is referentially equivalent to Goldberg's ditransitive construction, though its meaning is quite different.

This difference in hypothesized meaning is primarily because CS draws a sharp distinction that is not found in Cognitive Linguistics more generally: between the linguistic system and its use. 'Meaning' is a technical term in CS for what is posited to be the invariant contribution of the linguistic system itself (Saussure's *langue*). 'Messages,' on the other hand, are the communications that result from the use of that system, and are part of what Saussure called *parole*. In the CS approach, meanings are hypotheses about the constant semantic contribution that forms and word order positions make. Crucially, these meanings are signaled every time the signal appears (in this case, word order), and are distinct from the interpretations of those meanings in communication.

In the CS view, because meanings are analytical constructs, they are not amenable to observation. The observables for CS are the distribution of forms (including word order), along with messages, which are the result of communication; messages are implied by speakers and inferred by hearers, and are infinitely varying and extraordinarily complex (Reid, 1991: p. 95).³

Otheguy (2002) illustrates the futility of trying to imagine all the possible messages that can be communicated by a linguistic string in his analysis of Chomsky's (1957: p. 88) example, *the shooting of the hunters*. Chomsky observes that this phrase can be interpreted in one of two ways: the hunters were shooting, or the hunters were being shot (i.e., with *the hunters* as subject or object). However, this classification of message types leads us only to imagine some contexts in which the words might occur. Otheguy points out another possible interpretation: *The shooting of the hunters out of the cannon that was fired by other clowns was especially funny, because, after flying through the air, the hunters fell on a water tank filled with ducks* (p. 391). Here, the two message types that Chomsky identified were limited by expectations of typical messages, as well by a reliance on the categories of traditional grammar. Although Chomsky's interest is in the number of interpretations that correspond to different syntactic structures, Otheguy has illustrated a larger point: that we cannot enumerate the possible interpretations of any linguistic string in advance. Speakers can deploy the resources of their linguistic system to express messages that we have not even dreamed of, and in ways that we have not yet considered. In the CS approach, metaphorical usage and extensions are seen as part of the creative use of language, rather than as part of the linguistic system itself.

From a CS perspective, the set of senses that Goldberg describes for ditransitives (Figure 1 above) is a classification of message types. It is likely that there are other messages that this construction could be used to communicate. Croft (2003: p. 55) identifies an additional sense (as in *That vase cost him \$300*), and states that “[t]here may be other senses of the ditransitive construction lurking in the lexicon of English” as well. While this may be expected as speakers continue to extend the usage of the construction, it is not clear what types of uses would falsify Goldberg's analysis. By contrast, a CS hypothesis could be falsified in principle by attested data that cannot be accounted for by the proposed signal and meaning (see Reid, this volume).

Croft (2003: p. 55) also notes that in Goldberg's analysis, “[e]ach verbal semantic class is associated with only one sense of the ditransitive construction” and that “the modulation of the possessive relation specified by each constructional

3. A third category posited by CS analysts and which is different from both meaning and message is the ‘scene,’ which is the objective reality that in many (generative) theories determines the truth value of utterances.

sense – actual, enabling, and negative transfer of possession – matches a semantic component of these verbs.” This observation suggests that the message elements that have been identified as extensions to the basic sense of the construction actually derive from the choice of lexical items that appear in it.

Resolutely a functionalist approach, Columbia School is akin to Cognitive Grammar in having a symbolic view of linguistic resources and in assigning content to grammatical formatives. However, it shares with generativist theories the structuralist position that there is a clear distinction between language as a system (for CS, signals and their meanings) and language use as the product of the system (Boogaart & Foolen, 2015).

English System of Degree of Control

The English System of Degree of Control (hereafter, the Control System) pertains to the placement of what are conventionally called subjects as well as both direct and indirect objects, positing meaningful positional signals that indicate relative degrees of Control exercised by participants over/in an event (Diver, 1984).⁴ More specifically, the central claim of the Control System is that word order – or the position of the arguments in relation to the verb – makes a semantic contribution, determining how much Control each participant is signaled to have over the event named by the verb. Control may be volitional or it may refer to the degree of involvement, or level of participation, that the entities have in the event.⁵ These signaled meanings have to be interpreted by hearers, using linguistic and extra-linguistic information, in order to infer speakers’ intended messages.

The meanings signaled by the Control System depend on whether there are two participants with the event, corresponding to the traditional category of transitives (Phase I), or three participants, corresponding to ditransitives (Phase II).

4. Degree of Control systems have also been posited for other languages: French (Huffman 1997), Latin (Diver, 1974 [2012]:43; Diver & Davis, 2012), and Spanish (García, 1975), but these are not discussed here. Published accounts of the English Control System can be found in Diver and Davis (2012), Reid (1991), and Reid (2011).

5. Only constructs that are found to be analytically useful and empirically motivated find their way into CS hypotheses. Accordingly, terms like transitive and ditransitive are not found. In this paper, I have used terms like these, along with other traditional constructs, such as verb and object, only to facilitate communication, and not to make any analytical claims.

Phase I: Two-participant events

In a two-participant event such as (2), we have the name of an entity (*my father*) before the verb, and another entity (*the table*) after the verb, and we can see that both the father and the table participate in the event of leaving. They each have some level of Control in that event and so we refer to them as ‘Controllers’ (shown with the letter C, and a subscript to distinguish one from the other and facilitate discussion). According to the Control System hypothesis, *the father* has a higher degree of participation in the event of leaving than does *the table*:

- (2) My father left the table
 C_1 C_2
 Event = left

Otheguy (2002: p. 390) has pointed out that an isolated sentence “is not in fact being considered in isolation, but within an unacknowledged context that allows it to be associated with some sort of communication.” So we will examine this example in the context of its appearance in Marilynne Robinson’s (2004) novel *Gilead*, in which the narrator’s older brother Edward has refused to say grace at the family’s dinner table:

- (2’) And Edward replied, and this was very wrong of him, “When I was a child, I thought as a child. Now that I am become a man, I have put away childish things.” **My father left the table**, my mother sat still in her chair with tears streaming down her face, and Edward passed me the potatoes. I had no idea what was expected of me, so I took some. Edward passed me the gravy. We ate our unhallowed meal solemnly for a little while, and then we left the house and I walked Edward to the hotel. (Robinson)

Clearly, the father has greater Control, or a higher level of participation in leaving than does the table, because he is the one who makes the decision to do so, and then gets up and leaves. The table, inanimate and motionless, also participates in this activity, albeit with a LOWER Degree of Control in the leaving than the father. The table is not without any Control though; it is the site of the family altercation, and it is the place that makes leaving possible.

Note that the subscripts (1) and (2) are for identificational – and positional – purposes only. We could have said Controller-before-the-event and Controller-after-the-event. To summarize: according to the Control System hypothesis, the participant before the event (*the father*) is a signal of the meaning HIGHER Control, and the participant after the event (*the table*) is a signal of the meaning LOWER Control. That is to say that both participants have some Control,

or degree of participation, in the event of leaving, but *the father* has HIGHER Control than does *the table*.

To apply this analysis to the first of our *push-the-wall* examples (*the girl pushed the wall*), the Control hypothesis posits that the girl (C_1) has more Control over the pushing than does the wall (C_2). She is active and presumably volitional, and the wall is likely to be neither. However, the wall also has some Control in the event; that is, it has some degree of participation, because (at the very least) the pushing couldn't happen without the wall.

We have seen that in two-Controller events, the semantic substance of Control is divided into HIGHER Degree of Control (for the participant before the verb) and LOWER Degree of Control (for the participant after the verb). The relational meanings HIGHER and LOWER refer to a comparison of the Control exercised by the two Controllers over the same event. The signals and meaning of Phase I of the English System of Degree of Control are schematized in Figure 2:

Signal:	<i>Controller</i>	+	<i>Event</i>	+	<i>Controller</i>
Meaning:	HIGHER				LOWER
	CONTROL				CONTROL

Figure 2. Phase I, English System of Degree of Control (2 Controllers)⁶

There is no constant role that a participant plays as a result of being coded as a Controller and therefore of signaling HIGHER or LOWER Control over a particular occurrence. Instead, it is the *relative* Degree of Control in comparison to the other Controller that is signaled, as the particular amount of Control in a specific communicative event varies depending on the meaning of the verb and on the individual context.

Because Columbia School meanings are hypothesized to be invariant, Control meanings are held to be signaled each time there is a verb with a Controller before and after (loosely corresponding to subject and object), even though the nature of that role may be different in each case. Speakers can deploy these stable meanings in creative and unexpected ways, to communicate unexpected and new messages.

Example (3) describes a 1948 film based on a fairy tale by Hans Christian Anderson called “The Ballet of the Red Shoes,” and illustrates the creative use of the meanings of the Control system:

6. Columbia School analyses do not posit verbs as parts of speech or as syntactic categories. The more precise formulation of the signal would be *Controller + Satellite Cluster B + Controller*, where Satellite Cluster B (SCB) is a lexical item that can be modified by verbal morphology.

(3) The shoes dance her out into the street

In this film, a young girl wants to attend a dance in a pair of red shoes. She gets the shoes and all goes well until she becomes tired and wants to go home. But the shoes are not tired and they do not want the night to end. One of the characters explains, “They [the shoes] dance her not only out into the street, but also over mountains and valleys, through fields and forests” (Dargis, 2009). Clearly, the shoes have more Control in the dancing than does the girl whose feet, led by the shoes, are dancing against her will. Because the Control System underdetermines the specific roles each participant will play, speakers can use it in creative ways such as this.

In the CS view, the language itself provides only sparse hints (signals and their meanings) about the roles that participants play. The grammar assigns a participant the abstract role of Controller, and then specifies a relative Degree of Control for each participant, based on their positions. The relationally defined meanings are versatile, as they can be interpreted differently in different contexts. The interpretation of those meanings, along with the interpretation of all the other linguistic and non-linguistic information related to an utterance, belongs to the realm of the communicated message in a single instance of use, which is distinct from the formally encoded abstract linguistic meanings that are signaled. It is left to processes of inference on the part of language users, based on both linguistic and non-linguistic information, to determine what the intended messages are. As Davis (2004: p. 155) observes, “Possibly the fundamental insight of CS grammar is that a gap exists between the spare linguistic meanings encoded by linguistic signals and the rich communicative messages [that] can be inferred from those meanings.”

In a moment, we will see another example that further illustrates the creative deployment of the meanings of the Control system, involving the word *bunt*, which can be either intransitive, as in (4a), or transitive, as in (4b), and where typically the object is a ball:

- (4) a. With a runner on third, the batter bunted.
b. The batter bunted the ball down the first base line.

However, in (5), from a play-by-play commentary of a baseball game, a broadcaster is discussing the team manager’s decision whether to have a player (Rubén Tejada) bunt or swing. He sees Tejada getting ready to bunt, and announces the following:

- (5) Tejada is up. Is he going to bunt? In a similar situation in the 5th inning, he let him hit. But **he’s going to bunt him** here. (Cohen)

There are two participants in this act of bunting: the manager, and the player, Tejada. Tejada’s role in the event of bunting is quite different from that of *the ball* in (4b). Nevertheless, both Tejada (*him*) and *the ball* are categorized as exercising

a LOWER Degree of Control in the two events of ‘bunting.’ The manager is signaled as the HIGHER Controller because while Tejada will execute the bunt, it is the manager who has decided, and has directed him to do so. This example, like (3), shows that relative (and imprecise) Control meanings in transitive constructions can be used in novel ways to express a wide range of both familiar and innovative messages.

In the CS framework, messages do not have status as part of the linguistic system. We cannot enumerate the possible messages that any linguistic structure will be used to communicate. Instead, we can identify the linguistic resources (signals and their invariant meanings) that are deployed creatively by language users for ever-changing and often unpredictable communications. Otheguy (2002: p. 391) notes that “[d]istinguishing sharply between meaning and message, and insisting on the message’s lack of linguistic status, has been the key to the analytical success in the work of the Columbia School.” That is, by distinguishing between linguistically encoded meanings and communicated messages, the analyst can avoid plowing the unlimited sea of messages.

Phase II: Three-participant events

While Phase I of the Control System applies to transitive constructions, Phase II pertains to the positional signals when there are three Controllers (ditransitives). Phase II of the Control System is schematized in Figure 3:

Signal:	<i>Controller</i>	+	<i>Event</i>	+	<i>Controller</i>	+	<i>Controller</i>
Meaning:	HIGH				MID		LOW
	CONTROL				CONTROL		CONTROL

Figure 3. Phase II, English System of Degree of Control (3 controllers)

Example (6) is an instance of the three-member Control system, and is also from the passage in (2’) above (with subscripts for ease of discussion):

- (6) Edward passed me the potatoes.
 C_1 C_2 C_3

By hypothesis, the entity that appears before the verb, *Edward*, has the most Control (i.e., the greatest degree of participation in the giving), of the three participants. The participants *me* and *the potatoes* (labeled C_2 and C_3 respectively) are also involved in the act of *passing*, with *me* being more involved than *the potatoes*.

In (6), Edward is the agent who initiates the event of *passing*. In contrast to the father, who has left the table, and the mother, sitting “still in her chair with tears

streaming down her face,” Edward has taken an action, and has a HIGH Degree of Control. The person he passed the food to, named by the word *me*, is assigned the meaning MID Control, because in the event of passing, he had the option to refuse the potatoes; he does not do this, but rather, accepts the potatoes (he notes that he did so because he had no idea what was expected of him). *The potatoes*, an inanimate vegetable with no volitionality and no ability to move or make decisions, could not choose whether to sit at the table, or to be passed, or to whom. The potatoes stay or go where they are placed by others, so the author chose the meaning LOW Control for *the potatoes*.

It’s important to note again that these meanings – HIGH, MID, and LOW (like HIGHER and LOWER in the two-Controller Phase I system) refer not to specific roles such as agent, recipient, or patient, but to relative degrees of Control (relative to each other) by means of their position within the utterance. Again, the hypothesis is that these meanings are grammatically signaled *every* time this word order occurs, and that these signaled meanings help hearers construct an interpretation of the utterance.

According to this hypothesis, in (6), the message element of ‘transfer’ is the product of the hearer’s interpretation of the signaled meanings, including the lexical meaning of the word *pass*. By not building the notion of transfer into the meaning of the ditransitive construction itself, the Control System accounts just as easily for the interpretations of (7) a–e below as for (6).⁷ By contrast, Goldberg must posit a separate sense for each of those she claims to be an extension of the basic sense (transfer) that she has posited, or, following Croft (2003: p. 58), postulate that they are independently represented in the mind:

- (7) a. She owed me money.
 b. She denied him his reward.
 c. I admired him his easy way with people.
 d. His carelessness cost me a lot of time.
 e. I envied him his joie de vivre.⁸

7. Goldberg accounts for such examples by observing the relationship of these message types to the central sense of transfer. For example, with *owe* (7a), Goldberg (citing Searle 1983) notes that “transfer is implied by the ‘conditions of satisfaction’ associated with *owing*.” For ‘verbs of refusal’ as in (7b), Goldberg notes that the possibility of transfer has arisen but been denied (p. 32–33).

8. See Coleman and DeClerck (2008) and Goldberg (1995: 132) for discussion of ‘envy’ and ‘forgive’ in these constructions.

Comparing Phase I and Phase II

The original examples in (1) are repeated here as (8a) and (b):

- (8) a. The girl pushed the wall
 b. The girl gave the wall a push

These utterances differ in the Control meanings that are signaled. The chart in Figure 4 gives an overview:

Phase I			Phase II		
Two-Controller utterance: <i>The girl pushed the wall</i>			Three-Controller utterance: <i>The girl gave the wall a push</i>		
<i>the girl</i>	C ₁	HIGHER	<i>the girl</i>	C ₁	HIGH
<i>the wall</i>	C ₂	LOWER	<i>the wall</i>	C ₂	MID
			<i>a push</i>	C ₃	LOW

Figure 4. Comparing *pushed the wall* with *gave the wall a push*

So far, we have described different positional meanings for arguments in transitive (Phase I) and ditransitive (Phase II) utterances. We have not yet answered the question as to why we have three Controllers for what appears to be a two-participant scene. We might also wonder why a wall would be categorized as a MID Controller. Recall Goldberg's observation that in ditransitive constructions, the first object (which is hypothesized to be a signal of a MID Degree of Control) is rarely inanimate. It is not surprising that an entity with a relatively higher level of Control (MID as compared to LOW) would usually be animate. But what kind of Control does a wall have in these types of examples? And why would speakers want to encode *wall* as having this type of Control?

Invented examples do not illuminate this problem. In the next sections, we will review data to shed light on these questions, and will see that in Phase II (ditransitive) examples, speakers are communicating messages in which the wall has a greater degree of participation (signaled by the meaning MID Control) than it does in Phase I examples (where it signals the meaning LOWER Control).

Data: *Gave the wall a push*

The problem being addressed is the basis on which speakers choose between *give the wall a push* and *push the wall*; that is, the choice between using the three-member Control System and the two-member Control System for messages about scenes that involve only two observable participants.

A Google search (March 2016) for *gave the wall a push* yielded only 14 examples, but one thing was immediately striking: nearly half (6) describe adventures in mysterious houses with dark passageways and hidden dangers. This is a remarkable pattern, and one that suggests a certain type of context favors the use of *gave the wall a push*. Of the six that are not in mysterious houses, there are other contextual clues that point to the importance of the wall in the interaction, and its relatively greater Degree of Control in the activity of pushing. The Appendix shows all 14 examples. In this section, we will examine four of these, considering contextual clues and analyzing the contributions of the meaning MID Control in each example. In all cases, *the wall* is portrayed as an interactive participant, and there is some uncertainty as to whether it will move in response to the push. Following this qualitative analysis, I will compare the message elements in three-Controller examples (*gave the wall a push*) with a comparable set of two-Controller examples (*pushed the wall*).

As noted previously, six of the 14 tokens of *gave the wall a push* take place in mysterious houses. The following example is representative of all six; it is from an e-book that's described as a suspense novel:

- (9) They tried another bottle, found a few more candles, lit them, put “Caro Nome” back on the turntable, explored. There were no other rooms, and just one other door, directly under the Greek writing. They opened it, saw a stone staircase, followed it up ten steps – Nat counted them for some reason – came to another leather-padded door with brass studs. “This is so much fun,” Grace said, turning the knob, “like one of those interactive-theater evenings, only for smart people.” The [leather]-padded brass-studded door opened onto a brick wall. Izzie did something then that made an indelible impression in Nat’s memory. **She gave the wall a push with the fingers of one hand**, just a little push, as though it were a prop that would topple at the slightest touch. The bricks were real; it didn’t. (Abrams)

This is an act of exploration. Neither Nat nor Izzie knew what would happen when she pushed the wall. Izzie clearly has the greatest Degree of Control in the pushing, but the wall also has some Control in this act. The wall is motivating Izzie to give a push because she wants to see if it will move. In this 3-Controller utterance, the wall has a MID level of Control, as opposed to a LOWER level (in *pushed the wall*).⁹ If the writer had said *She pushed the wall with the fingers of one hand*, the position of *the wall* would have signaled LOWER Control. But *the wall* has a more significant degree of participation than being just an inanimate recipient of the push. By expressing

9. Of course, speakers have many other options to express messages like this. They might also choose intransitives like *pushed against the wall*, or even other lexical items like *tested the wall to see if it would move*.

this message with the 3-Controller system, the wall becomes a MID Controller – which prevents the reader from thinking of the wall as contributing little or nothing to the pushing. It has a greater Degree of Control in this action because it may or may not resist the push – the pushing is intended to find out what the wall will do.

What about the *push*? What Control does *it* have? The push is categorized grammatically as an entity and hence a participant in the event. It has LOW control because it is brought into existence only when Izzie acts against the wall; and it is entirely dependent, for its existence, on the other two participants – the ones with HIGH and MID Control.

It is not surprising that in houses with unexpected entrances, dark corners, and hidden passageways, the components of those buildings (such as walls) would be presented as more active (MID Control) than in traditional, inert buildings. Therefore, rather than examining the other five examples that resemble (9), we turn to passages that do not take place in such houses. Example (10) is from a blog, titled *Ill Omens*, in which the writer also describes a house:

- (10) It was an apocalyptic dream. I was standing in the hallway of the apartment where I used to live, from 1993 to 1998. I felt like the house was rocking slightly. For some reason, **I experimentally gave the wall a push**. The whole house collapsed. Somehow, when it collapsed, I ended up on the outside of it, looking at the wreckage. (Omens)

The wall is categorized grammatically as exercising a MID Degree of Control in the pushing, because clearly there is an element of uncertainty about what it will do in response to being pushed. The word *experimentally* provides textual support for the interpretation that the person pushing the wall does not know what will happen. In fact, it turns out that the wall had a lot of Control on the scene, because after it was pushed, the whole house collapsed. By making the wall a MID participant (i.e., a signal of MID Control rather than LOW Control), the writer involves the wall more, making it less the helpless victim of a push, and more an interactive participant in the event.¹⁰ Another important component here is that the experience takes place in a dream. In real life, under the laws of physics, walls are rarely interactive; but in dreams, all objects may be viewed as agentic.

The next example is from a book about an unborn dragon, Sandy, who is encased in an egg whose shell forms the wall to be pushed:

10. The word *experimentally* also interrupts the word order sequence (C₁ Event C₂ C₃). Hearers must use inference not only to interpret the signals, but to identify them as well.

- (11) Wall? What wall? He lazily blinked his eyelids in an effort to clear his vision. The wall was right in front of his nose, and it seemed to be all around him, blocking his view. It was a funny, lightly colored wall, and the squeaking noise seemed to be seeping right through it. **Sandy gave the wall a push with his nose**, making his own squeak with the effort of pushing.

The wall seemed ready to open up, and Sandy suddenly had the strongest of urges **to push the wall away from his face**; so he pushed, poked, and battered it. He kicked his legs and thumped his tail as much as he could while twisting his body into the shape of a battering ram. He was determined to get that wall down.

And he did. With a tinkling and a slurping sound, the eggshell cracked, and a small piece fell from the wall. Sandy poked his nose through the little hole and immediately was assailed with the noisy new world that flooded his senses!

(Porteus)

Here again - for the first token of push, in which the unborn dragon *gives the wall a push* - the push is a test. Sandy doesn't know what will happen; he is curious and he's testing it out, and the wall will play a role in determining what happens next. The meaning MID Control is useful here because the wall plays an interactive role, and has a significant Degree of Control in the event.

The passage also gives us *push the wall*, in which the wall is signaled to have LOWER Control in the action. By this point in the process, Sandy fully expects to be able to move the wall out of the way. And indeed, the wall falls, as expected, merely as a passive recipient of the action.

Another example that refers to a house, but not one with surprising passage-ways and tricky and mysterious walls, is this:

- (12) Houses never rest; I knew that, but the Trevors' house was especially full of chatter. I could hear the mice, the mosquitoes, the cats crying, and then sometimes from within the walls the sound of large mammals making their way; I heard as they tumbled above me, their play so raucous that bits of my ceiling sifted down, and I heard as they walked right by my bed, nothing between us but cracked plaster, paint, and paper. 'Raccoons,' Annie said when I asked her. 'They live in the walls here.'

Every night they came, walking in the wall beside my bed. I started to scratch at that space, trying to widen a tiny puncture already present. I found an X-acto knife and, after checking to see that my door was closed, I used its precise point to trace a small porthole. Flexing my first finger, **I gave the wall a push**, surprised at how cleanly it all gave way, leaving a quarter-sized hole, perfect for peering.

(Slater)

This pushing was ginger, careful, respectful of the power of the wall; the writer says it was done by *flexing my first finger*, instead of a full-on push. And the word *surprised* shows that the writer was uncertain about what would happen when she pushed it. In fact, in each one of the *gave the wall a push* examples we have seen, the outcome of the pushing was uncertain – the pushers didn't know for sure what would happen. These walls are not just passive and inert. Instead, they have an interactive role in the pushing, which means a higher degree of participation – a greater Degree of Control than would be signaled by the two-Controller *pushed the wall*.

The writer's linguistic maneuver of making *the wall* a MID Controller is a way to signal that the wall has an active role in the act of pushing; that is, the interactive element is suggested when *the wall* is a MID Controller. This usage demonstrates that there is no 'animacy constraint,' as Goldberg (1995) noted, but rather, that speakers only deploy MID Controllers when they have an interactive role in the event, which is not usually the case for inanimates.

We turn now from an examination of 3-Controller examples (*gave the wall a push*) to a review of 2-Controller examples (*pushed the wall*).

Data: *Pushed the wall*

For comparison, I examined a set of 2-Controller events by selecting the first 20 passages that were returned by a Google search (July 2017) of *pushed the wall*.¹¹ As a reminder, the hypothesized meanings of the 2-Controller System (Phase I) are HIGHER (for the entity before the verb) and LOWER (for the entity after the verb), so that in *pushed the wall*, the wall is signaled to have LOWER control in the pushing (as opposed to MID in *gave the wall a push*). Unlike the *gave the wall a push* examples, not a single *pushed the wall* example described a place with secret passageways, hidden doors, or dark corners. But there were two contexts that did occur with some frequency: five of the 20 pertained to physics (textbooks, an instructional website, Q&A for a class), and four of the tokens pertained to information for building contractors. As shown in the next two representative examples, in these contexts, walls are seen as inert objects, with little Control over events. The first, (13), is from a contracting website, and the second, (14), from a physics textbook:

11. Omitted from this collection were examples in which, in the string 'pushed the wall,' *the wall* and *push* were not Controllers, such as 'pushed the Wall Street bailout' and 'If pushed, the wall will disappear.' Also excluded were hits from websites that required a log-in.

- (13) I am in the process of buying a house. **Inspection revealed the roof was sagging and had pushed the wall** out a little bit. I got a structural engineer to take a look at. He told me that rafters are slipping out. (ContractorTalk.com)
- (14) While hanging a picture, you accidentally dent the wall with a hammer. Did the hammer do work on the wall? [Answer:] Yes, **it pushed the wall** inward and the wall dented inward. (Exercises)

In both cases, as well as in the rest of the 2-Controller data, the walls do not have an active role in the pushing, and are not expected to interact in any way other than to be moved by the push. There was one example in the small set of 20 in which *the wall* refers to a sheet of ice that was indeed moving and active. However, as in the previous examples, the wall does not interact, and thus is signaled to have a LOWER Degree of Control in the pushing:

- (15) Around 9:30 a.m., people watched in disbelief as ice crawled into doors and windows at Izatys Resort. **Winds as strong as 40 miles per hour pushed the wall of ice onto the southeastern shorelines.** (High Winds)

These examples illustrate that when someone or something *pushes a wall*, the wall moves, as it behaves only in response to the pushing, with practically no input of its own. This is different from what we saw about *giving the wall a push*, in which the pusher is not certain whether the wall will yield, and the wall is conceptualized as having an interactive role in the pushing.

Patterns of distribution

In the preceding section, I identified several message elements that co-occurred with *gave the wall a push* and *pushed the wall*. A review of the patterns found among these two alternatives in the small set of data analyzed is presented in Table 1. The results are shown using just the first 14 *pushed the wall* examples to match the 14 tokens of *gave the wall a push*. The numbers in these columns total more and less, respectively, than 14 because some tokens fall into more than one category (i.e., a mysterious house and expression of uncertainty regarding the result of pushing), and some fall into none of them.

While the number of examples for both columns is small, the clustering around certain types of messages is quite striking. The previous sections described possible motivations for the use of the meanings MID and LOWER Control for the message elements shown here.

Table 1. Patterns in the distribution of *gave the wall a push* vs. *pushing the wall*

	Number* of <i>gave the wall a push</i> (MID Control)	Number of <i>pushed the wall</i> (LOWER Control)
Mysterious house	7	0
Description of a dream	2	0
Physics/geology	0	3
Contractors	0	2*
Wall represents a political movement	1	0
Expression of uncertainty regarding result of pushing	6	0

* In the remaining six tokens in the set of *pushed the wall* examples, there are two additional passages pertaining to contractors.

We have already seen that it makes sense for a writer to portray a wall as having a greater Degree of Control in dark, mysterious houses where the occupant/explorer is not sure what will happen next, than in ordinary houses and buildings where walls are solid and unchanging. Further, in dreams, all things are possible; any element worthy of being mentioned in a dream might have an allegorical, and therefore not ordinary, role. By contrast, scientific texts (physics, geology) or contracting jobs give straightforward descriptions involving walls that do not move, and have no role other than to function as a stereotypical wall – an inanimate object that is acted on by outside forces.

There is one token of *gave the wall a push* in which the wall metaphorically represents political forces, meaning that it consists of the actions and views of people and institutions, animate agents who would be expected to have a greater degree of participation than actual inanimate walls:

- (16) There was a problem of course – but it was only a law. Wall Street’s crash of 1929 had led to some restrictions. They called it Glass-Steagall. It prevented insurers and securities firms from also controlling a bank. Attempts had been made to break down this wall, but small banks and consumers had in each case fought back. The last years of Clinton were the best window yet. The market was booming and the Dems had gone corporate. Glass-Steagall remained after a ’98 onslaught. Then, **Sandy gave the wall a push.** (Lee)

Two other tokens in the *pushed the wall* set also refer to metaphorical walls, as parts of human psyches. In both cases, though, the wall is not accorded power, but rather is conceptualized only as a static object to be acted on. An examination of one of these passages will illustrate:

- (17) Let me emphasize again how important it is to be aware of the necessity of one day destroying your wall. ... Your spiritual rebirth can occur only after the wall has disappeared. Then, your soul stands naked in front of your Maker, in front of yourself. ... As long as your rigid wall remains, however weak, however pushed back it may be, the divine substance is ineffective to the degree of the strength of the wall. In other words, the stronger the wall, the weaker the effect of the divine substance that is waiting to penetrate and fill you. So, my dear friends, all of you who work on this path so successfully, visualize this wall within yourselves. ... Wherever the conscious emotions, opinions, thoughts, conclusions, and desires are separated from those who are unconscious, we can see a wall in the human soul. ... Let us assume you have started on this path with good faith and goodwill and you have progressed well. You have made certain major recognitions ... **Thus, you have broken the first resistance and pushed the wall considerably into the background**, allowing certain information to filter through. ... But do not imagine that then all resistance is overcome for good. For as long as you maintain the wall, resistance is inevitable, and only the form and manifestation of the resistance will be different. (Saly)

In (17), the wall is conceptualized as separate from the individual, and is described as 'rigid,' suggesting that the wall is fixed, a type of resistance that can be pushed into the background, and through which information can filter. The wall itself is not seen as having an active role; in this passage, it merely stands, inert, between the person and their spiritual rebirth, so it is categorized as a LOWER Controller rather than a MID Controller.

This comparison of message elements provides additional support for the meanings hypothesized as part of the Control System, which account, in part, for speakers' choices between *pushed the wall* and *gave the wall a push*.

Other contextual factors

Of course, the Control System meanings are not the only ones signaled in any utterance and other factors affect the choice between 2-Controller and 3-Controller utterances. For instance, in *gave the wall a push*, the speaker has chosen to make the verb *giving* rather than *pushing*; the semantic contribution of *give* is present as well, as are the semantic contributions of *the* and *a* (in *the wall* and *a push*), and past tense. An examination of the semantics of these forms is beyond the scope of this paper (cf., Epstein, 2001; Huffman, 2009; Newman, 1996). The meanings of all these forms, along with extra-linguistic factors, also feed into speakers' choices and inferential processes in interpreting these utterances.

‘Giving the book a read’ and other types of examples

As noted earlier, the object of explanation for CS analyses is to account for the distribution of forms in attested language use. In the case at hand, Phase II of the Control System hypothesis is applicable to other ditransitives as well, including (but not limited to) *give the door a push*, *give the car a push*, etc. An extensive review of such data is beyond the scope of this paper. We will examine here a single example of *give the book a read*, which illustrates that the writer of this passage has used the meanings of the Control system to signal that this inanimate participant (*the book*) does have an active (MID) degree of involvement or Control.

This passage was the first result of a Google search (March 2016) for *give the book a read*. It’s from a reader’s review of a self-help book on Amazon:

- (18) For those who feel an author must be “credentialed” (MD or PhD, or whatever) to offer this type of advice, or who are looking for hard data research to back up the claims he makes, this book may disappoint. I would just encourage such readers to suspend judgment long enough to **give the book a read**, take what they find to ring true and useful and leave the rest. There is a lot of value here, and I have found it to be “proven” in my own experience ... (Amazon)

The writer of this very positive review is encouraging people to read the book, and by so doing she believes they will discover its usefulness. In fact, she is encouraging those who are initially skeptical about the validity of the author’s ideas to suspend judgment, so that the book itself can help them make up their mind. She says that the book (unlike the author’s credentials or the lack of hard data) will reveal its value, so she signals that *the book* has a MID level of Control in the event of reading. In *read the book* (Phase I), the *book* would have been categorized as a LOWER Controller; the author opted instead for *give the book a read* (Phase II), making the book a MID Controller. One example certainly cannot confirm a hypothesis, but this passage does provide additional support for the meanings of the Control System.¹²

While Columbia School analyses pertain to attested data and are not intended to account for intuitions, the Control System also explains an observation Goldberg (1995: p. 33) makes regarding the difference between sentences like the following:

12. Additional evidence in support of the Control System hypothesis is offered in Huffman (1996), who analyzes the use of ditransitives and datives with the word *to* in three texts, and in Stern (2016), who illustrates that Control meanings in transitive and intransitive constructions can account for speakers’ choices between utterances such as *she behaved herself* vs. *she behaved*.

- (19) a. Mary taught Bill French
 b. Mary taught French to Bill.
- (20) a. Mary showed her mother the photograph
 b. Mary showed the photograph to her mother (but her near-sighted mother couldn't see it)

Goldberg notes that the (a) examples imply that the activity (*teaching, showing*) was successful, whereas in the (b) examples, there is no such implication. She explains this distinction as follows, and argues that:

these facts can be accounted for once we recognize actual successful transfer as the central sense of the [ditransitive] construction; we need only state that metaphorical extensions have as their source domain this central sense. (p. 33)

While we would want to collect actual instances of use to confirm the intuition, Goldberg's observation would be explained by the hypothesized meanings of the Control System. In the 3-Controller (a) examples, Bill and her mother are MID Controllers. That is, they are grammatically signaled to exert a certain amount of Control over the events (*teaching, showing*). They have a greater degree of participation in these events in the 3-Controller examples than in the corresponding 2-Controller (b) examples where, as objects of prepositions, they are not grammatically categorized as exercising any Control at all.

To reiterate, both Goldberg's (a) and (b) sentences express events with three participants. However, under the CS Control System analysis, the difference is that in (a) all three participants are categorized and ranked as Controllers, whereas in the (b) sentences, only two of the participants are categorized and ranked as Controllers. This is a grammatically encoded structural and semantic difference. This analysis accounts for both the sameness in message, as both describe three entities in relation to an event, as well as the difference in message, which is the implication of successful completion in the (a) examples and the absence of that implication in the (b) sentences. Here again, the meanings (the linguistic system) explain the distribution of forms which are chosen by speakers for the purpose of communicating messages (the use of the system).

Conclusions

Both Columbia School and Cognitive Construction Grammar view grammar as primarily semiotic, and for both approaches, 'meaning' encompasses far more than the truth conditions of an utterance. However, the frameworks diverge in where they situate linguistic meaning. For CS, the locus of systemic linguistic meaning is the semantic side of a linguistic sign, the fixed notional content that is invariably paired with a signal. By contrast, for CCG meaning resides in the interpretation of a sentence or utterance, which is what CS calls a 'message'.

CS analyses do not account for either messages or for communication. Messages constitute personal subjective mental experiences, whereas CS analyses address the more directly observable manifestations of language, namely the distribution of linguistic signs in spoken and written texts. In situations where two signs are viable expressive options, accounting for their distribution takes the form of explaining speakers' choices between them. In the case at hand, we have proposed an explanation for speakers' choice between two word-order signs, one for the 2-member Control System and the other for the 3-member System. We have closely inspected instances of each sign in attested language use, and have identified consistent message differences that are plausible consequences of the different Control meanings that have been posited for the two word order signals.

A crucial element of this account is the sharp distinction between the linguistic system and its use. This distinction – between meaning and message – is a hallmark of CS linguistics and is crucial to the success of all its analyses. Because context is infinitely varied, each utterance and each use of a linguistic sign produces different messages; an analytical focus on these acts of communication would necessarily result in a list of message types and subtypes. Instead, in the CS analysis described and applied here, we have identified fixed and stable communicative tools (meanings, in CS terms) that provide an explanation for the distribution of forms, and the choices that speakers make when they deploy these forms in acts of communication.

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Appendix. 14 tokens of *gave the wall a push*

Example	Evidence for the wall’s active participation	Source
They tried another bottle, found a few more candles, lit them, put “Caro Nome” back on the turntable, explored. There were no other rooms, and just one other door, directly under the Greek writing. They opened it, saw a stone staircase, followed it up ten steps – Nat counted them for some reason – came to another leather-padded door with brass studs. “This is so much fun,” Grace said, turning the knob, “like one of those interactive-theater evenings, only for smart people.”	See above (9)	Abrams

(continued)

Example	Evidence for the wall's active participation	Source
<p>The leader-padded brass-studded door opened onto a brick wall.</p> <p>Izzie did something then that made an indelible impression in Nat's memory. She gave the wall a push with the fingers of one hand, just a little push, as though it were a prop that would topple at the slightest touch. The bricks were real; it didn't.</p> <p>There, at the top of the stone stairs, the bottle went around again. Grande Champagne Cognac, Berry Bros. & Rudd, 108. Nat, trying to remember what furniture he'd seen, building a mental tower that would allow them to climb out of the hole in the bedroom ceiling, realized he was a little drunk.</p> <p>Grade said: "I could get on your shoulders and Izzie could get on mine."</p>	<p>Mysterious house: <i>In the near dark; stone wall; pressing and probing; loud click; small section of the wall swung back; revealing darkness; a chill, damp wind; "How far did you explore?"; dungeon.</i></p>	Betancourt
<p>In the near dark, Ker tried to watch Kalan's every move. The Syrnae seemed to shimmer and ripple as he neared the stone wall at the back of the room. At last he seemed to flow rather than walk across the floor. Reaching the wall, he crept up it, clinging like a spider to the stones, pressing and probing here and there with large, clawed hands. At last Ker heard a loud click, and Kalan dropped to the floor.</p> <p>The Syrnae gave the wall a push with one hand and a small section of it swung back, revealing – darkness. A chill, damp wind came from it.</p> <p>"This is the way," he said.</p> <p>Ker hesitated. "How far did you explore?"</p> <p>"I traveled the entire length, starting from the dungeon. The way is very straight, carved through the bedrock upon which Zelloque rests; we will not get lost."</p>	<p>Mysterious house: <i>trick panel; in the dark, feeling ahead of her, wall swinging open</i></p>	Cain
<p>...She stepped past him into the closet and pulled the light chain. "I need dark," she said, closing the louvered door behind her.</p> <p>The slats of the louvered door let in light from the office, so it wasn't really dark, just shadowed, but that was fine. It wasn't the dark Kick needed; it was the solitude.</p> <p>She approached the wall.</p> <p>Funny how stuff comes back to you.</p> <p>Sometimes, when they moved into a new house, the box was already there, and sometimes they had to build one, framing it out, wiring it, putting up drywall. Mel</p>		

Example	Evidence for the wall's active participation	Source
<p>was handy. Sometimes he would let Kick help build the spring loading for the door while he built the mechanically controlled lock. You could do a lot with a tiny speaker, a gear reduction motor, some PVC pipe, and a few suction cups, and an open-source prototyping platform. You could hide a door in plain sight.</p> <p>Kick put her hand in the lower right corner of the closet where the wall met the carpet, and then walked her fingers up five steps And over to the left five steps.</p> <p>“Well?” Bishop asked through the louvered door.</p> <p>“Go away,” Kick said. She made a fist against the wall where her finger had been, and knocked.</p> <p><i>Shave and a haircut.</i> One knock, followed by four quick knocks, followed by one knock.</p> <p>The back of the closet popped open an inch.</p> <p>That sound. She had forgotten the sound the doors made when the spring released.</p> <p>Kick gave the wall a push and it swung open.</p> <p>The louvered door started to rattle open behind her. She glanced over her shoulder at Bishop.</p> <p>Lit from behind, he was a faceless, dark shape.</p> <p>The edge of the bright rectangle of light touched Kick's knee.</p> <p>She scrambled forward, through the door, into the box, slamming the trick panel shut behind her. She moved on her hands and knees in the dark, feeling ahead of her, until she found a corner to sit in.</p>	<p>Mysterious house: <i>narrow section of the stone moved back revealing a dark opening behind. He caught up a lamp which he had placed behind him on the floor, flashed it into the opening, and I saw, extending down from it, a broad sheet of polished brass, pitched at a sharp angle, above which was a wheel and a rope</i></p>	Doughty

(continued)

Example	Evidence for the wall's active participation	Source
<p>"I think by now you can go back to being Cristina Neagu," Vestergaard told her early in 2013. They were getting ready for the first home game in the main group of the Champions League, after having won them all in autumn, and Cristina had scored for each of them.</p> <p>On Monday morning, the 28th of January 2013, during gym training, she told a teammate that she was feeling so good that, if she gave the wall a push, she would bring it down. That same afternoon, during court training, she was chasing winger Ada Nechita and leaped for the ball. When she landed, her knee slipped out from under her.</p> <p>She rolled onto the floor, screaming so loudly that she scared the whole team. "What's wrong, Cris?" Paula Ungureanu asked from the goal. "My knee slipped, my knee slipped," she answered between cries.</p>	<p>Uncertain result; participation of the wall illustrates Neagu's strength</p>	Giuclea
<p>A bit later, Link and the scientist were off through the underground tunnels, with Link leading the way. "Fascinating," said the scientist "The Zora's have a very extensive and impressive tunnel system." "Yeah, I used to get lost in here sometimes," remarked Link. They then came to a dead end. "Hold on," said Link and gave the wall a push with his shoulder, the wall swung round and opened up to the royal chamber. "Ok, here we are," said Link. "Well, let's get down to business," said the scientist, pulling out a curious looking device. "-and what does that do?" Link asked "Cover your eyes and ears," advised the scientist, hurling it towards a wall near the back of the cavern. There was a huge explosion that shook the cavern, leaving a large, but clean rectangular hole. Link pulled his hat away from his eyes.</p>	<p>Mysterious house: <i>a very extensive and impressive tunnel system; 'I used to get lost here'; dead end; royal chamber; wall swings open</i></p>	Gronker
<p>There was a problem of course – but it was only a law. Wall Street's crash of 1929 had led to some restrictions. They called it Glass-Steagall. It prevented insurers and securities firms from also controlling a bank. Attempts had been made to break down this wall, but small banks and consumers had in each case fought back. The last years of Clinton were the best window yet. The market was booming and the Dems had gone corporate. Glass-Steagall remained after a '98 onslaught. Then, Sandy gave the wall a push.</p>	<p>See above (16)</p>	Lee

Example	Evidence for the wall's active participation	Source
<p>Jin looked at Xiaoyu playfully. "I've an idea. This way," he said, as if to show her his biggest secret. They sneaked past the guests and was running away.</p> <p>"Where are you taking me?"</p> <p>The handsome couple came to a complete stop, right in front of one of the tall vined walls. Still holding Xiaoyu with one hand, Jin tried to search for some kind of opening. "Here it is." He gave the wall a push and it swung open. "After you."</p> <p>"Oh my. I-I never knew of this place. But I swear I've gone to every room and corner of this estate." Not long after, she realized that she had said too much and clamped her hands over her mouth. To make things worse, Jin looked at her, an eyebrow raised suspiciously.</p> <p>"I never went to your room without your consent!" she protested.</p> <p>"Never said anything to you, love."</p> <p>Love? Did he just call me 'Love'?</p> <p>"This is a secret place. My parents brought me here when I was younger. I remember it just like it was yesterday. They told me that if I wanted to be by myself ... or if I wanted to spend so much time with someone I love ... then I should bring her here," Jin said, his blush very much evident ... if only Xiaoyu could see it without his back turned on her. "Come, sit beside me." He patted the spot right next to him.</p>	<p>Mysterious house: <i>in front of one of the tall vined walls; tried to search for some kind of opening; "I've gone to every room and corner of this estate."; wall swings open</i></p>	Mako-chan
<p>Tonight I woke up from a dream where I was walking around in my hometown. To be clear it was basically the main street and a side street that leads to my home – just the dreamy super fancy version of it. Well, as I said I woke up from it and thought 'what a silly dream'. Out of a habit I looked at my hand to count my fingers. Lol, they were 6! I easily adapted to the fact that this was a dream and did what I do most of the time. I decided to slip through the wall in front of me. Pressing my fingers against a wall or a desk is one of my reality-checks, so obviously the first thing I do in a lucid dream is merging with these kinds of objects. Yet this time the wall seemed too solid and would not give in. Yet I knew it was a dream and persisted on my request. I gave the wall a push and</p>	<p>Uncertainty of the outcome; dream; discussion of the wall's roll (<i>Pressing my fingers against a wall or a desk is one of my reality-checks, so obviously the first thing I do in a lucid dream is merging with these kinds of objects. Yet this time the wall seemed too solid and would not give in.</i>)</p>	Nukeble

(continued)

Example	Evidence for the wall's active participation	Source
<p>suddenly a square fell in horizontally and duplicated (sic) itself as it was pushed about 50 meters away: a passage was opened – looked very much like an optical illusion (like standing between to mirrors). I went in and could see that the square-formed paths lead to both sides at several instances. I took about the first or second on the right side and started to run – I was keen on exploring this dream reality just like when I play a video game. So on I went and I recognized the world to shackle as an answer to my intrusion. I noted a feeling of being chased and I didn't feel neither capable nor interested in facing the pursuers. That's when I jumped down and ended up in something looking just like the Ether of Minecraft (lots of lava and minerals in pixel-blocks). I had so much fun smashing the cubes and eventually flying up since touching them would pop them up without any effort (kinda like when you're in Creative Mode in the game).</p>		
<p>It was an apocalyptic dream. I was standing in the hallway of the apartment where I used to live, from 1993 to 1998. I felt like the house was rocking slightly. For some reason, I experimentally gave the wall a push. The whole house collapsed. Somehow, when it collapsed, I ended up on the outside of it, looking at the wreckage.</p>	Dream; See above (9)	Omens
<p>A crowd of people was gathering. I assumed they were looking at the wreckage in disbelief, as I was. But when I looked over at them, their eyes were not on the ground but on the sky. The sun was going out.</p>		
<p>It faded to a black ball, a terrible sphere hanging in a slate gray sky. I don't know why the sky didn't fade to black as well, and it didn't occur to me in the dream, but there was something more ominous about the contrast than there would have been with a uniform darkness.</p>		
<p>Wall? What wall? He lazily blinked his eyelids in an effort to clear his vision. The wall was right in front of his nose, and it seemed to be all around him, blocking his view. It was a funny, lightly colored wall, and the squeaking noise seemed to be seeping right through it. Sandy gave the wall a push with his nose, making his own squeak with the effort of pushing.</p>	See above (11)	

Example	Evidence for the wall's active participation	Source
<p>The wall seemed ready to open up, and Sandy suddenly had the strongest of urges to push the wall away from his face; so he pushed, poked, and battered it. He kicked his legs and thumped his tail as much as he could while twisting his body into the shape of a battering ram. He was determined to get that wall down.</p> <p>And he did. With a tinkling and a slurping sound, the eggshell cracked, and a small piece fell from the wall. Sandy poked his nose through the little hole and immediately was assailed with the noisy new world that flooded his senses!</p>	See above (11)	Porteus
<p>Houses never rest; I knew that, but the Trevors' house was especially full of chatter. I could hear the mice, the mosquitoes, the cats crying, and then sometimes from within the walls the sound of large mammals making their way; I heard as they tumbled above me, their play so raucous that bits of my ceiling sifted down, and I heard as they walked right by my bed, nothing between us but cracked plaster, paint, and paper. 'Raccoons,' Annie said when I asked her. 'They live in the walls here.'</p> <p>Every night they came, walking in the wall beside my bed. I started to scratch at that space, trying to widen a tiny puncture already present. I found an X-acto knife and, after checking to see that my door was closed, I used its precise point to trace a small porthole. Flexing my first finger, I gave the wall a push, surprised at how cleanly it all gave way, leaving a quarter-sized hole, perfect for peering.</p>	See above (12)	Slater
<p>Estelle gave a small nod and totaled after her brother. The tower was a place their father wouldn't allow them to go. It was a place of an ancient demon, which not even her father could control. Slowly she peeked her head out from the doorway. It was empty, happy she walked in, it was very old with spider webs everywhere. "look big brother" she squealed happily pointing at the spiders.</p> <p>Her brother let out a small chuckle as he pat her head "I see looks like there was nothing here," he muttered to himself "I'm going to look around outside, go play with your spiders Elle," he said leaving her there.</p> <p>Estelle puffed out her cheeks "stupid brother" she said going after the spiders. She chased them around</p>	<p>Mysterious house: <i>Tower; a place their father wouldn't allow them to go; place of an ancient demon; empty; very old with spider webs everywhere; clicking sound; dark stairwell; scary</i></p>	Soulsenpie

(continued)

Example	Evidence for the wall's active participation	Source
<p>the room until they got close to the back wall. Her eyes widen as she watched them disappear between a crack. "hey that's not fair" she said banging her little fist again the wall.</p> <p>After the third time she heard a faint click "huh?" She gave the wall a push. Slowly it opens enough for her to squeeze herself through. It was a dark stairwell leading down, carefully she took a step down. A strong gust of wind slammed the door shut loudly. Estelle let out a frighten cry "no its scary" she cried. (sic)</p>	<p>Mysterious house: <i>back in the corner, boxes stacked around it; sparkling got off it and looked at the boxes before an idea formed in his processor; he could see the room beyond the corner; a very dark place; he wasn't afraid and kept going; unfortunately, that was a grave mistake; he fell.</i></p>	9a2a
<p>It was official: his creator didn't like him.</p> <p>Seeklet whimpered at his epiphany and tears threatened to fall.</p> <p>He knew he was making his creator angry, but he was only doing it to get noticed. It worked, he did get noticed, but not once did the sparkling get the affection he so desired and was now back in the corner, only this time there were boxes stacked around it so he couldn't leave and put another bolt in his mouth.</p> <p>Saddened, Seeklet leaned against the box wall. Suddenly, the wall moved a little. The sparkling got off it and looked at the boxes before an idea formed in his processor. He gave the wall a push; it gave way easily. A harder push and he could see the room beyond the corner, including the open door not too far away.</p> <p>Fine, if his creator didn't want him, then Seeklet will just leave.</p> <p>Once the boxes were out his way, the sparkling slowly walked to the open door. When he reached it, he noticed that it lead (sic) to a very dark place. He wasn't afraid and kept going.</p> <p>Unfortunately, that was a grave mistake on Seeklet's part.</p> <p>He took one step too many and felt nothing but air. Before he could stop himself, he fell.</p>		

LatinUs* and linguistics

Complaints, conflicts, and contradictions – The anthro-political linguistics solution

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The languages we study, as well as their speakers and our students, would benefit from a re-imagined approach to linguistics – one that underscores the historical, social, and political contexts surrounding the structures we investigate. Particularly for LatinUs and others whose ways of speaking are stigmatized, a linguistics that focuses on forms while ignoring what people say about their lives alienates the members of those groups who are attracted by the study of language, and its emancipatory possibilities. To combat the reproduction of linguistic and educational inequality, I advocate an anthro-political linguistics, emphasizing the central role that power plays in language and exposing the ways in which language is falsely constructed as the root of educational, cultural, social, and political problems.

Keywords: anthro-political linguistics, linguistic anthropology, LatinUs, Spanish in the U.S

Introduction

At the annual meeting of the Linguistic Society of America in 2016, a panel sponsored by the Committee on Ethnic Diversity in Linguistics (CEDL) discussed “Latin@s in Linguistics: Challenges and Opportunities”; *su servidora* was one of four speakers.¹ The significance of the issue and the interest it generated was obvious; the

* The universal U/Us is my solution to the debated o/a/x/@ gender alternatives because it literally represents US, distinguishes singular from plural, is in accordance with Spanish spelling norms, and can be pronounced easily in both Spanish and English. Moreover, the popular x, which does not meet these requirements, also conveys negativity, elimination. But I have not changed the gender markings chosen by others whose opinions or research I cite.

1. The other panelists were Dr. Manuel Diaz-Campos, Dr. Ana Sánchez-Muñoz, and Ms. Erica Verde.

panel was held in a large hall, was well attended, and John Rickford, then President of the LSA, was in the audience. My paper, “Re-imagining Linguistics for the Benefit of All: Nobody Speaks from Nowhere,” was based on my personal experiences as well as on the views of fourteen linguists who responded to my queries regarding the need to re-imagine linguistics; this contribution expands on that work.² The title of my paper was an obvious challenge to the classic generativist concern with linguistic competence v performance, and its focus on the ideal speaker-listener, in a completely homogeneous speech community (Chomsky, 1965). Insisting that everyone speaks from someplace – or nobody speaks from nowhere – underscores the historical, social, and political contexts surrounding the sounds and structures that speakers employ. In my view, this is an essential first step in re-imagining linguistics in a way that would include members of groups who are marginalized, because their ways of speaking and their class and racial and gender backgrounds represent little “capital” in Bourdieu’s sense of who/what is deemed worthy (Bourdieu, 1991). Working with Ricardo Otheguy as a colleague and co-author, I came to appreciate the extent to which we shared, as the editors of this volume note, “a penetrating skepticism towards presumed truths, and a mindfulness of the responsibility of linguists to foster public understanding of language.” A skeptical approach to the “ideal speaker-listener” and greater public understanding are ensured when a greater diversity of speakers, including but not limited to LatinUs, are welcomed and supported in linguistics; that requires a re-imagined linguistics.

The languages we study, as well as their speakers, our students, and the general public, would all benefit from a re-imagined approach to the field of linguistics. Particularly, for LatinUs and others whose ways of speaking are stigmatized, a linguistics that focuses on forms while ignoring what people are saying about their languages and lives, as well as dismissing the socio-political context in which they speak, promotes bad science and also alienates the members of those groups who are attracted by the study of language, and its emancipatory possibilities. Labov has lamented the fact that “relations are not good” between linguistic anthropology and linguistics, noting that when he “entered the field, there was no barrier or distinction almost” between them. He attributes the weak connection to “the isolation of linguistic theory from the description of languages” (Gordon, 2006: p. 346). I call for an anthro-political linguistics (Zentella, 1995, 1997) to strengthen the connection between linguistics and linguistic anthropology in order to produce research that

2. Throughout this paper I cite responses to my November 2015 emailed request for input concerning the lack of LatinUs in linguistics, from Kendra Calhoun, José del Valle, MaryEllen García, Eric Johnson, Rodolfo Mata, John Moore, Kim Potowski, John Rickford, Jonathan Rosa, Ricardo Otheguy, Jacqueline Toribio, and Bonnie Urciuoli. I omit “personal communication” after each quote. I am also indebted to Tracy Rhone and Suzanne García Mateus for their responses.

combats the reproduction of linguistic and educational inequality; I also believe it will attract a more diverse group of scholars to the field. My approach is similar to others in the past, e.g., when Dell Hymes called for reinventing anthropology with “responsiveness, critical awareness, ethical concern, human relevance, a clear connection between what is to be done and the interests of mankind” (Hymes, 1972: p. 7). We must investigate the forms and functions of language in their social context, with emphasis on the central role that power plays, in order to expose the ways in which language is falsely constructed as the root of educational, cultural, social, and political problems. Anthro-political linguistics openly declares its intention to unmask the language and power connection because whether we choose to discuss it or not, there is no language without politics. Central principles include:

1. valuing non-standard ways of speaking, and the switching among varied standard and non-standard ways, disavowing elite definitions such as “double monolinguals” (cf., Heller, 1999);
2. challenging the symbolic domination of English and its ‘naturalized’ connection with Anglo Americans, and the insistence on speaking ‘pure’ Spanish, English, et al.;
3. rejecting ideologies, processes, and structural inequalities that produce rigid linguistic, cultural, and national boundaries, recognizing instead that “different types of identity are neither exclusive nor singular” (Kroskrity, 2001: p. 107).

These fundamental principles of anthro-political linguistics are designed to promote greater linguistic tolerance and educational equity in addition to contributing to linguistic theory, in the hope of attracting committed scholars of varied backgrounds in the process. Above all, anthro-political linguistics demands a fourth step – taking action regarding language issues that promote social justice. Increasing the number of LatinUs in linguistics demands action.

WHY do we need more LatinUs in linguistics?

LatinUs represent the largest and fastest growing minority in the USA: between the years 2000 and 2010, the population grew by 43%, and as of 2016, it totals 57 million, or 18% of the US population. The contribution we make as Spanish-English bilinguals is profound and widespread, a reality which is of interest to linguists not only because of the research opportunities LatinUs represent, but because linguists champion and protect linguistic diversity. One major point to keep in mind is the diversity within what is often mis-identified as “the Latino community,” as if it were monolithic. The fact is that whereas, as of 2015, “people of Mexican origin account for almost two-thirds (63.4%) of the nation’s Hispanics, another 9.5% are of Puerto

Rican origin, and five other Hispanic origin groups each have more than 1 million members: Salvadorans, Cubans, Dominicans, Guatemalans, and Colombians” (American Community Survey, 2015). Indeed, Spain and all the nations of Latin America are represented in “the Latino community” in the US.

Clearly, linguists who come from a variety of LatinU communities are in the best position to deepen our understanding of many research questions involving languages and dialects in contact, second language acquisition, and bilingual competence and performance, among others. Their investigation of these issues challenges the misguided notions of “the ideal bilingual” who never switches codes “in unchanged speech situations, and certainly not within a single sentence” (Weinreich, 1953: p. 73) or the “ideal speaker-hearer” as the model generator of deep grammatical structures, with no role in grammar for language use. The application of theories that ignores the specific “somewheres” that all speakers speak from, i.e., their class, ethnic, gender, racial, generational, and regional locations, necessarily results in incorrect analyses. Dominicans who identify as Black Americans in some contexts will adopt the lexicon, phonology, and syntax of African Americans that may disappear when they interact with other monolinguals or bilinguals, or in other settings, and Puerto Ricans in predominantly Mexican communities in the US will/will not adopt Mexican Spanish depending on the extent of inter-group contact, individual class status, and racial identification (Zentella forthcoming). Young ChicanUs on the US–Mexico border may accept Spanglish, but begin to act like La Migra Bilingüe (the Bilingual Border Patrol), patrolling the borders between English and Spanish, as college students (Zentella 2013). Only close attention to the many social and linguistic variables at work in each case will achieve the best results, and researchers with community experience and linguistic knowledge [in- or out-group members] can help pinpoint and analyze those variables successfully. I thought the articulation of syllable final –s by Dominicans, referred to as “*hablar fiSno*” [‘speaking fine,’ with intrusive s], indicated class pretensions, until Dominicans explained that it was also considered a gay marker.

Moreover, when members of racial/ethnic and language minorities clarify and draw upon the difficulties they face in an increasingly English-only nation in their analyses, they enhance our ability to address questions regarding language acquisition, proficiency levels and loss, as well as language reclamation, language education, and language policy. But the disturbing figures regarding educational achievement in our communities reveal a major hurdle that must be overcome before the ranks of LatinU linguists can increase: although 86% of Hispanic students were born in the US, and the vast majority are fluent in English, their high school graduation rates are low (76.3 % in 2013–14) (U.S. Department of Education, 2015), only 13% have a bachelor degree, and only 4% have completed a graduate or professional degree (Díaz-Campos, 2016). Sub-groups of distinct national origins differ radically when the percent of earned college degrees is compared: “less

than 10% of Mexican, Honduran, and Salvadoran populations hold a bachelor's degree (Puerto Ricans = circa 12%), while 32% of Venezuelans and about 20% of Argentineans and Colombians have similar levels of degree attainment" (Zerquera & Flores, 2016: p. 2). I take these data to be a call to action. One part of the solution involves the recruitment and training of future linguists who can teach and work with educators and professionals in the legal, health, and social service fields to ensure that LatinUs succeed in school and on the job, and live healthy lives. And encouraging LatinUs to become excited about the study of language can help ensure their academic success.

There are linguists, however, who believe the problems LatinUs face are not widespread, or are part of problems too large for linguists to solve, e.g., one reviewer of our LSA panel abstract doubted that our presentations could make an exciting, new, or meaningful contribution:

I found the overall idea of the panel to be less than exciting. It is well known that some parts of the Hispanic community are economically disadvantaged and this translates into educational disadvantages. Other parts of the Hispanic community in Florida, Texas, New Mexico, and California are not similarly disadvantaged and there is no evidence that these parts are educationally disadvantaged. So it's not clear that the issue at hand is one that linguists can intervene in directly except as concerned citizens interested in creating economic equity. I am also sceptical about the solutions suggested [with all due respect to the excellent work of Montrul and Camacho] – American varieties of Spanish and the Spanishes spoken by the Hispanic community are the subject of intensive linguistic inquiry. There are major NSF funded projects on New York Spanish, Boston Spanish, and so on. So it's not clear to me what this panel is supposed to achieve. I'm not sure that the people attending this panel will learn much that is new or come with some new ideas of how to change the status quo. Score: 2/5 (anonymous panel reviewer, LSA 2015)

Although this reviewer appeared to accept our panel's objective as worthy; s/he doubted the need for it, as well as our ability to have an impact on issues s/he attributed to economic inequity. As my experiences and the comments of the linguists who corresponded with me will make clear, the issues that contribute to low numbers of LatinUs in linguistics go beyond economic burdens and educational disadvantage, to the heart of the misrecognition of our needs and abilities, and to other powerful barriers that push us away/out. The fact that a few well-funded studies of Spanish in the U.S. exist should not obscure the problems that linguistics in general, and linguistic departments in particular, must resolve in order to become more inclusive, since those studies do not address the problems related to diversifying the field and attracting LatinUs directly. And it is worth noting that the two NSF funded projects mentioned by the reviewer are both the result of the efforts of Ricardo Otheguy, with some help from *su servidora*, and Otheguy's student, Daniel Erker.

But whether or not linguists should get involved in taking a stand at all is more fundamental. In the now famous 1992–93 debate between Ladefoged and Nancy Dorian concerning language reclamation, Ladfoged insisted that the job of the linguist is to analyze, not intervene. In Ladefoged’s opinion:

We must be wary of arguments based on political considerations ... it would not be the action of a responsible linguist to persuade them to do otherwise [not give up their language] ... In this changing environment, the task of the linguist is to lay out the facts concerning a given linguistic situation.

(Ladefoged, 1992: pp. 10–11)

In response to Ladefoged’s “just the facts, please” view, Dorian insisted that “It seems a defensible intellectual as well as emotional position to hold that each loss in linguistic diversity is a diminution in an unusually powerful expression of human cultural life, given the nature of language” (Dorian, 1993: p. 578). She advocated, and personally contributed to, language reclamation projects. In my view, Dorian was correct. Ladefoged ignored an important contradiction in his position, i.e., the act of supplying or omitting socio-political facts, not just linguistic facts, are both political. That is why we need an anthro-political linguistics.

Still other linguists believe that it is akin to racism to address the needs of LatinU students in particular; this was the Chair’s response when Ricardo Otheguy and I petitioned a graduate linguistics department to take specific steps in order to encourage LatinU students and future applicants:

It is discomfoting and ultimately unfair to our students to think of them in ethnic terms ... I conclude that Hispanic students do not differ in any academically relevant way from any other group of students.

(Linguistics Dept Chair, 2001 letter³)

Anyone who has taught many students from diverse backgrounds would agree with Stanford professor and former LSA President John Rickford’s emailed observation: “I think the issues that affect one group also affect the others, although of course there are issues specific to each group.” He goes on to note, regarding LatinUs and other minorities, that there is “a lack of sensitivity and knowledge on the part of faculty, and administrators and students, although there are some striking exceptions.” The negative response that Otheguy and I received to our request to diversify the department – more of which will be cited below –, and the failure of our effort, more generally, was in part responsible for my leaving NYC for a job in California, in Ethnic Studies.

3. The Chair (whom I have not named) wrote a four page single spaced letter that summarized the faculty’s deliberations on the issues that Otheguy and I raised.

Attracting LatinUs

We do not know how many LatinUs become linguists; the membership data that LSA gathers is voluntary. Of the 3,705 people who signed up for membership, 2,180 did not mark an ethnicity. Only 91 identified as “Hispanic or Latino”; 58 were students (David Robinson, LSA Director of Membership, personal communication, 4/10/2017). Few LatinUs present papers at linguistic conferences, and most linguistics professors have taught many classes with no LatinU students. Even in NYC, San Antonio, or Chicago, where LatinUs are a significant percent of the population, relatively few are in college, fewer in graduate school, and very few in linguistics. There are almost no organized efforts to attack the problem, as if there were no solutions possible. In the opinion of Jonathan Rosa, a Stanford faculty member of Puerto Rican background:

Hegemonically White institutions and Departments often performatively throw their hands in the air and feign ignorance in the face of Latin@ under-representation (as well as the under-representation of other minoritized groups) ... This is a no-brainer and it is insulting that White people who are otherwise positioned as experts about everything act like they do not know how to solve this problem.

Resolving the problem necessarily involves recognizing and addressing the sources. Professors with many years of teaching experience in universities with a large proportion of LatinU students point to a variety of challenges that those students face. Maryellen García, retired from UT San Antonio, underscores the personal and family issues that deter LatinUs from going beyond the B.A.:

It is no secret that getting an advanced degree takes time and money apart from the passion for the subject matter that provides the impetus to jump through the hoops required by the academy. Therefore, we enter the workforce later than our non-academic peers or we postpone the degree in order to start a family. In some cases, this means that the masters or doctorate is postponed or jettisoned altogether.

Moreover, the few LatinUs who take a linguistics course rarely pursue the major. Kim Potowski’s experience in Chicago is not unusual: “I’ve had close to five hundred Latin@ undergrads (in my classes to date), but I’ve had only two Latina undergrads go on to graduate study in linguistics.”

Of course, the problems involved in attracting linguistics majors are not limited to LatinUs; there are no/few high school classes that introduce young people to the field, almost no one has met a linguist who talks to them about their work, and who even knows what linguists do? If they try to define it, many people assume linguists are polyglots. One recent UCSD PhD in Linguistics, Rodolfo Mata, of Mexican background, shared his experience:

When I told my family [about choosing to major in Linguistics], no one had any idea what I was talking about and no matter how many times I explain it they still think I am learning a bunch of foreign languages so I can one day be a UN interpreter. ... Since there are very few Latino/as in linguistics, word-of-mouth about the field doesn't spread in our communities ... it is not very clear what one does with a linguistics degree.

I'm sure many of us who are much older than Rodolfo are still trying to explain linguistics to our loved ones. These problems are not unique to LatinUs; still others pose more serious challenges to all working class and racialized language minority group members who must support their families. Bonnie Urciuoli, who has mentored many LatinU undergrads in upstate NY (Hamilton), including Juan Valdez, who earned his doctorate at CUNY where he studied with José del Valle and Ricardo Otheguy and interviewed fellow Dominicans for our NYC study – observes that – “while they often take courses, it rarely develops into a major, perhaps because any such major might seem impractical?” John Moore, once the Chair of the UCSD Linguistics Department and later Provost, confirms that the increase in STEM and Economics majors by minorities “of course, is driven by perceptions of which degrees are marketable.” Financial difficulties make the pull of high paying opportunities from industry impossible to ignore. John Rickford recalls, “We had an excellent Latina grad student, but she got recruited by AT&T’s excellent program for attracting women of color into their management ranks.” The need to provide for our families is often paramount, and the linguistics jobs available are too few and too low paying to meet those needs. But if linguistics were re-imagined and its relevance to many fields, including in STEM, were made clear, it would generate greater interest and become more profitable investments for universities and business enterprises.

Access and admissions criteria

Many, if not most, of the colleges that LatinUs attend do not offer a major in Linguistics, as Eric Johnson noted, “since there is an over-representation [of LatinUs] in community colleges as compared to universities (see the 2011 minorities in education report) – and most community colleges don’t have robust linguistics programs.” He wonders whether or not enrollment in linguistics courses at Hispanic Serving Institutions (HSIs) reflects the higher percentage of LatinUs in the university or if it is consistent with national trends. In any case, all of these institutions offer Spanish majors, but their Spanish departments usually do not teach courses that focus on the structure of the language, stressing literature instead. Most undergraduates never hear about linguistics, and some run across it accidentally, as

in Rodolfo Mata's case: "I didn't know what linguistics was or even that it existed until as a senior I needed a social sciences elective and Intro to Linguistics was the only class that fit into my schedule."

The few LatinUs who apply to graduate programs in linguistics often encounter unwelcoming attitudes, and are discouraged by overly stringent requirements that do not take into account their talents, experience, and commitment. One Latina who had been a teacher applied to another doctoral program – where she ended up doing excellent work – after she was discouraged by the Linguistics department: "My GPA was a concern and [the Lx faculty member she consulted] said that I had the lowest compared to all the other applicants (I have a 3.78)." One Latina Linguistics faculty member at a Tier I institution acknowledged the mismatch between what some LatinUs have to offer, and what the department deems is necessary, especially regarding formal test performance: "I can say that Latino students often do not show the so-called 'quality indicators' of GRE and GPA that our colleagues and administrators seek."

My personal experience with the GRE is quite illuminating. When Bill Labov and Dell Hymes reached out to researchers in the New York Puerto Rican community for prospective students in 1976, the University of Pennsylvania offered me a University Fellowship, and I jumped at the chance to work with such esteemed linguists and activist scholars. At the end of my first year, I was informed that U Penn was remiss in not having required the GRE, and I was instructed to take the test. I refused, because the GRE seemed an unnecessary waste of time and money, i.e., it was supposed to indicate whether or not a candidate was capable of graduate level work, and I already had a Phi Beta Kappa undergraduate key, straight A's earned in an MA in Romance Languages and Literatures, and straight A's in one year of classes at Penn. Determined to stand my ground, I wrote up an anti-GRE statement (before the computer age, so unfortunately I can't find a copy) which I handed out throughout the Linguistics Department, where Labov taught, and the School of Education, where Hymes was Dean. The university stuck to its guns until it relented at the last minute. Who knows what I would be doing now, had I left.

What kind of linguistics?

It is sometimes through language and literature studies, as in my case, and sometimes via exposure to linguistics in anthropology or education courses, that students become interested in linguistics. But the kind of linguistics they encounter can be discouraging, even disturbing. Professor José del Valle eloquently pointed out that hegemony and belligerence serve to promote a narrow approach to linguistics that dominates the field and discourages LatinUs:

Linguistics (as an institutionalized intellectual practice anchored in professional associations, conferences, and university departments) has been dominated by the pursuit of formal theories of language. This dominance has been, at times, hegemonic (it has operated with our consent) and, at other times, belligerent (it has operated through open exclusion of and aggression to alternative approaches).

Because of del Valle's grasp of the issues LatinU students face, and his own research on the political ramifications of Spanish purism and colonialism, Otheguy and I urged the Spanish department at the CUNY Graduate Center to hire him, where he has mentored a number of LatinU scholars.

There is evidence that students are dissuaded if their interests lie outside of a purely formal approach. Prof. A.J. Toribio wrote that "For some time, sociolinguistics was poo-poo'd as not real linguistics." She reminded me that when I was a visiting professor in Linguistics at Cornell in the late 1980s, while she was a graduate student there, her interest in the variation course I was teaching, and that of other LatinU students, was treated dismissively. Jonathan Rosa also underscored the negative impact of an overemphasis on universal grammar: "Prevailing approaches to Linguistics as the study of Universal Grammar have their merits, but a potentially unintended consequence is the marginalization or erasure of scholars and scholarship that focus on culturally specific linguistic form and function." The lack of a community focus is another gap noted by Otheguy: "In many Linguistics departments, the heart of the program is still centered on a linguistics that needs little involvement with the community, since the data can come from one's own intuitions. Perhaps Latinos still want the kind of work that is more community centered, and they find it hard to locate those kinds of programs, especially in a climate where there are no jobs."

Concrete examples of the hegemony and belligerence that del Valle suggests are the heart of the problem are evident in the response by the department chairman, reporting on the deliberations of the faculty regarding the concerns that Otheguy and I had raised:

The unanimous response of all the relevant faculty, including me, is an emphatic denial about the content of our courses, and an equally vigorous rejection of the notion that we should change our teaching the way you want us to. We do not see the content of our courses as "very narrow". In fact, most of us explicitly teach our subject matter so as to inculcate within our students a scientific attitude and methodology that is applicable to a broad range of areas of language study. One major goal of these courses is to equip students to read the professional literature in linguistics critically; the material students read in these courses is drawn from the writings of well-established scholars and published in the leading journals of the discipline. Our selection of materials reflects a theoretical orientation that is

well within the mainstream of American linguistics. Most, if not all, of the doctoral programs in linguistics that are highly ranked by the National Research Council share the theoretical orientation reflected in our courses.

(Linguistics Dept Chair, letter, 2011)

Apparently, the faculty were unaware of the reasons why some students who are interested in “a scientific attitude and methodology” see a need for – and no contradiction involved in – incorporating a “theoretical orientation” that enables/encourages linguists to study language in context and to work for linguistically relevant social justice and change. As José del Valle acknowledges, “while Latin@s may very well be interested in language as a formal system (and some do indeed pursue careers in formal linguistics), I’d dare say that Latin@s often experience language POLITICALLY (the Latin@ language experience is necessarily political) and do not see in Linguistics tools to confront that experience through systematic intellectual models.” Why/how do we experience language politically, and how can a re-imagined linguistics make a difference?

We want a linguistics that will help us respond effectively when our varieties of Spanish are denigrated by Spanish speakers and non-Spanish speakers alike, when our relatives are ridiculed because of their accents in English, when we are dismissed on the phone by prospective landlords, and when we are treated unfairly by teachers who place us in a lower grade because Spanish is spoken at home. Another personal example reflects the dual standard that applies to bilinguals, further convincing us that our linguistic skills are devalued. At my Junior High School graduation, I was denied the Spanish medal although I earned 100% on all tests; “because you speak it” was the reason given. It was awarded to someone who earned 96%. But I was refused the English medal (with a 98% score); that was awarded to the monolingual English speaker who scored 99%. These are powerful language lessons that the linguistics we pursue must be able to explain, confront, and change.

The natural connection between being a Spanish speaking LatinU, or a member of a Spanish-speaking family, is in our favor as we attempt to attract LatinUs to linguistics. In fact, Otheguy reminds us, “there are Latinos in linguistics, but NOT in Linguistics departments ... most of the jobs for Latinos in linguistics are for those who do Spanish linguistics in Spanish departments.” Toribio confirms this is the case in Texas: “Despite the large number of Latino students in the UT system, we find that the Latino linguists are represented only in language departments and/or in satellite campuses.” Even more problematic, however, is the fact that the Spanish departments are not only not hiring many US LatinUs, they tend to favor scholars from Spain instead of the Caribbean or the rest of Latin America. Based on 2015 data, an article in the *Chronicle of Higher Education* corroborates this reality, decrying the “Colonialism in Spanish Departments”:

... nearly all Spanish-language departments in the United States are overwhelmingly Eurocentric. ... while many departments in the data set did not have a single Mexico specialist, 97.7% of departments surveyed had multiple specialists on Spain. (Herlihy-Mera, 2016: p. 1)

Sadly, this is the case in both the literature and linguistics sections of Spanish departments. Entering a Spanish department for graduate studies or seeking a position in one after graduating with a linguistics degree is all the more daunting for US LatinUs who must compete with candidates raised in Spain and other Spanish speaking nations. Otheguy attributes this to the fact that the faculty in Spanish departments “want people like themselves who came from Latin America or Iberia and find it hard to identify with the U.S. born.” But it is also likely, as Toribio notes, that “there is the idea among some on admissions committees that U.S. Spanish speakers do not command the language.” When our Spanish is viewed as unacceptable, this compounds the linguistic insecurity many of us already suffer about the varieties of English that we speak and write. I had a high school teacher who made me practice pronouncing “which,” “whether,” etc., with an initial aspiration (/hwhich/, /hwhether/); she also insisted that ‘room’ and ‘roof’ not have an /u/ sound, but one approaching the vowel in ‘rum’ and ‘rough.’ The lack of post-vocalic r in my New YAWK English was yet another problem. Sadly, the Spanish department was not a refuge.

Struggling against prescriptivism and linguistic insecurity

Toribio laments “the prescriptivism that pervades the field; the ‘standard’ language data that is typically illustrated for analysis does not correspond to our experiences. And we perhaps don’t do enough to explain the fact that non-standard does not mean sub-standard; more commonly, we discuss our own speech under the rubric of variation.” Her personal experiences as a speaker of often maligned Dominican Spanish, both as a linguistics graduate student and as a professor, are illuminating, albeit painful:

Even as a graduate student, my judgments about my own language were dismissed as invalid (whereas the Argentine and Peninsular students’ judgments were always taken as given). In fact, my own advisor consulted with a Dominican linguist to see whether my data could possibly be reliable. In sum, a central issue is linguistic insecurity, which many of us arrive with and which is exacerbated in the classroom.

(Even after 20 + years in the profession, I don’t even use my native variety with colleagues or in the classroom.)

In my life, my exposure to my mother's Puerto Rican Spanish and my father's Mexican Spanish enabled me to experience the vagaries of linguistic discrimination first hand. In NYC, my Mexican Spanish was much preferred over my Puerto Rican Spanish, but when I was teaching Spanish to Peace Corps Volunteers in Texas, the opposite was true. Thankfully, confronting diverse attitudes triggered my interest in language variation and attitudes to varied dialects, which propelled me towards linguistics and linguistic anthropology. But many other LatinUs are discouraged, and dissuaded. At Potowski's university, where circa 60% of the Spanish majors are LatinUs, students complain to her about the attitudes and grading practices of some colleagues, but do not dare stand up for themselves:

They tell me frustrating stories about some of my colleagues (both TT and lecturers) going all red pen on their written work. This undermines the possibility of them seeing that their families and communities are linguistic gold mines, and that they as insiders are uniquely positioned to 'mine' them and contribute to linguistic understandings. Even though these students are armed with knowledge about variation – which is in part what empowers them to come talk to me about the misguided feedback from my colleagues – they don't feel they should dare stand up for their linguistic rights to these instructors.

Understandably, Potowski believes that LatinUs ask themselves, "How can I possibly make as my profession a language that I've been criticized about my whole life?"

The fourteen linguists who responded to my questions included graduate students, junior and senior faculty, and emeritae; only six were LatinUs; I am grateful to them for taking the time to send helpful comments. In addition to the problems discussed above, respondents mentioned several other obstacles, including lack of mentoring, since many Linguistics departments, as Rickford reminds us, "don't have profs who know much about Spanish or Latino English, so often are not ideally prepared to advise students with interests in those areas." But even in the rare department where such mentors exist, as in the UCSD Linguistics department, where "three faculty work on Spanish and one is Mexican (and works on indigenous languages of Mexico)" – constituting "one third of the faculty" (Moore), there are only two LatinU graduate students, although 15.8% of the undergraduates are LatinUs. Still, that situation is much better than what is true for other under-represented groups: "We [at UCSD] have no African, African American, or Native American graduate students, although we have a few faculty who work on African languages and, again, one who works on Native American languages." Of course, there are those who are not interested in studying the ways of speaking that are linked to their cultures, and they face the problem of possible "tracking." An African American graduate student explained that "Linguistics students of color may feel like they

are expected/obligated to do sociolinguistics and do research on issues of [their] language and race/ethnicity” (K. Calhoun). Clearly, we have our work cut out for us if we are to attract LatinUs, and enable them to pursue the kind of linguistics they are most drawn to; fortunately, my correspondents offered many excellent suggestions for how to proceed, and succeed.

Concrete solutions for attracting and ensuring successful LatinU majors in linguistics

The following solutions focus on recruitment, mentoring, tailoring course topics and examples, and encouraging community relevant assignments and research.

Recruitment and retention in high schools and colleges

Perhaps the most ambitious suggestion, and the one which has resulted in one award winning project, involves introducing linguistics in the early grades. Rosa maintains that the Linguistic Society of America nationally, and Linguistic departments locally, “should promote the study of Lx in K-12 schooling so that the field will be more recognizable at the college and university level.” Mary Bucholz is the leading innovator in this effort at the high school level; her SKILLS (School Kids Investigating Language in Life and Society) program earned her the 2015 Society for Linguistic Anthropology Award for Public Outreach & Community Service. SKILLS represents the first attempt to teach linguistics in California’s public schools, with the collaboration of UC Santa Barbara faculty and students (undergrad and grad), as well as Master Teachers in high school classes in Santa Barbara County. Students are placed “at the center of linguistic discovery by guiding them through the process of carrying out original research on language use in their own peer groups, families, and communities.” Hundreds of high schoolers have formally presented their findings, examples of which appear on the SKILLS web page, including the curriculum (<http://www.skills.ucsb.edu/>).

Reaching out to high school students can be rewarding for the students, the undergraduates and graduate students who welcome and orient them, and the faculty who participate. I found it enlightening to have high school visitors sit in on one of my courses, and interact with the college students. Toribio suggests that, in turn, linguistics faculty should visit high schools and colleges to introduce the field, “further adding to our service duties,” so she wonders if “Perhaps the LSA could sponsor a fellowship/stipend/travel funds for faculty members to recruit Latinos?” At UCSD,

I assigned graduate students to accompany me on a visit to the high schoolers who attended my class. More consistent relationships might encourage future linguists, but even one visit helped the graduate students re-think their research topics and methods in ways that could be explained to adolescents.

Financial support is essential. Because recruitment is adversely affected by the economic hardships that force many LatinUs to seek work instead of entering college or going on to graduate school, Rosa urges offering “admission/scholarships to Latin@s in the top 10% of their graduating classes (that’s an arbitrary number, but some such program that identifies high achieving Latin@ students should be implemented).” As tuition burdens increase across the nation, some states, e.g., NY, are considering offering free tuition; without such a program I would never have attended college.

To make linguistics more interdisciplinary and compelling to undergraduates, Mata recommends that “Departments of Linguistics forge closer relationships with departments of Spanish, Education, Chicano/Latino Studies, Ethnic studies, Anthropology, etc., to offer joint degrees and market the field differently.” Moreover, if – as Rosa suggests – universities asked “about prospective students’ language backgrounds on application forms” departments could share that information and reach out to potential majors. Another way to retain LatinUs, in Potowski’s experience, is to encourage students to attend “summer research programs like SROP (<http://grad.uic.edu/summer-research-opportunities-program-undergraduates>)”; linguistic faculty can offer introductory lectures. Because student exchanges also offer invaluable learning experiences relevant to linguistics, Potowski also urges that universities “establish a pipeline with universities in Mexico and other countries, to encourage student exchanges. This connection would mean more TAs from Mexico, with whom our Latin@s might identify more (claro, assuming the former are sociolinguistically informed and not prescriptive jerks) y por ende decide that they, too, could pursue linguistics as a profession.”

Finally, the Linguistic Society of America can play a significant role in the recruitment and retention of LatinUS, by sponsoring symposia and panels that present linguistic research by and/or relevant to LatinUs, by organizing workshops that discuss the problems and solutions, and by offering travel awards to undergraduates and grad students who present posters on their research. Finally, an LSA produced FAQ brochure on LatinUs and Linguistics could explain the field to LatinUs and encourage high schools, colleges, universities, and departments to undertake specific recruitment efforts.

Provide mentors

LatinU university students at all levels require effective mentoring in order to succeed, and those mentors should include LatinU faculty. Research proves that “Students who share racial and/or gender characteristics with their teachers tend to report higher levels of personal effort, happiness in class, feeling cared for, student – teacher communication, post-secondary motivation, and academic engagement.” (Egalite & Kisida, 2017), yet many LatinUs have never had a LatinU teacher (Pratt, 2016). Accordingly, Rosa advocates implementing “faculty recruitment programs specifically geared toward admitting and hiring Latin@s and other minoritized groups” ... “No Lx or Spanish Department in the U.S. should be staffed without Latin@s at all ranks (i.e., adjuncts as well as assistant, associate, and full profs). Additionally, these Departments should include an emphasis on US Spanish/ Spanglish.” Enthusiastic and concerned mentoring helps students succeed, and provides a model to be emulated.

Tailor topics and examples: “Discouraging professors should be avoided”

Many linguistic examples, including syntactic structures, morphology, lexicon, phonology, and features of discourse, can be tailored to student interests and experiences in order to engage them in dynamic analysis. “Flying planes can be dangerous” is probably the most well-known sentence in syntactic studies, but the same analysis of its deep structure that explains why/how its ambiguous surface structure can refer to “planes that fly” or “the act of flying” can be applied to “Starving children can be dangerous,” “Discouraging professors should be avoided,” or “Murdering policemen must be stopped.” Examples and topics that resonate with LatinUs can generate enthusiasm and provide excellent research opportunities; these include bilingual language acquisition, hate speech, languages and dialects in contact, regional/gender/class/age variation; music/rap/poetry styles, Latinx and other gender markings, the dependence on English “likes” v “o seas” in Spanish, *voseo* v *tuteo*, formal v informal greetings and leave takings, bilingual jokes, and texting styles. Students who can link their personal interests and community concerns to their academic studies are more likely to remain committed to accomplishing their goals.

Encourage students to relate linguistic studies to social realities

In addition to adopting/adapting examples, topics, assignments, and teaching formats in ways that involve and engage students in linguistic studies, majors that cross several relevant fields help attract and retain LatinUs. Mt. Holyoke’s

innovative and united Spanish, Latina/o, and Latin American Studies department was instituted in 2008 with the goal of preparing students “both to operate in a translangual context and to understand themselves and others against the transcultural and transnational backdrop of twenty-first-century societies” (Magaña, 2013). We trust they reach out to the college’s linguists, and that linguists reach out within and beyond their field. Rosa suggests that “Sociolinguistics, Applied Linguistics, and Linguistic Anthropology scholars, courses, and requirements should be integral to any vibrant/viable Linguistics program.” It is also advisable for faculty who teach the core courses in Linguistics to consider how they might follow Calhoun’s suggestions: “highlighting (1) the ways that studying linguistics can help Latinx understand their social experiences, and (2) the role of language in movements for social justice and racial equality.” The work of the Task Group on Language and Social Justice of the Society for Linguistic Anthropology is directly relevant; consult its webpage (<http://linguisticanthropology.org/blog/category/language-and-social-justice/>) to sign up for its listserv, and for links to its many projects. These have included successful efforts to eliminate the “I” word (“illegal” immigrant), to challenge the demeaning use of Native American mascot names, to eliminate the Census Bureau’s “linguistically isolated” label for those who speak English less than Very Well, and to change the deformed definition of “*españolish*” in the dictionary of the Royal Spanish Academy. Teaching resources including readings, humorous links, videos, and syllabi are also available (<http://teach.linguisticanthropology.org/Categories>).

Encouraging students to write up research they conduct on specific language concerns – local and/or national/international – can also reap important benefits for the larger community. My undergraduates at Hunter College produced the bilingual manual – How to raise a bilingual child/Cómo criar un[a] niño [a] bilingüe (http://potowski.org/sites/potowski.org/files/media/Zentella_manual_0.pdf).

The manual has been adapted by students and faculty in multiple states to provide resources for parents in NY, CA, MD, VA, and Washington, DC, and it is currently being translated/adapted for Mandarin speakers. Book length projects on local communities were produced by my undergraduates at UCSD (*Multilingual San Diego; Portraits of Language Loss and Revitalization*; University Readers, 2009), and at Swarthmore College (*Multilingual Philadelphia: Portraits of Language and Social Change*; Swarthmore Linguistics Dep’t). They also shared their findings in other formats – on line, with a poster at a linguistics conference, and in person at community meetings. While very few of the student writers pursued graduate study in linguistics, their studies in other fields and their responsibilities as professors, teachers, counselors, librarians, social workers, medical personnel, etc., have benefitted from the insights gleaned as a result of the linguistic research they and their peers conducted.

Conclusion

All of these suggestions are meant to help LatinUs find a home in linguistics that does not distance them from their families and communities, or from their fellow LatinU students on campus. One professor shared the experience of a graduate student on whose behalf he intervened:

One student who is doing well in our current program, despite financial and family issues, is doing so in part because of VERY strong involvement with the Chican@ community here – an office in the Chicano Student Center, working with Chican@ students in need, and so on. I don't think the department was enthusiastic when she opted for an office there (outside of the department), but I defended and supported the choice, and it did/continues to do wonders for her state of mind.

In conclusion, in the spirit of Otheguy's commitment to a linguistics that contributes to greater public understanding, I advocate a re-imagined linguistics that strengthens connections to family and community, and underscores the role of language in the pursuit of social justice. In my view, an anthro-political linguistic approach to re-imagining linguistics would strengthen the field, and enable LatinUs and others to pursue a career that would bring them personal joy while allowing them to make meaningful contributions to society as a whole. That has been my experience, as well as that of the linguists whose words I cite throughout this paper. We hope to make it easier for the next generation to achieve those goals.

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Reviving the unicorn

Linguistic reconsiderations for the existence of Spanglish

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This chapter engages the debate on the term *Spanglish*. Ricardo Otheguy asserts that the term *Spanglish* has no real-world referent and is “technically flawed.” In response, it is argued that the term *Spanglish* is neither objectively inaccurate nor technically flawed because the term *Spanglish* refers to a ‘real’ linguistic phenomenon. Furthermore, the necessary conditions for labeling linguistic varieties as varieties are theoretically untenable or practically unimplementable. The chapter also highlights that linguistic systems are mental objects whose existence is inferred from behavioral epiphenomena and that such a set of behavioral epiphenomena is in evidence for Spanglish. The chapter concludes that sufficient linguistic and sociolinguistic evidence exists to posit the existence of, and thereby justify the use of, the term *Spanglish*.

Keywords: Spanglish, Spanish in the U.S., glottonyms, onomastics, naming, linguistic autonomy, linguistic differentiation

1. Introduction

This chapter examines the linguistic and technical evidence for positing or denying the existence of Spanglish. Of late, the linguistic phenomena that constitute Spanglish and the appropriateness of the term itself have been topics of debate among sociolinguists and, particularly, among scholars of Spanish in the U.S. This debate has come to be symbolically embodied by two well-known contemporary opponents on the matter: Ricardo Otheguy and Ana Celia Zentella.¹ The extant

1. See, for example, the “Debate sobre el término *Spanglish*” at the University of Miami in 2009. (Available from: <http://potowski.org/sites/potowski.org/files/articles/attachments/TranscripcionDebateSpanglish.pdf>.)

motivation of both is to validate the linguistic resources of U.S. Latinos, yet each concludes with somewhat contradictory recommendations as regards application of the term *Spanglish* to the linguistic productions that come from the mouths of the bilingual among the U.S.'s largest minority group (U.S. Census Bureau, 2016, "Quick Facts: U.S. Population"). Otheguy suggests replacing the term with one such as *Spanish, popular Spanish of the U.S.* (Otheguy & Stern, 2010: p. 86) or *Spanish in the U.S.* (Otheguy & Stern, 2010: p. 98); Zentella supports continued use of the term *Spanglish* (Zentella, 2016).

Both scholars have highlighted the systemic marginalization of Latinos,² especially as experienced through intolerant educational practices, which themselves reproduce inequity by attacking the language practices of this group (e.g., see García, Evangelista, Martínez, Disla, & Paulino, 1988). Likewise, both scholars seek, ostensibly, to battle the denigration of Latinos as well as validate the speech behaviors of Spanish-English bilingual Latinos by equipping them with linguistic confidence through knowledge. Yet each approaches the task of arguing his/her position armed with different sorts of evidence. Otheguy contends that, rather than continue to use a term (*Spanglish*) that is imbued with negative connotations, Latinos would be better served if it were recognized that their speech practices overlap to a great extent with speech behaviors that already enjoy socio-political legitimacy: those called (*standard*) *Spanish*.³ To argue that the term *Spanglish* should be dropped, and for the good of the speakers themselves, Otheguy (with his sometimes co-author, Stern, 2010) presents an analysis of the linguistic phenomena and processes to which the term *Spanglish* could be referring, and finds that there is

2. *Latino* is used to refer to individuals that claim ancestry to a predominantly Spanish-speaking Caribbean, Central or South American country, although *Latino* in more general usage refers to a person claiming ancestry with any Latin American country, Spanish-speaking or not. The use of the visual symbol *Latino* in the current chapter is used inclusively, but not primarily, with respect to sex and gender identity and is meant to be compatible other visual representations such as *Latin@* or *Latinx*.

3. The term *standard Spanish* in this piece is meant principally to refer to whatever it is that Otheguy and Stern had in mind when using this term. It should be noted that a precise definition of the term *Spanish* may not strictly speaking be necessary to understand this chapter, since it deals mainly with the nature of the principles and arguments put forth by Otheguy and Stern as a basis for naming practices in linguistics. Nonetheless, I offer my own understanding of Otheguy and Stern's use of the term *standard Spanish*. *Standard Spanish* in Otheguy and Stern (2010) seems to refer to the dictionary lexicon and syntax defined as standard by the Real Academia Española. This version is, I perceive, intended to be understood as the syntax, lexicon and phonology shared in common by all those said to 'speak Spanish'; in that way, it is also an abstraction over (or common denominator of) individual differences and other community, regional or national, or other varieties of Spanish.

no particular phenomenon indicative of how U.S. Latinos speak that is either not Spanish (in the current socio-political sense of the word), not English (in the current socio-political sense), and not the type of novel or innovative uses that would characterize any other variety in other parts of the world simply called *Spanish*. In an earlier Spanish-language publication addressing the same topic (Otheguy, 2007), he concludes that Spanglish, quite simply, does not exist. It is like a unicorn: a word with conceptual content but no referent.

On the other hand, Zentella, an anthro-political linguist, would educate her Latino students about the use of labels and the ideologies that racialize, marginalize, and control Latinos, thereby keeping the powerful in their dominant positions (Zentella, 2016: p. 15). Her remedy, one that has been adopted in the past by other racialized or discriminated groups, involves an act of semantic inversion: wresting the term from those who would use it disparagingly and employing it with pride, thereby exposing the systematic marginalization that constitutes their lived experiences. Keeping the term *Spanglish* in play, she says serves the function of “disrupting the hegemonic status of languages associated with nation-states”⁴ (Zentella, 2016: p. 15). That is: we should support the continued use of the term *Spanglish* [not on linguistic grounds, but] because it forces us, as a society, to stop and think.

The impression left by these works is that Otheguy and Zentella, as scholars, are in fact aligned ideologically with respect to the linguistic integrity of the speakers in question, but not with respect to a socio-political game plan for these speakers. To use a baseball metaphor: it seems as though they are batting for the same team, but are playing the game on two different fields. The provocatively clinical and reasoned discussion of Otheguy (with co-author Stern), in particular, seems to merit a response constructed upon commensurately technical grounds.

The purpose of this paper is to engage directly with Otheguy on the theoretical field he has selected in order to argue against his thesis that “Spanglish does not exist” (Otheguy, 2007), which serves as the major premise for his position that the term *Spanglish* should not be used as a technical linguistic term. That is, this chapter will, like Otheguy, enter the debate from a technical, linguistic point of view, arguing that Otheguy’s conclusion that Spanglish *does not* exist is untenable because the linguistic and philosophical premises on which the case is made are themselves questionable. In order to accomplish this, I deconstruct Otheguy’s argument and make recourse to linguistic science and nomenclaturic practice to challenge the validity of the foundational assumptions of his position. The chapter finishes with a consideration of how Spanglish measures up against more reasonable and widely accepted criteria for glottonymic differentiation and linguistic autonomy.

4. This is a quote that Zentella uses from a personal communication with colleague J. D. Rosa (in 2014).

In the remainder of this chapter, I refer to the speech practices of Spanish-English bilingual Latinos in the U.S. as *duolingual*, as in: *duolingual speech*, *duolingual practices*, and *duolingual discourse*. The term *duolingual* is used to avoid implications that would otherwise arise were the expression *bilingual* (*bilingual discourse*, *bilingual speech*) used. *Duolingual*, for example, acknowledges the presence of two or more elements with origins in linguistic varieties historically considered (both by linguists and speech community members) to be distinct languages (i.e., Spanish and English), without implying anything for the time being about the degree of bilingual proficiency of the speaker or what language the speaker believes him/herself to be doing at any one time.⁵ Said differently, expressions like *duolingual speech* and *duolingual variety* in this chapter are intended to be agnostic on the extent or nature of the individual bilingualism of speakers – whether what speakers do with their words indicates that they have a single, unified code, juxtaposed separate codes, or multiple integrated codes (see MacSwan, 2017) – or whether what they are doing counts as a language system distinct from Spanish or English: questions at the very center of this debate.

2. A summary and deconstruction of Otheguy on Spanglish

Otheguy (2007, 2010 with Stern) ponders the linguistic practices to which the term *Spanglish* might apply in the minds of speakers, pundits, poets, and linguists. He considers several linguistic phenomena, including: loan words (e.g., *beismen* ‘basement’), morphological variants (e.g., *noticario* vs. *noticiero* ‘newscast’), semantic shifts and novelty (e.g., *aplicación* ‘(job) application’ rather than ‘(topical) application’), novel phraseology (e.g., *llamar para atrás* ‘to call back’), and syntactic phenomena (e.g., *corriendo es divertido* vs. *correr es divertido* ‘running is fun’ and *enamorarse con* vs. *enamorarse de* ‘to fall in love with’). In each of these domains, he finds that the linguistic features or processes to which the term *Spanglish* might refer do not constitute sufficient grounds to justify the use of a novel label, a label other than *Spanish*. In the remainder of this section, the phenomena are reviewed in order to highlight three criteria he frequently employs to ultimately conclude that each “does not justify our coining a special term [apart from Spanish] like Spanglish” (Otheguy & Stern, 2010: p. 88) and that ultimately the word “Spanglish,” like the word “unicorn,” has conceptual content but “no se refiere a nada” (‘it does not refer to anything [real]’) (Otheguy, 2007: p. 14). It will later be shown that, however intuitively appealing, these criteria are based on unsubstantiated premises

5. Varra (2018) shows, for example, that in New York City almost all Spanish speakers use English lexical borrowings, including individuals that are essentially Spanish monolinguals.

concerning the nature of glottomastic practice and, thereby, misleadingly imply the existence of an established protocol for differentiating and naming language varieties, which cannot in fact be substantiated in theory or practice.

As regards lexical phenomena, Otheguy and Stern apply what I will call the ‘Uniqueness Criterion’ to argue that they do not constitute viable evidence for a linguistic variety meriting the name *Spanglish*. They argue, for instance, that all over the world, Spanish speakers use words from local indigenous languages (e.g., the word *churí* ‘child’ in Montevideo, Uruguay) that are not recognized as standard Spanish (where the word for ‘child’ would be *niño*). Yet, because those varieties are referred to as *Spanish* (or at times more specifically as *Uruguayan Spanish* or *Montevidean Spanish*), so too should the speech ways of bilingual Latinos be labeled *Spanish*.⁶ They claim that because lexical borrowing affects the patrimonial lexicon of all languages, and in particular what is called *Spanish* elsewhere in the world, these phenomena do not constitute “unique or defining feature[s] of [Spanglish] in the USA” (Otheguy & Stern, 2010: p. 90). The same Uniqueness Criterion is applied to dismiss from consideration the phenomenon of semantic shift (e.g., *carpeta* as ‘carpet’ versus ‘folder’; *introducir* as ‘introduce [someone to someone] by name’ versus ‘insert or bring into [something]’) and morphological variations (e.g., *terapista* vs. *terapeuta*), since these also occurs in language varieties called *Spanish* elsewhere. In other words, because these lexical processes and phenomena in the duolingual discourse of U.S. bilingual Latinos are not unique to *it*, the features they give rise to cannot be said to characterize a distinct language. Or, said another way, the assumption is that for glottonymic differentiation to be justified, a speech variety must possess or demonstrate a unique linguistic feature or process. Otheguy and Stern thus conclude that these phenomena are insufficient grounds by which to justify the application of a distinct language name.

6. This discussion also begs the question of whether the term *Spanish* as applied to these varieties is appropriate and whether the term *Spanish* itself refers to anything ‘real’ (see Section 5). I point out in a footnote (#34) that the rationale that Otheguy presents to argue against the existence of Spanglish would apply generally to any linguistic variety, including so-called ‘Spanish.’ That is, no linguistic variety, based on Otheguy’s criteria, could be said to ‘exist,’ either materially (since linguistic entities are immaterial), as abstractions or as theoretical constructs. In fact, in other publications, Otheguy seems to be of the position that what is called *Spanish* does not exist as linguistic or theoretical object either. He says, “a [geopolitically] named language [e.g., Spanish] cannot be defined linguistically (...) such categories are not linguistic but socio-cultural, and as such are extraneous to the enterprise of analyzing the idiolectal features” (with García & Reid, 2015: p. 286, 289; also see Erker, 2017 for a similarly aligned perspective). In that work, the implication seems to be that idiolects are real(er) objects or else linguistically realer objects. The position espoused in the current chapter is that both idiolects and sociolects exist as theoretical concepts (abstracted over linguistic behavior and each serving distinct theoretical purposes) and both have mental reality for speakers, albeit probably of a different order (see Section 5).

With respect to morphosyntactic phenomena⁷ associated with duolingual discourses produced by Spanish-English bilinguals in the U.S., Otheguy and Stern not only utilize the Uniqueness Criterion, but also employ two additional criteria to dismiss these features. The first of these I term the ‘Criterion of Systemic Difference.’ For example, it is well known that verbal paradigms of duolingual discourses, as compared to that which is called *standard Spanish*, manifest fewer options, in particular in the subjunctive mood. In addition, duolingual discourses seem to manifest an increased use of subject personal pronouns (e.g., *ellos dicen que ... y ellos publicaron un ...*) as compared to that of *standard Spanish*. Otheguy and Stern argue that, in instantiating what can be analyzed as reductions or expansions of syntactic patterns available in what is elsewhere called *Spanish*, such phenomena do not represent true interruptions or differentiations in the linguistic system of Spanish as passed from parent generation to offspring.⁸ In other words, for a speech way to merit glottomastic independence from its near linguistic neighbor or parent variety, it should demonstrate systemic difference from that neighbor.

One final criterion features prominently in Otheguy and Stern’s argument: what I call the ‘Criterion of Sufficient Minimal Difference.’ The introduction of this criterion follows on their concession that some syntactic phenomena characteristic of Latino duolingual discourse in fact satisfies the Criterion of Systemic Difference. As part of their review of morphosyntactic phenomena, they concede that at least two phenomena that manifest in Latino speech in the U.S. in fact represent bonafide systemic differences, when compared to standard Spanish. These are: a use of Spanish gerunds in ways that parallel English *-ing* forms (e.g., *después de.. ayudando con la gente*) and novel patterns in verb–preposition collocations (e.g., *estar enamorado con alguien; yo ví ___ Juan*). Otheguy regards these as “verdaderos cambios estructurales en el español popular de los EEUU” (‘true structural changes to popular Spanish of the U.S.’) (Otheguy, 2007: p. 14). Nonetheless, in order to disappear these phenomena from consideration of that to which the term *Spanglish* may be applying, he argues that the incidence of “the presence of structural elements of

7. For the sake of space, I omit mention of Otheguy and Stern’s coverage of “phraseology” (e.g., *llamar para atrás* ‘to call back’). This is because their argument against such phenomena constituting evidence for a novel moniker (i.e., that phraseological novelties, like *llamar para atrás* ‘to call back,’ are not part of linguistic structure, and represent cultural, not linguistic, borrowing (Otheguy, 2013)) is another example where they apply the Criterion of Systemic Difference.

8. Otheguy and Stern (2010: p. 94) also remind the reader that these morphosyntactic processes also characterize speech ways called Spanish in other parts of the world. The implication is here, again, that because these processes are *not* unique to U.S. duolingual discourse, duolingual discourse does not merit a name other than *Spanish*.

English represents a very small proportion of the total [speech sample of a speaker]” (with Stern, 2010: p. 96), and furthermore, that “se hace difícil creer que tan magro material se pueda construir un nuevo idioma” (‘it is difficult to believe that such scant material would be able to constitute [the basis of] a new language’) (Otheguy, 2007: p. 14). In other words, in addition to qualifying as ‘systemically’ distinct from standard Spanish, thereby meeting the Criterion of Systemic Difference, such linguistic phenomena must be quantitatively sufficient in number in the speech ways of a group for that speech way to merit glottomastic independence.

In this way, Otheguy and Stern consider and then discount each potentially so-called Spanglish phenomenon using a combination of three criteria. In other words, they claim that the linguistic features or processes to which the label *Spanglish* potentially applies:

- are not unique to the speech ways of Latinos in the U.S. (Uniqueness Criterion);
- do not represent systemic change in the speech ways of U.S. Latinos (Criterion of Systemic Difference);
- do not occur with sufficient frequency, despite satisfying the criterion of Systemic Difference (Criterion of Sufficient Minimal Difference);

The well-constructed argument, in which these criteria feature, lends the criteria both plausibility as well as communicates theoretical authority. Yet, such criteria are (i) based on unattested assumptions concerning the nature of glottomastic practice, as well as (ii) imply the existence of an established, theory-based linguistic protocol by which varietal differentiation and nomenclaturic determination are justified. Section 4 addresses directly some of the glottomastic misapprehensions that underlie these criteria. I turn now, however, to the task of deconstructing the theoretical ground on which Otheguy and his co-author’s position relies. Section 3 demonstrates simply that even were the underlying assumptions left intact, these criteria are not only *not* practiced in linguistic science, but also they are theoretically unsustainable or impossible to apply. In fact, what we will see is that, quite contrary to what Otheguy and Stern imply, glottomastic differentiation is widely accepted by linguistic scholars for language varieties that differ from each other in precisely the way that the duolingual practices of Latinos differ from standard Spanish. The analysis and deconstruction of their criteria, on linguistic grounds, begins with the Criteria of Systemic Differentiation and Sufficient Minimal Difference, and concludes with the Uniqueness Criterion.

3. A critique of Otheguy's criteria for glottonymic differentiation in linguistic science

Otheguy and Stern have thoroughly examined the linguistic phenomena to which the term *Spanglish* might apply. In rejecting each as a plausible basis for bestowing the term *Spanglish* upon what bilingual Latinos do when speaking, they have employed a number of questionable criteria. Yet, the technical nature of their argumentation suggests that their rejection of glottonymic differentiation for duolingual speech ways follows a reasonable and standard linguistic protocol for making such determinations.⁹ I show here that, with respect to each of these criteria, naming practices in linguistics in fact supports glottonymic distinction under parallel linguistic situations involving other linguistic varieties. In some cases, it will further be argued that a particular criterion is linguistically unimplementable or theoretically unsustainable. Introductory textbooks of sociolinguistics, in fact, provide many of examples where two speech ways differ from a standard (or some other speech variety) in similar ways, but which nonetheless are unproblematically called by linguists and lay people by different names and are considered to be different languages. While it is fairly certain that Otheguy (and Stern) is aware of such examples,¹⁰ it remains now to review these sociolinguistic situations for the sake of argumentation. If the untenability of their criterion, and thereby the role of linguistic evidence in glottonymic practice, is adequately demonstrated, then the existence of *Spanglish* (or not) as a language rests on entirely different grounds (as indeed is the case for glottomastics in general). We shall see what those are in Section 5.

3.1 The criterion of systemic difference: Must a variety differ in its system in order to justify glottonymic differentiation?

Otheguy and Stern note that the distinction between a language as a system and the uses to which that system is put is a conceptual cornerstone of modern linguistics (Otheguy & Stern, 2010: p. 91). It shows up, for example, as the difference between *langue* and *parole* in Saussure's work (1959, 1966) and as the difference between

9. The observation that political (i.e., power) asymmetries masquerade as objective truth in the garb of scientific discourse is, of course, not novel but is a constitutive theme of critical approaches to science. Thank you to an anonymous reviewer for highlighting this point.

10. Indeed, the latter work of Otheguy (with García & Reid) affirms, albeit indirectly, the contention that the linguistic criteria proffered in his argumentation (with Stern) cannot factor into such discussions. He says, "the student of language in sense (b) [as mental or psychological construct] ... has no theoretical basis for adjudicating disputes about separability and namability, since these are social and political matters that pertain only to language in sense (a) [as social construct]" (with García & Reid, 2015: p. 287).

competence and *performance* in Chomsky's (1965), to name but two of the most popular terminologies.¹¹ *Competence* and *langue*, speaking loosely, refer to language in the mind of the speaker, a system of concepts and oppositions that finds expression in concrete gestural, graphic, and phonological symbols employed by humans to communicate or express ideas. These concrete symbols, when used, are *performance* or *parole*. They serve, further, as the data whose patterning suggests to observers that the abstract system, a *langue* or *competence*, exists at all (Saussure, 1966: p. 138).

Several contemporary frameworks for linguistic analysis are primarily concerned with describing and explaining language in the mind, or *langue*. These frameworks, thus, tend to associate the lay term *language* with the notions of *competence* or *langue*: language in the mind. In like fashion, Otheguy and Stern associate the term *language* with *langue* and consider that two speech ways represent distinct languages only if the speech ways appear to result from distinct underlying competences, or systems. By extension, they assert:

For the appellation Spanglish to be justifiable, one would have to demonstrate that there exists in the USA a community of speakers who have a new, and different, underlying linguistic system. (Otheguy & Stern, 2010: p. 92)

Drawing as it does on foundational concepts familiar and fundamental to most practicing linguists, this criterion, the Systemic Difference Criterion, appears merely to be informing the reader of 'the way things are' in mainstream linguistic practice. Examples do exist in which linguists attempt to measure the extent to which two speech varieties differ in order to determine whether what is at hand are different systems or different versions of a largely similar system (e.g., see Simons, 1979).¹² However, the linguistic canon also contains well-known examples where a differentiated language label is not only accepted but also adopted by linguistic experts for varieties that differ systemically little or not at all from a near linguistic

11. The distinction is also captured in Zentella's (1997) work on language acquisition of Spanish-English bilingual children in New York, when she refers to phenomena as *out of head* and *out of mouth*.

12. For example, linguists at the Summer Institute of Linguistics seem to require a difference of at least 15 percent (<https://www.ethnologue.com/about/language-info>) in the lexicon of two speech varieties for them to be given a different language code in the Ethnologue database. Assigning programming codes to speech varieties clearly responds to factors other than establishing whether a speech variety in question represents a new language or a variation in an already existing language. Nonetheless, some of the coders' decision-making seems to in fact rest upon such a determination. It is there written, for instance, that "the ultimate objects of identification are languages themselves" (<http://www-01.sil.org/iso639-3/scope.asp>), further clarifying that two purposes for coding changes include to "split an existing code element into two or more separate language code elements" [presumably because a 'parent' language should be considered to have split into two daughter languages] and to "create a new code element for a previously unidentified language."

neighbor. Several of these examples are so well-known that they comprise the core of the linguistic didactic literature and are included in that canon precisely to demonstrate the idea that language naming is not a matter of systemic or, more narrowly, morpho-syntactic difference.

That language naming or differentiation does not proceed on the basis of systemic or morpho-syntactic considerations is demonstrated in the phenomenon of bilingual mixed languages. Media Lengua is a speech variety that can be described as utilizing syntactic oppositions that overlap almost entirely with Quechua (Winford, 2003: pp. 135–136, citing Muysken’s 1981, 1997 work). The following examples from Quechua and Media Lengua (from Winford, 2003: p. 176) demonstrate this.

- (1) a. Media Lengua
 No sabi-ni-chu Xwan bini-shkda-da
 NEG know-1SG-NEG John come-NOM-ACC
- b. Quechua
 Mana yacha-ni-chu Xwan shamushka-da
 NEG know-1SG-NEG John come-NOM-ACC
 ‘I don’t know that John has come’

In (1a), despite the fact that the lexical roots for ‘know’ and ‘come’ are different in Media Lengua (being related to Spanish *sab-* ‘to know’ and *ven-* ‘to come’) and Quechua (being *yacha-* and *shamu-*), the morpho-syntactic affixes are identical. For instance, in both Media Lengua and Quechua, the verbal suffix for first person singular is *-ni-* and negation is *-chu*. That is, despite the fact that it today shows almost no systemic differentiation from Quechua in its morpho-syntax,¹³ Media Lengua is considered to be different language and is permitted to be distinguished from Quechua through the name by which it is called.

Another example of terminological differentiation where little or no systemic difference in the syntax of two related language varieties is that of Hindi and Urdu. Hindi and Urdu, as described by Gumperz and Wilson (1971), have not only virtually identical syntactic systems, but are also nearly identical in lexicon.¹⁴ In fact, linguists consider the so-called languages Hindi and Urdu to be so similar that they in fact constitute a single linguistic system (previously called Hindustani). Such

13. Thomason says, “the grammar [phonology and syntax of Media Lengua] is almost entirely Quechua” (Thomason, 2001: p. 203).

14. Hindi and Urdu have been diverging over the last several decades through attempts to purify their respective vocabularies from perceived influences from the other language group or its speakers and by borrowing vocabulary from different sources; Hindi takes from Sanskrit, while Urdu takes from Arabic and Persian. This divergence is also supported and reflected in the selection of different writing systems to represent each: Devanagari for Hindi and the Perso-Arabic script for Urdu.

perspectives are reflected in orthographic practices of graphically uniting the two names as in *Hindi-Urdu* or *Hindi/Urdu* (Haspelmath, 2017: p. 83). Nonetheless, these speech ways are often referred to by linguists and speakers alike as just Hindi or just Urdu (see, for example, Thomason, 2001: *Language Index*, which lists them separately). In other words, the Criterion of Systemic Difference does not factor into language naming conventions among lay people or linguists, and counterexamples to the criterion can be found in any Sociolinguistics 101 textbook.

To these examples, the reader might object that certain crucial facts about the history or development of the above-mentioned varieties have been omitted and, furthermore, that it is *those facts* that justify glottomastic differentiation for the varieties involved. For instance, Hindi and Urdu are speech ways associated with different religious and national groups (Hindi with Hinduism and an official India language throughout the country; Urdu with Islam and official in Pakistan, as well as a national language principally in northern provinces of India). Glottomastic differentiation is, thus, justified in order to embody historical, religious, and political divisions, which are still relevant to speakers today. In the case of Media Lengua, one might note that what makes it different from Quechua is that the lexicon of root morphemes (i.e., word stems), which are also by definition part of the linguistic system, appear to be wholly different from those in Quechua, being as they are imported (and linguistically adapted) from Spanish. In other words, it is the unique combination of this particular lexicon with that particular morphosyntax that justifies the nomenclature for Media Lengua.

Both objections are, of course, valid. Both also demonstrate that a criterion of systemic differentiation, as Otheguy and Stern would have the reader believe, are not focal considerations as regards issues of nomenclatural differentiation. In the case of Hindi and Urdu, it is precisely the significance of the political and ideological differentiation of naming their speech ways that is relevant to language naming, *not* a so-called ‘objective’ comparison of linguistic facts. And in the case of Media Lengua, the point is that, in language naming, it is a feeling of ‘novel’ or ‘difference’ as emerging from the system *as a whole* that seems to justify novel nomenclature. I return to both of these issues in Section 4. The point here is that there appears to be no precedent in the linguistic canon that suggests that a criterion of systemic difference factors prominently into glottomastic practice.¹⁵ Our linguistic didactic materials in fact provide counterexamples that shed considerable doubt on the existence or actionability of such a criterion.

15. When languages are named and classified by linguists, they most often try to characterize instincts regarding the continuity or difference between two speech varieties with respect to the notions of genetic relatedness, typological similarity, socio-political role (e.g., see Tetel Andresen & Carter, 2016) and transmission (e.g., see Thomason, 1997).

3.2 The criterion of sufficient minimal difference: How much must linguistic systems differ to be considered different languages?

Otheguy concedes that what is sometimes called *Spanglish* in fact manifests “estructuras inglesas” ‘English structures’ (Otheguy, 2007: p. 13), which, in accord with mentalistic conceptualizations of language, indicate that its underlying system differs from that of standard Spanish. Nonetheless, he goes on to discount these structures as sufficient for distinguishing so-called Spanglish from other languages. In 2010 (with Stern), for instance, he says of an excerpt of a bilingual New York Latino:

the speaker relies on a linguistic mechanism that is almost entirely Spanish, in which the presence (variable, not categorical) of structural elements of English represents a very small proportion of the total. (Otheguy & Stern, 2010: p. 96)

This contention, that those systemic differences that *do* underlie duolingual discourses are few, leads to the conclusion that glottonymic distinction for so-called Spanglish is, thereby, unjustified. In concluding thusly, Otheguy and co-author Stern suggest that (a) language differentiation and, consequently, (b) nomenclaturic distinction *would be* justified where systemic differences reach a particular threshold. This begs the question: What is that threshold? How much systemic difference do linguists accept as ‘sufficient’ in order to accept glottonymic independence from a near linguistic cousin or sibling?

Otheguy’s (2007) work seems to suggest an answer. With respect to English-origin structures in duolingual discourse, he says that the term Spanglish cannot be justified because “nadie ha demostrado que constituyen [las estructuras inglesas] el grueso del habla de los latinos de EEUU” ‘nobody has demonstrated that they [English structures] make up the bulk of speech of U.S. Latinos’ (Otheguy, 2007: p. 13). This criterion of ‘the majority,’ ‘the bulk,’ or even ‘half or more’ seems a reasonable theoretical estimation for a minimal threshold of difference; yet, such reasonableness is only imagined. This threshold is not only *not* borne out in practice, but also cannot be applied with consistency in the actual world. And, linguists, further, have long accepted glottonymic differentiation where a far lesser quantity of differentiation between two varieties is attested. This is true both with respect to lexicon as well as syntax. Several of these instances are reviewed below.

3.2.1 *Glottonymic differentiation and the lexicon*

The idea that a variety should manifest some sufficient minimal difference from a near linguistic neighbor in order to justify its being considered as ‘different’ or ‘new’ is by no means unfamiliar. The idea is embedded in the visual representation of the *Stammbaum*, where daughter languages branch down from a parent language and are found equidistant from each other, from the parent language and on the same

horizontal axis. Although the *Stammbaum* is not meant to depict the degree to which two sister languages are systemically similar or different from each other or the parent language, the visual representation may perhaps lead unintentionally but understandably to the idealization that to be distinguished at all each must be sufficiently different from each other and/or their source. Furthermore, SIL linguists, appear to require a minimal threshold of difference in order to approve a petition for a unique ISO-639-3 code to be assigned it in the Ethnologue database.¹⁶ In other words, the concept of sufficient minimal difference is one with appeal among language professionals.

Nonetheless, sufficient minimal difference is unsustainable as a language-naming criterion. If taken seriously, it would mean either that (a) where two varieties differed by sufficient amounts, new names would be merited, or else (b) where a new language variety is brought to light, unique or new names for it could only be supported if the variety in question differed by a sufficient amount from some other variety. It is easy enough to see that version (a) is nowhere supported in linguistic practice and I will argue that version (b) should not be supported.

The implementation of version (a) of sufficient minimal difference would mean, for instance, reclassifying speech ways like Mandarin and Cantonese as something other than Chinese, since these two varieties are so different lexically as to not be mutually intelligible. Likewise, the vast majority of content lexicon of Cairene Arabic (a.k.a. Aamiyya) is so different from Modern Standard Arabic, and certainly from classical Arabic (a.k.a. FusHa) in the naming of everyday objects, so as to also be unintelligible without explicit education in the latter forms. Similarly, English, as is well known, employs a lexicon estimated to be 60 percent French and/or Latinate in origin (Finkenstaedt & Wolff, 1973), much of it acquired in the centuries following the arrival of French Normans in England (Pyles & Algeo, 1993: pp. 295–299).

16. That a minimal difference is required is noted, for example, in the rationales provided for rejecting a requester's petition (a.k.a. a change request) to create a new language code. This happened, for example, with respect to a request to create a new language code for Masurian, to which the Registration Authority (RA) replied: "There is anecdotal evidence ... but no evidence along the lines of which the RA usually looks for (e.g., non-intelligibility or low percentage of lexical cognates) to show that Masurian is different enough from its surrounding languages. For these reasons, the request to create the code [zrm] 'Masurian' is rejected" (http://www-01.sil.org/iso639-3/cr_files/PastComments/CR_Comments_2016-038.pdf). In other places, SIL linguists note that "[lexical similarity] percentages higher than 85% usually indicate a speech variant that is likely a dialect of the language with which it is being compared" (<https://www.ethnologue.com/about/language-info>). Executive Editor of Ethnologue, Dr. Gary Simons, has communicated that while mutual intelligibility cut-offs are not strict, there seems to be a consensus that approximately an 80+ percent cognate similarity suggests that individuals are speaking the same language, while less suggests two separate languages (G. Simons, personal communication, December, 2017).

Said differently, the lexicon of today's English is about 60 percent different from its lexicon prior to 1066. Nonetheless, it continues to be called *English*. Considering the great proportion of its lexicon that is no longer shared in common with its pre-Norman version, a consistent application of version (a) of minimal sufficient difference would require that what is today called *English* should in fact be called something else. It should, given the lexical evidence, at the very least accept as reasonable a proposition for renaming today's English: perhaps *Norman English* or *French English*.¹⁷ Nonetheless, linguists have, in my own experience, unproblematically accepted nomenclaturic equivalencies between the varieties in question, despite the great proportion of lexical differentiation between the two linguistic varieties being compared.

An implementation of sufficient minimal difference only in the case of newly proposed languages or varieties (version b) is, by far, more strange than the naming scenarios proposed above. It would most certainly mean holding speakers of newly proposed languages to more rigorous standards for establishing glottomastic autonomy than speakers of language varieties that have already been recognized in the linguistic cannon or political arena. For instance, it is almost certain that those who share the intuitively appealing contention that the 'bulk' of any one linguistic domain must be differentiated would surely not ratify nomenclaturic independence for any variety that shared 90 percent or more of its lexicon with another variety. Estimates of differentiation and overlap in the lexical inventory of New York City Spanish speakers, for example, show that on average about three percent of the words of each individual's interview are of English origin.¹⁸ That is, given that about 97 percent of words used are part of some variety of Spanish, glottonymic differentiation would *not* be justified since a minimum threshold of difference is not attested. Nonetheless, a similarly small amount of linguistic differentiation accompanies glottomastic autonomy among speech varieties in Northern Europe.

17. On the other hand, one might argue that no special differentiating moniker is needed because the vast majority of the core vocabulary – the most frequently used lexical items were and have remained Germanic in origin. By the logic of version (b) of the criterion of sufficient minimal difference, then, one might have to ask why today's English is not instead called *Angle's German* or *North American German*. The reason is, of course, that the extent of lexical overlap, however much one may wish it to be a criterion for language naming, simply factors little or not at all into actual glottonymic practice.

18. Otheguy and Stern cite Varra (2007) in providing an estimate of less than one percent of lexicon being of English origin (Otheguy & Stern, 2010: pp. 90–91). This figure is based on word counts (i.e., tokens) in discourse. An inventory count (i.e., types) of a subsample of the OZC shows that English represents at least three percent of each speaker's lexical inventory. The estimated proportion of English words in the lexical inventory used by each individual informant during their interview ranges from 1/3 of a percent to 12 percent.

The Scandinavian languages are known to be similar lexically. Nynorsk and Danish, for instance, overlap considerably in the lexicon (not to mention the syntax) and are mutually intelligible (Wardhaugh, 2000: p. 27). One estimate suggests that Nynorsk and Danish share approximately 97 percent of their lexicon.¹⁹ Nonetheless, those living in Kristiansand, Norway are said to speak a different language than those in Hirtshals, Denmark. In Otheguy's view though, such a small quantity of lexical difference between duolingual practices of U.S. Latinos and standard Spanish would be insufficient for distinguishing the former from the latter.

Clearly, if objective accuracy is, as Otheguy and Stern suggest (Otheguy & Stern, 2010: p. 87), of central importance to linguistic nomenclature, then criteria must be impartially applied across sociolinguistic scenarios. Given the aforementioned, the application of a sufficient minimal difference criterion would, in practical terms, require either renaming varieties that are systemically too similar (e.g., Nynorsk and Danish) in order to communicate their systemic similarity (i.e., with variations of the 'same' name, as for example *Norwegian Scandinavian* and *Danish Scandinavian*) or accepting the lowest common difference among linguistic varieties as the minimal sufficient difference. Granting that a difference in lexicon of three percent has been sufficient for distinguishing Nynorsk and Danish, one would thereby have to conclude that it is also sufficient for distinguishing duolingual discourse from standard Spanish. Not implementing an equitable application of the criterion directly undermines its intuitive appeal and the apparent reasonableness of the criterion itself. In other words, the extant *raison d'être* for the criterion – the detached application of objective linguistic analysis – dissolves if the criterion is not consistently applied across sociolinguistic contexts.

3.2.2 *Glottonymic differentiation and syntax*

It is fairly easy to find counterexamples to the suggestion that anything like a criterion of sufficient minimal differentiation is at stake in assessing acceptable instances of glottonymic differentiation. To use a previous example, Classical Arabic and Cairene Arabic differ substantially in syntactic features,²⁰ and are yet considered by Arabic scholars to be of the same system and both are called *Arabic*. Modern English syntax lacks case endings for the most part and has a very different word order than what is called *Old English*, and yet the two systems are still called *English*. Thus, we see that, in practice, linguistic varieties can vary in the majority of their

19. See <http://www.ezglot.com/most-similar-languages.php>.

20. For example, even where both Cairene Arabic, on the one hand, and Classical and Modern Standard Arabic, on the other, make provisions for similar grammatical features, like a three-tense verbal paradigm, their allomorphological realizations are very different (e.g., 'he will go' in Cairene is *howwa Hay-roaH* vs. *howwa sa-yadahab* in Classical and Modern Standard).

features and be considered as part of the same system and partake in similar names. Likewise, two varieties can show largely similar syntactic systems but be considered distinct languages. Such is the case with Hindi and Urdu, as well as Nynorsk and Danish.

While the implementation of a criterion of sufficient minimal difference with respect to lexicon would at best be inconsistent, its implementation with respect to syntax fundamentally contradicts our current knowledge about the nature of language and linguistic systems. The criterion depends, it seems, on the position that language change happens abruptly and results in obvious differences from parent languages. Yet, linguistic study has shown that, as a rule, structural changes (particularly, syntactic ones) tend to enter a language slowly and very often indirectly through pragmatic shift (as opposed to direct importation of novel structures) (see Bybee, 2015; Silva-Corvalán, 2002).²¹ Cases of sufficient minimal syntactic change between generations (dismissing for the moment what that quantity would be or how one might calculate it) would be extremely unlikely under circumstances of normal language transmission.²² In other words, if a criterion of sufficient minimal difference were to be objectively applied to glottonymic practice, virtually no normally transmitted language would ever again merit a novel moniker; the degree of difference that a variety manifests as measured from a linguistically near neighbor (e.g., the parent generation) would always be small or insufficient. The only time a speech variety worthy of a distinct name could arise, barring the discovery of heretofore unknown peoples, would be under conditions of interrupted language transmission, such as would be the case for pidgins, creoles, or bilingual mixed languages.

21. One exception to the tendency of gradual linguistic change happens with respect to certain situations associated with the emergence of bilingual mixed languages of a particular type. In one particular sociolinguistic configuration (the need to create or maintain a group-identity in response to factors that would otherwise lead to complete language loss), a group creatively combines elements from the subdomains of the languages in the local context, often within a single generation (Thomason, 1995).

22. Thomason (1997: p. 74) defines *normal language transmission* as the passing of a multi-domained linguistic system (e.g., lexicon, syntax, phonology) from parent generation to child generation more or less intact, without serious change or alteration to any one or more of the domains.

3.3 The uniqueness criterion: Must linguistic features or processes characterizing a speech way be unique to merit glottonymic differentiation?

Otheguy and Stern, as above outlined, argue that the duolingual discourse of U.S. Latinos does not merit a novel name because processes and features that characterize it in contradistinction to standard Spanish are “not unusual” (Otheguy & Stern, 2010: p. 89) and “parallel” (Otheguy & Stern, 2010: p. 88) those in other linguistic varieties. This line of argumentation suggests that a different name for a linguistic variety *could* (or should) be justified if it manifests linguistic processes that are novel or that do not occur in similar varieties. In other words, what would suffice as sufficient for coining a new language name is the occurrence of features not found elsewhere in varieties related to the variety under consideration. But such a requirement is unattainable for any language variety, regardless of what it is being compared to.

That uniqueness is an unreasonable requirement for linguistic differentiation is supported by modern linguistic science. What linguists and lay people alike consider different languages can, in fact, have many features in common or encode similar meanings using similar mechanisms. Spanish and Italian, despite being considered distinct languages, both encode tense, aspect, person, and number as verbal affixes. Similarly, Spanish and English encode number on nouns. Indeed, as Greenberg (1963) helped establish and Baker (2001: p. 31) reminds: similarities between human languages seem to emerge “from general properties of human cognition.” If this is the case, then the existence of a uniqueness condition for glottonymic differentiation is an inherently self-defeating requirement. For, if languages emerge from general and shared properties of cognition, then all linguistic systems will inevitably share some structural similarities with at least some other systems. Furthermore, it seems unlikely that there would exist any feature or process in any linguistic variety that does not appear as similar to something in another variety. Expecting any variety to demonstrate this type of uniqueness, it seems, is to expect language users to not be human.

Let’s consider the untenability of a uniqueness requirement from another perspective: that of change and innovation in the lexicon, a domain where, for reasons given above, innovation may be more likely to occur. While innovation occurs constantly, truly novel innovation is fairly rare. Consider the mechanisms associated with innovation in the lexicon: *compounding*, *affixation*, *shortenings* (e.g., acronyms), *blendings* (e.g., portmanteaus), *functional shift* (e.g., the noun *google* being used as a verb), *borrowing* and *new word creation* (Pyles & Algeo, 1993, Chapter 11). Of these, it will be noted that only that last one – that of *creation* – results in linguistic material that is truly novel, which is not already present in the variety itself or some other variety. Yet, creation seems to account for proportionally

fewer lexical innovations than any single other process. For instance, at the end of the 20th century, novel creation in English had been estimated to represent less than half a percent of new words in the language. Meanwhile, compounding accounted for about 40 percent and borrowing another two percent (Pyles & Algeo, 1993: p. 285, citing Algeo, 1991: p. 14).

Assuming the above statistics represent, with some degree of variation, tendencies in other varieties, a uniqueness criterion for glottonymic differentiation could not result in the successful identification of a distinct language variety. Assuming that the criterion will not be applied in such a way as to qualify *any* variety with a newly created word as 'distinct' (leading as that would to a proliferation of differently named language varieties), then it would be necessary to require that a speech way demonstrates a sufficient amount of uniqueness to merit a new name. But as those statistics for English indicate, the appearance of truly novel language material in a language variety is very infrequent (representing fractions of a percent of just the *new* words in a variety). Applying the criterion on the level of process (rather than to particular linguistic features), we return to Otheguy's original line of argumentation as used with respect to borrowing: since creation (like borrowing) is a linguistic phenomenon that occurs in other languages and varieties, it cannot qualify a variety as distinct from others.

The Uniqueness Criterion, that a variety must manifest unique linguistic features or processes in order for it to merit a new name, then, is both linguistically and practically unfeasible. It imposes impossible-to-satisfy conditions that overlook the nature of language change and innovation: gradual processes in which speakers tend overwhelmingly to reproduce wholly or in part that which is already known to them.

This section has provided data and argumentation to show that the criteria that Otheguy (along with Stern) utilizes to deny duolingual discourse glottonymic autonomy are either (a) not in fact part of linguistic glottonymic practice in any consistent way (i.e., they are glottonymically irrelevant), (b) implementationally unfeasible or inconsistent, or (c) linguistically untenable. Given the significant doubt shed upon the objective application of these criteria, any conclusions reached as a result of their application in argumentation are rendered uncertain.

In particular, the conclusions that Spanglish 'does not exist,' that the term *Spanglish* refers to nothing, or that Spanglish is not a language stand once more in need of verification or rejection. Indeed, having cast significant doubt upon the criteria that served as the basis of glottonymic differentiation for Otheguy and Stern now raises questions like: If not these criteria, on what basis are linguistic varieties considered to be languages? On what bases are glottonymic differentiations accepted? And, more to the point: How does so-called Spanglish measure up? These issues are taken up further in Section 5. Before taking up that discussion, I touch here briefly on other problematic aspects of the Otheguy–Stern analysis.

4. On the implementation of linguistic analytical methods, the application of scientific principles and necessary conditions for naming

The previous section demonstrated that the criteria that Otheguy and Stern would have a reader take to be the heart of linguistic differentiation and naming are everywhere in linguistic practice violated or else not implementable with respect to linguistic configurations of named varieties. This section touches upon the dubious utilization of scientific principles and philosophical stances that undergird some of the secondary, but pivotal, arguments Otheguy employs to disqualify *Spanglish* as a term to name duolingual practices. These have to do with the execution of the analysis, problematic applications of scientific axioms and difficulties concerning the necessary conditions for naming and its relationship to reality.

4.1 Atomistic analysis in defining linguistic varieties: A problematic implementation of analytical methods

One issue that arises in Otheguy's analysis is the presentation of a methodologically problematic analysis as linguistic doxa to students of language. Otheguy, in atomistic fashion, isolates and examines individual linguistic phenomena in each subdomain of duolingual discourse. While analytically and presentationally acceptable, even necessary, the atomistic nature of the overall evaluation does not represent how linguistic science in the domain of language contact and sociolinguistics proceeds.²³ It is a long-established practice of sociolinguistics, for instance, that linguistic varieties are not distinguishable from one another on the basis of a single 'unique' feature. Rather, it is the unique *combination of features* that distinguishes one variety from another. Wardhaugh, author of *Introduction to Sociolinguistics*, for example, articulates this in a discussion of the difficulties associated with defining language. He says:

... if we can identify (...) *a unique set of items or patterns* for each group in question, it might be possible to say there are such varieties as Standard English, Cockney, lower-class New York City speech, Oxford English, legalese, cocktail party talk, and so on. One important task, then, in sociolinguistics is to determine *if such unique sets of items or patterns do exist*. (Wardhaugh, 2000: p. 21, my emphasis)

23. Similarly, Thomason and Kaufman (1988: p. 61) criticize precisely this type of atomistic argumentation when it comes to proposing sources of language change. She critiques those who would negate the existence of external causation or multiple sources for language change by explaining away individual features of the focal language one at a time, without considering the situation as a whole.

In other words, in order to establish that some speech of some individual *is* a variety for purposes of linguistic analysis, sociolinguists require the identification of a series of features whose geographic or social diffusion overlaps to some extent (i.e., they are recurrently found in the speech of a particular group). It is the set, as a set, that must be unique. It is, then, not any single feature in any single domain of what bilinguals in the U.S. do, but in the joint combination of the linguistic features in their speech that determines if it could be considered a unique linguistic variety. The question of whether such a joint set of exists in the case of duolingual practices and what comprises it is addressed in Section 5. Here, the conclusion is simply that it is not the term *Spanglish* that is “technically flawed” (Otheguy & Stern, 2010: p. 98), but rather the application of atomistic argumentation and evaluation.

4.2 Axioms of science, names and their referents

Otheguy (and Stern)’s conclusion that *Spanglish* is an unsuitable moniker is arrived at with arguments that are based on questionable positions about the scope of operation of scientific axioms and the relationship between words and their referents. These beliefs include the ideas that:

- The morphological composition of names should be maximally transparent with respect to the ontological status of its referent;
- Naming should adhere to principles for the sound construction of scientific theories, in particular that terminological entities should not be posited unnecessarily (à la Ockham).

4.2.1 *Plurality is not to be posited unnecessarily: A problematic application of Ockham’s Razor*

Otheguy offers two additional arguments to support his claim that *Spanglish* is a misnomer. Both appear to rely on the application of Ockham’s Razor (see Boehner, 1990),²⁴ a principle of elegant theory construction, to onomastic behavior. The first is that the term *Spanglish* is inappropriate²⁵ because it gives a proper name to something (i.e., duolingual discourse) for which a proper name already exists

24. Ockham’s Razor is often summarized to undergraduate students of science as “The simplest explanation is the best.” This is, however, an unfortunate misstatement of Ockham’s position. His actual words (“Plurality is not to be posited unnecessarily”) suggest **not** that the simplest explanation is the best, but that the **simplest and most complete** explanation, in which each theoretical construct is posited for empirically justifiable reasons, is the best (see Boehner, 1990: pp. xx–xxi).

25. Otheguy says the term *Spanglish*, together with words like *unicorn* in the biological sciences, “confunden las cosas más simples” ‘confuse the simplest of things’ (Otheguy, 2007: p. 5).

(i.e., Spanish). That is, the Otheguy –Stern argument assumes that concepts or referents should only be named once: in science, linguistics and/or in general. The second argument goes like this: Since English features are not present in the systematically novel syntactic features of duolingual discourse (when compared to standard Spanish), and since Spanglish is a clear portmanteau of the words *Spanish* and *English*, *Spanglish* inaccurately signals a (syntactic) mix of grammar where there is none (with Stern, 2010: p. 86, 92, 93, 94). Their contention would seem to be that the morphological construction of a name should correlate with the ontological status of the referent. Both of these arguments appear to be corollaries of a generally accepted principle for elegant theory construction known as Ockham's Razor: "Plurality is not to be posited unnecessarily" (Boehner, 1990: pp. xx–xxi). In other words, the best explanation is the one that accounts for all the phenomena with the most economical model. In asserting as they do, Otheguy and Stern seem to be applying these axioms of responsible scientific behavior to onomastic practice, which is a domain of human behavior (not theory), as well as to the whole of linguistic science (as opposed to using the axioms as tools for comparing the suitability of competing models). A few brief examples should demonstrate that the Razor cannot apply to the whole of scientific practice, much less to the naming behavior of people in general.

As regards human naming practices, Otheguy's examples of doublets, used by the same individuals even within the same discourse, such as the use of *niño* and *churi* in Montevideo or *beismen* and *sótono* in New York, demonstrate that were a 'plurality' of terms to refer to one and the same referent an impediment, such word pairs would not be nearly as prevalent as they are. Human languages are filled with near synonyms and contact languages with doublets.²⁶ With respect to onomastic practice, where the referent of the name is more obviously intended to be 'the same' across occasions of use, a plurality of forms may also abound. The system of street names in Miami, FL is a clear case of apparently semantically unmotivated proper name proliferation. For instance, Southwest 57th Avenue is also called Red Road. Southwest 62nd Avenue is also called Officer Ephriam Brown Avenue and Paul Tevis Road. While the bestowing and invocation of each name has clear semiotic motivation and each indexes particular historico-cultural realities, the referent in each case remains the same. In other words, referentially unmotivated terminological plurality abounds in human lexicon and onomastic practice.

26. Some contend that common noun doublets tend to specialize over time, so that in fact they do not refer to the exact same thing (e.g., see Otheguy & García, 1988 with respect to Spanish of Miami Cubans). In other words, as in general linguistics, there are no true (i.e., 'exact') synonyms in language.

Even in science, labels for what are in essence the same concept or referent are plentiful. Objects of natural science can be named twice (e.g., Hesperus and Phosphorus). This is also so in linguistic inquiry, where the terminology can shift according to the particular theory at stake. For example, popular usage in linguistics employs the terms *word* and *morpheme*, which approximate what axiomatic functionalists would call a *moneme* (see Mulder, 1993). Likewise, the phenomenon whereby a single word with a historical origin associated with a particular language is used in a discourse whose other lexical elements have a historical association or origin in a different language is variously called a(n) *loan word*, *borrowing*, *intrasentential codeswitch*, *transfer* or *insertion*, depending on the theoretical framework and objectives of the investigation. The point is that what is considered terminologically or conceptually unnecessary ‘plurality’ with respect to evaluating the quality of theoretical models does not and cannot govern science as a whole or human naming behavior in general. These domains, generally speaking, are not subject to principles of elegant theory construction.

4.2.2 *The morphological composition of names should be ontologically transparent: A problematic perspective on the relationship between a name and its referent*

As mentioned, a pivotal element for Otheguy’s argument that *Spanglish* is a misnomer rests on the assumption that there should be a correlation between the morphological composition of a name and the ontological status of its referent. The position, reminiscent of descriptivist theories of naming, seems to signal a misunderstanding of onomastics. In a manner evocative of Donnellan (1966), Otheguy takes the descriptive stance²⁷ further, contending that the term *Spanglish* is mal-suited because it is clearly composed of the words *Spanish* and *English*, both of which refer to languages, and yet its referent (i.e., the duolingual speech ways of U.S. bilinguals) (i) is not an independent language, and (ii) does not demonstrate syntactic admixture.

Examples abound that suggest that morpho-ontological correlation is not a necessary condition of naming practices (however, much we might wish it to be). Baby naming is an easy case in point. One need only mention the existence of children named *April* who were born in June or infants called *Grace* when nothing is

27. Descriptivist theories of proper names posit that the meaning of a proper name is a descriptive phrase. So, for instance, the meaning of *Shakespeare* would be ‘the author of *Romeo and Juliet*,’ or ‘the famed English playwright of the 1500’s.’ Such theories run into problems with respect to accounting for the truth value of statements in which they are contained. These theories appear to have been remedied by Kripke’s (1981) Causal Theory for reference, whereby the meaning of the name is accounted for by the speaker’s intention on a first occasion of utterance.

yet known of their character. On the other hand, and directly to Otheguy's point, portmanteau creations (like *Spanglish*) do frequently have referents whose ontological status matches the referents of its compositional parts.²⁸ For example, the term *hibernosh* is comprised of the terms *hibernate* and *gnosh*. The resulting term, likewise, refers to an activity. Similarly, the referent of *spork* shares its ontological and functional purpose with the referents of its component elements, *spoon* and *fork*. Nonetheless, such correlations are by no means necessary for referential success. Portmanteaus exist for which the referent does not follow the extant pattern. A *pornado*, for instance, is the *activity of viewing* pornography in a mindless or out of control manner, not the pornography itself, nor a weather event. *Flavorite* (> *flavor* + *favorite*) refers to a type of taste (i.e., *flavor*), not a type of favorite.²⁹

That naming is not governed by semantic nor morphological transparency is also apparent in the sciences, linguistics included. For example, take the term *percolation* in government and binding theory (Haegeman, 1994). There is nothing transparent about using the term for a chemical–physical process to explain the arrangement of syntactic elements in a sentence. In fact, the very objective of science precludes terminological consistency or transparency. With new discoveries, our understanding of previously identified and named phenomena is modified. For example, in the 1800s, the word *atom* was given to the tiny molecules thought to make up matter, an idea from the Greeks for whom the word *atom* means ‘indivisible.’ These tiny components were so named because they were thought to be the absolute smallest components of physical existence and, in that way, ‘indivisible’ into anything smaller. Yet, nearing the turn of the century, it was discovered that even smaller elements existed (e.g., electrons). The term *atom* in this respect could now be considered a misnomer, since atoms are clearly not the smallest components of matter and, as is now widely known, are able to be divided under particular conditions.

28. A glance at the portmanteau words listed on *Wikipedia*'s “List of Portmantueas” page (https://en.wikipedia.org/wiki/List_of_portmanteaus) suggests that in the majority of cases, the ontological status of the referent matches either the second element (e.g., a *murse* (< *man* + *purse*) is a type of bag for carrying things, not a type of human being) or both elements when they are ontologically similar (e.g., a *frenemy* (< *friend* + *enemy*) is a person with whom a relationship manifests both friendly and adversarial characteristics).

29. It might be tempting to believe that the head element of this phrase is actually *favorite* and thus that it denotes a type of ‘favorite.’ To see that this is not the case, however, consider appropriate answers to the question: “What’s your favorite?” If it were about types of “favorites,” the answer could be “red shoes,” the TV show “Scandal,” or your hobby “scuba diving.” However, such is not the case. Appropriate responses to “What’s your favorite?” would be: “strawberry,” “key lime,” or “grape,” thus showing that the term refers to a type of flavor.

Nonetheless, one considers whether striving for a terminologically unredundant system is not without merit. Otheguy points out that:

In the world of science [biological science, environmental biology and linguistics], and of real life problems, words like “unicorn” are counterproductive because they confuse the simplest of things and weaken direct comprehension of the things that we want to understand, obliging us to be alert, so as not to become convinced that something real exists just because a word for it exists.

(Otheguy, 2007: p. 5, my translation)

While the argument seems applicable in the biological sciences, I suspect the situation is not so immediately urgent in the construction of mental and psychological models. Terminological proliferation in the construction of linguistic models, given the multidimensional phenomenon that language is, does not seem to have made us worse for the wear. There seem to be mechanisms that control for the unwieldy and unproductive proliferation of terminology, human working memory being one of them.

Returning to the point, given that naming in linguistics and in general seems not to be subject to conditions of morphological, semantic, or ontological transparency in order to perform its primary function of indexing and referencing, there seems to be no reason to require the term *Spanglish* to live up to this standard. If speakers had decided on the term *Radish* or *Faddish*, it would present no more or less difficulty to endeavors to understand and characterize the phenomenon to which it refers.

5. On duolingual discourse of the Latino community and Spanglish: What now?

Having cast doubt on the arguments and criteria used to reject the linguistic and glottonymic independence of duolingual practices, the question still remains: What is the status of the duolingual productions of bilingual Latinos in the U.S. and their Spanish-speaking community? Do they constitute an independent linguistic variety? A language? Does Spanglish exist? What if anything does it refer to? Discussion begins with the two questions on which others are contingent: *Can duolingual discourse be considered a linguistic variety in its own right?* and *What is the ontological status of linguistic entities?*

5.1 Do duolingual practices constitute a linguistic variety?

In sociolinguistic terms, linguistic varieties are speech patterns and practices associated with a group of speakers (Wardhaugh, 2000). According to scholars, like Wardhaugh, such a definition allows for a linguistic variety to refer to a set of behaviors that are more circumscribed than what would, in every day terms, be called a dialect (Wardhaugh, 2000: p. 21). Additionally, linguistic varieties can also subsume verbal repertoires that are, in every day terms, thought to be constituted by different languages. In theory, all that is required is a “specific set of ‘linguistic items’ (...), sounds, words, grammatical features, (...) which we can uniquely associate with some external factor” (Wardhaugh, 2000: p. 21).

Defining the duolingual practices of U.S. Latinos as a linguistic variety, then, requires correlating (a) a uniquely defining set of linguistic features to (b) an external factor. We might posit here that the external factor uniting individuals in the U.S. is a home life and/or upbringing dominated by the language practices of Spanish-speakers, usually as a result of Spanish-speaking parentage or grandparentage. A set of uniquely defining features might include (among others):³⁰

- morphosyntactic frames associated with standard Spanish syntax and lexicon;³¹
- a verbal paradigm including three (rather than four) forms for speaking about the past;
- a verbal paradigm including present subjunctive forms but few or crystalized instances of past subjunctive forms;
- fluidity in prepositional content (e.g., *Me fui en/por la mañana / Me enamoré con/de él.*);
- lack of simultaneity in gerund use (e.g., *Corriendo* es buen ejercicio.);
- frequent use of subject personal pronouns and with different ordering of some of the constraints that govern overt pronoun use (e.g., *yo le dije ... y entonces yo le di ...*; Otheguy & Zentella, 2012);
- occasional preposition stranding (e.g., *la persona que lo hizo por ...* ‘the person she did it for ...’);

30. With few exceptions, the majority of these features are the features that Otheguy himself enumerates.

31. Since, ultimately, the goal of Section 5 is to arrive at a conclusion about the social and linguistic status of that which is called *Spanglish*, I begin with specifying an etymologically Spanish morphosyntactic frame. This is because my observations suggest that practices that include a morphosyntactic frame associated with English (e.g., *so then I was like ‘pa’ que ‘?’ why did he do that?’*), while they are also technically ‘duolingual’, would not be considered by Latinos to be examples of Spanglish.

- lack of a grammatical marker for animate indirect objects (i.e., omission of personal ‘a’);
- the presence of English-origin words and phrases with and without morpho-syntactic adjustments to form.

In this way, then, the duolingual practices of U.S. Latinos can, as with other linguistic varieties, be satisfactorily defined in theoretical terms as a linguistic variety, even if not every feature is found in the speech of every individual for whom the external factor applies. For the sake of argument and for expositional simplicity in the remainder of this discussion, let’s call this variety *Spanglish*.

5.2 Can Spanglish be called a ‘language’?

In this chapter, as well as in Otheguy and Stern (2010), the term *language* has been employed with respect to two constructs. It has been talked about as a sociolinguistic phenomenon (e.g., when referring to what lay people call ‘language’) as well as a mental object (e.g., when referring to ‘language’ as an ordered, symbolic system of meaningful oppositions). This section examines the extent to which duolingual practices (as partially defined in linguistic terms in Section 5.1) meet either or both conceptualizations of *language*. Yet, given the context in which this examination occurs – a debate about the existence of Spanglish and the suitability of that moniker for what bilingual Latino communities do when they talk – the reader can be fairly certain that duolingual practices will not end up as a prototypical example or the metonymic reference point for category membership (Lakoff, 1990, summarizing Eleanor Rosch’s work) for conceptualizations of ‘language’ examined here.

5.2.1 *Spanglish as mental object and the ontological status of linguistic entities*

Language, on a Saussurean view, is a meaningful system of signs in opposition. It is a mental object.³² Most linguists would concede that evidence of this (or any) mental system cannot be apprehended directly. There is no way to touch, measure, taste, or see language as a mental object. Rather, its existence, as in the case of other

32. It may occur to some readers at this point that the foregoing critique of Otheguy makes it impossible to use Saussurean (i.e., mentalistic) concepts to argue that Spanglish in fact stands on similar ontological grounds as that which is called *Spanish*. In fact, however, this is precisely the purpose of this chapter: to use the same linguistic theoretical constructs as Otheguy to build the counterargument. Furthermore, the critique contained in this chapter regards the domain of application of his criteria and some of its glottomastic or philosophical underpinnings; the critique is not taking aim at the conceptualization of language that Otheguy espouses *per se*.

mental objects, is ascertained indirectly. In the case of language, this occurs via (i) detailed observation of human behavior, and (ii) analysis and consecutive abstraction of that behavior. For instance, when human beings speak, a series of sounds emerges.³³ These sounds are not the system. The nature of these acoustic signals is that of a largely continuous flow with occasional stops and pauses. Furthermore, there is no single part of the acoustic flow that replicates *exactly* any other part of it. That is, each segment of the sound flow, however determined, is acoustically unique from every other segment.

Humans, nonetheless, perceive regularities in the flow. Segments, despite their acoustic uniqueness, are perceived as ‘the same.’ This perception of acoustic or articulatory similarity is an abstraction from the physical stream of speech that seems to result from consistent occurrence of the phenomenon under some perceived repetitive exterior condition. The similarity emerges as a function of selecting characteristics of the acoustic signal, of innumerable possibilities, to which one will attend. For example, perceiving the sound [b] in the acoustic signal is a matter of attuning to physical conditions of its articulation – the fact that two lips come together and that the vocal cords vibrate – rather than its speed or volume of utterance. In turn, sequences of sounds (such as [b] – [a] – [k] – [a]) under repeated circumstances (such as the presence of a four-legged animal of particular anatomical composition) lead to positing the existence of words (such as *vaca* ‘cow’ in Spanish). Or, the continued use of the sequence [k] – [o] – [m] – [e]/[i] leads to the positing of the existence of a word or morpheme [kom-] that means ‘eat.’ And finally, it is the reoccurrence of these patterns in combination, such as *la vaca comía*, and the lack of occurrence of other purely logical combinatorial possibilities (e.g., *vaca comía la*) that lead to the positing of the existence of rules called syntax.

Neither the phone, nor the morpheme, nor the word, nor the rule is directly observable. Their reality is, as mentioned, posited first by abstraction from the sound flow, and, second, by the high rate of consistency in occurrence of perceived sound segments, morphemes, or word order patterns under particular communicative conditions. That is, no element of that which pertains to ‘language’ is directly observable, but only inferable from patterns in human behavior.

All this suggests a means for answering one version of the question “Can Spanglish be called a language?”³⁴ If linguistic entities cannot be apprehended

33. While for expository simplicity I refer to spoken languages, the same principles, of course, apply to the physical gestures of signed communication systems.

34. The discussion also leads to a conclusion that permits a direct address to the question of whether Spanglish exists. Otheguy suggests that Spanglish is a fiction; like the word *unicorn*, it “carecía[n] de referente” ‘lack[s] a (real-world) referent’ (Otheguy, 2007: p. 5). But if what has been elaborated in this chapter is accurate, it must probably be conceded that Otheguy’s position

directly, but only inferred through abstraction and regularity in human behavior, establishing Spanglish as a language requires only a discovery of recurrent patterns of behavior (i.e., behavioral epiphenomena) under particular communicative conditions. Do people behave in ways that suggest the mental realness of Spanglish? Do behavioral epiphenomena suggest that Spanglish has procured mental status as a language? They do.

5.2.2 *The behavioral epiphenomena of Spanglish*

In establishing the existence of language as mental object, I distinguish between two types of behavioral epiphenomena, which I posit correspond to two classes of mental existence. The first set of behaviors consists of, as discussed above, regular patterning in the acoustic signal under certain communicative conditions, which suggests an internal systemic organization of the acoustic signals themselves. (This behavior corresponds, generically, to the existence of a language in the sense of *langue* or *competence*.) A second type of existence that mental objects can have is existence as perceived objective (i.e., outside the perceiver) reality, as would be the case, for example, for a perceived threat in one's environment. When something is perceived as real, behavioral epiphenomena (e.g., like sweating, rapid heart rate, running, frowning, or crying) suggest the *mental* realness of the cause of the behavior, regardless of whether there is in fact anything in the physical or material world to which it corresponds. In terms of language, this type of mental realness is exemplified in lay conceptualizations and sociolinguistic experiences of *language*. The behavioral epiphenomena signaling this type of existence consists of behaving 'as if' something is real, whether it corresponds to a systematic and regular patterning in the acoustic signal or not.

The second set of behaviors signals a meta-cognition about the acoustic signals that usher from people's mouths. It is the perception that these signals constitute a cohesive entity and in perceiving things in this way, the perception results in tangible and concrete consequences for human decisions and actions. One behavior that both signals the meta-cognition as well as contributes to the mental substance

that *Spanglish* does not exist is due, not to the fact that it lacks a real-world referent, but because it is the natural extension of the more general position that languages do not exist. In other words, linguistic entities, in the first place, do not exist materially like actual dogs, horses, or trees. And, if a parallel to the physical world must be established, languages and linguistic entities exist, at most, only as theoretical constructs. They exist the way Pluto existed prior to its discovery in 1930: as a theoretical entity posited to explain (ir)regularities in the movements of the observable world. In this way then, not only does Spanglish not exist, but neither does any so-called variety or language. No where can one point, in the case of linguistic entities, to be able to apprehend directly the existence of mental systems, whether they be called *Spanish*, *English*, *Spanglish*, or by any other name.

of that cognition is the act of naming. That is, accepting and calling a perception by a name is evidence of perceptual distinction. Naming, in turn, serves to further establish the named *as* mental object, and in contradistinction to other objects. In the case of duolingual speech such as those enumerated in Section 5.1, these practices (or parts thereof) have variously been called *heritage Spanish*, *U.S. Spanish* and, yes, *Spanglish*. In being picked out and named, they take on reality as a mental object.³⁵

In attaining a degree of psychological reality, mental objects give rise to behaviors that both confirm and iteratively re-establish their existence. For instance, the perception that one speaks Spanglish, may lead to conversational exchanges in which perceived elements of Spanglish are ‘translated’ using different words for a listener.³⁶ In the educational context, it may result in policies meant to circumscribe its occurrence – as when an instructor reduces points on an essay for not using a past subjunctive form in what in standard Spanish would be a context for past subjunctive. It can give rise to labeling others as ‘smart/educated’ or ‘not so educated,’ which in turn very concretely affects behavior, such as whether or not one decides to respond to an after-hours email from that ‘uneducated’ person or to volunteer to help them on a project.

As concerns patterning in acoustic signals (the first type of behaviors that evidences the mental existence of language), it has been established in Section 5.1, and in accord with the features that Otheguy himself enumerates, that the phenomena associated with Spanglish (lexical expansion and semantic shift, expansion of verb forms to replacement of others, preposition stranding, overt use of subject personal pronouns and loss of personal ‘a,’ to name but a few) are regular, if variable, features of this verbal repertoire taken as a whole and abstracted over individuals.

In sum, then, the duolingual practices sometimes called *Spanglish*, as a whole, are sufficient enough when taken together to establish these practices as (i) having psychological reality as well as (ii) meeting technical, mentalistic definitions of ‘language.’ It remains to us, to consider the extent to which these practices manifest ‘linguageness’ in a sociolinguistic sense.

35. In addition, Zentella (2016: p. 30) indicates that a large percentage (70+ percent) of individuals in a convenience sample have indicated through nomenclature that what they do is called *Spanglish*, and not *Spanish* or *English per se*, although certainly they feel that those options also exist to them.

36. For example, Varra (in press) found that speakers with more diverse exposure to Spanishes were more likely (than those with less exposure) to use restatements around English-origin expressions while speaking Spanish. They might say, for example, “Lo compré en un *pet store*, una tienda de animales,” restating that part which they perceive could result in communicative obstacles with their interlocutor. (The example is from the Otheguy-Zentella Corpus of Spanish in New York City. An NSF-funded project, grant number 0004133.)

5.2.3 *Spanglish as sociolinguistic language*

Bell (1976: pp. 147–157, cited in Wardhaugh, 2000: p. 29) offers seven criteria by which to classify languages of different types. In addition, these appear to function as a rubric to approximate the degree to which a speech way garners socio-political acceptance as a language. Of note is the fact that sociolinguistic establishment of a language relies little, if at all, on the linguistic features of the variety in question. The characteristics which Bell enumerates include: *standardization*, *de facto norms*, *mixture*, *reduction*, *autonomy*, *historicity*, and *vitality*. While not currently in a position to provide data on how speakers would rank so-called Spanglish with respect to these, I address, very impressionistically, how I believe speakers of so-called Spanglish (of the second generation) might rank duolingual practices. Of course, as my observations are limited to the places where I have lived and interactions with acquaintances and students, these would stand in need of verification with systematic data; it is likely that the ratings of Spanglish on any one of these measures would vary by community and region, among other factors.

Standardization refers to the degree to which a speech way is codified and includes some agreement about what linguistic forms are part of the language and which are not. *De facto norms* refer to the idea that there are right and less right (or wrong) ways to ‘do’ a linguistic variety. Spanglish, clearly, does not manifest standardization independent of the norms that govern English or Spanish. Nonetheless, some English-origin lexical items seem, in academic circles at least, to enjoy a degree of orthographic standardization. Words like *beismen*, *sóbbuey*, *quora*, and *bildin* are some English-origin loan words that appear in the work of scholars with consistent orthographic renderings. With respect to *de facto norms*, my impression from teaching and speaking with heritage students is that, although many say that they do not speak Spanish correctly, they also have a fairly well-developed sense about what is considered ‘acceptable’ and ‘authentic’ or ‘deviant.’ For instance, when asked if *corriendo es bueno para la salud* sounded good to them, about 80 percent of students (in a 200-level heritage class) responded that it was.

Mixture refers to whether speakers feel that what they speak is pure. Wardhaugh mentions that the importance of ‘purity’ and mixture for the purposes of defining a language may be more or less central for different groups. When these concepts are important, however, purist idealizations of speech ways are likely to correlate with a heightened sense of its sociolinguistic ‘languageness.’ It probably goes without saying that for bilingual Latinos in the U.S., sociolinguistic pressure to assimilate, chiding from relatives in home countries and the general accessibility of public education where standard language ideologies are transmitted (among other factors) combine to heighten the sense that duolingual speech ways are highly mixed and/or ‘impure.’ The question of critical import for sociolinguistic inquiry, however, is:

How much does this matter to speakers? This seems to be a matter for extensive empirical probing among various communities, as speakers' articulation of their ideological position and their actual linguistic behaviors often do not align, especially when it comes to stigmatized linguistic behaviors.³⁷ *Reduction* refers to the fact that there may be few(er) opportunities to use the variety in society, in comparison to some other variety.³⁸ As would likely be agreed to by any speaker or observer, Spanglish clearly suffers reduced contexts for use in the U.S., especially as compared to English.

Autonomy refers to the feeling that speakers consider the speech way to be different from other languages. Zentella's (2016) data (a convenience sample) suggests that speakers have begun to elevate Spanglish to a level on par with English or Spanish and view it as distinct from the latter two. *Historicity* means that a group finds a sense of identity through the variety. Whatever identity had previously congealed around Spanglish, the publication of literature that makes use of features associated with Spanglish in recent decades certainly both signals an emergent identity and also helps to establish this identity. Finally, *vitality* refers to whether there exist living speakers, or whether, like Latin, self-professed speakers are either no longer living or are so few or so linguistically isolated as to be irrelevant. Needless to say, there exist those, alive and well, who say they speak Spanglish. An attempt to represent these impressions graphically, in order to gain an overall sense of Spanglish's sociolinguistic 'languagey-ness,' might look like something in Figure 1.

Figure 1 pictorially represents the ratings that I believe Spanglish speakers might give to Spanglish with respect to each of Bell's sociolinguistic language criteria. The indicators are meant to be read as relative to each other as well as, implicitly, to Spanish and English, which would both rank as 'high' on many of these items. Given that just a few decades ago, Spanglish would probably have ranked very low indeed on indicators of autonomy and historicity, the situation might in fact suggest the movement of Spanglish, if very slow, far and improbable, towards sociolinguistic independence.

37. This truism is essentially the reality captured in Labov's (2001: p. 215) notion of *covert prestige*.

38. Wardhaugh (2000: p. 35) says reduction may also refer to the feeling that what one speaks is just a version or sub-variety of something else. However, this understanding of reduction appears to be the complement of *autonomy*, as defined here.

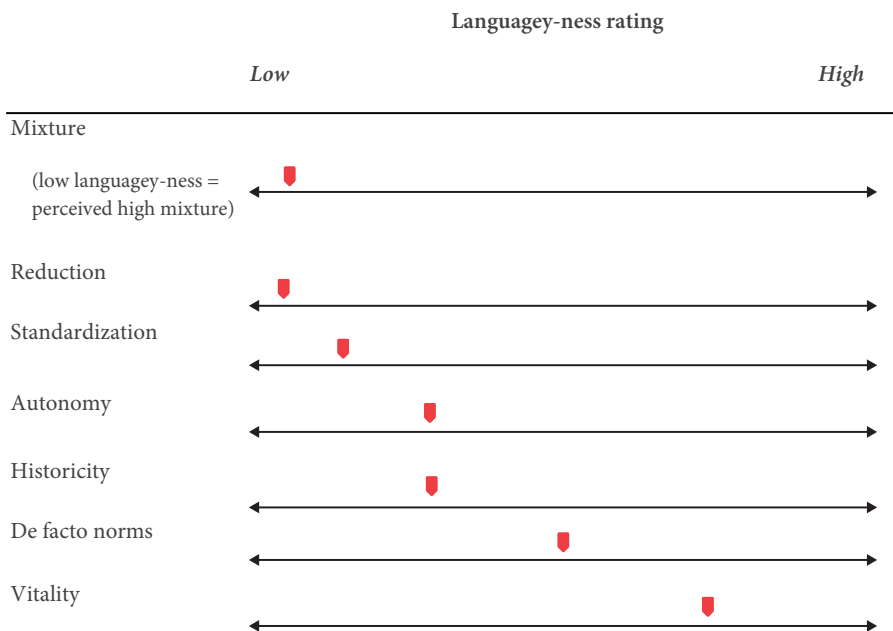


Figure 1. Impressionistic ratings of Spanglish's languagey-ness on sociolinguistic criteria

6. Summary and concluding statements

This chapter has engaged directly with Otheguy on the playing field he has selected – linguistic facts and technical analysis – to argue against his thesis that “Spanglish does not refer to anything [real]” (Otheguy, 2007), which serves as a major premise for his position that the term *Spanglish* should not be used as a technical linguistic term or by language users themselves. The intuitively appealing criteria which he (with co-author Stern) has applied to arrive at his conclusion have been articulated and doubt has been cast on the centrality of their role in linguistic inquiry, particularly as regards determinations of glottonymic autonomy. These criteria included the Criteria of Uniqueness, Systemic Differentiation, and Sufficient Minimal Difference. It has been seen that because these criteria either could not be applied with consistency or else could not result in the linguistic and glottonymic autonomy of *any* linguistic variety, they could not serve, in linguistic science, as objective criteria for determining glottonymic autonomy. This chapter has also highlighted the misapplications of scientific practice and axiom in reaching the ‘Spanglish as misnomer’ conclusion. Drawing out these problematic aspects of his argumentation destabilizes the conclusions reached thereby. Finally,

this chapter has highlighted that the evidence that Otheguy uses to disprove the hypothesis that duolingual discourse is undergirded by a unique linguistic system, when taken together, suggests in fact that they do comprise an emergent and unique (abstracted) system (compared to that of Spanish), no matter how similar these systems may appear.³⁹ My impressionistic evaluation of Spanglish with respect to widely accepted indicators of sociolinguistic ‘languageness,’ additionally, suggests that while low-ranking on these ideological and affective dimensions, duolingual discourse has gained somewhat with respect to autonomy and differentiation, at least since the term *Spanglish* was first employed (also see Zentella, 2016).

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39. My own research in lexical borrowing (Varra, 2018) suggests that, in light of the strong tendencies toward assimilation to English and loss of Spanish in three generations, along with the historically continual replenishment of Spanish-speaking immigrant populations (Roca, 2000), the persistence of these features appears to be due, in historical perspective, to an on-going re-creation of such practices by each new group and its children.

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Bilingual acquisition

Difference or incompleteness?

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In the context of heritage languages, the term *incomplete acquisition* implies that the bilingual child has acquired a language system that is different from that of the providers of language input. The notion of incomplete acquisition has recently been criticized. For example, some scholars argue that grammars cannot be incomplete. This chapter addresses the critiques of the concept of incompleteness and shows, in contrast, that the outcome of reduced exposure and production of a minority language in simultaneous bilingual acquisition indeed reflects the incomplete acquisition of some aspects of the input language. I argue that incompleteness is not a mechanism, but an acquisitional outcome or a stage in language development.

Keywords: incomplete acquisition, bilingual first language acquisition, subject pronouns, tense acquisition, verb clitics, Spanish-English bilingualism

Introduction

Research dealing with the acquisition of heritage languages in contact with a majority language has shown that insufficient exposure to and use of the heritage language contribute to its incomplete acquisition (Benmamoun, Montrul, & Polinsky, 2013; Bolonyai, 2007; Montrul, 2006, 2008; Polinsky, 2006, 2008; Silva-Corvalán, 2014). In the context of heritage languages, the term *incomplete acquisition* implies that the bilingual has acquired a language system that is different from that of the providers of language input (Benmamoun et al., 2013). Stated briefly, a heritage language (HL) is a minority language of indigenous or immigrant speakers who learn it at home in childhood. Heritage speakers (HSs) grow up in a social environment where there is a predominant language and are normally more competent in this language.

The notion of incomplete acquisition has recently been criticized. The argument is either that the concept itself to refer to the language of heritage speakers is

flawed, or that the term is not adequate to describe the grammars of these bilingual speakers. I address the criticisms of the concept of incompleteness put forth by some researchers (e.g., Irizarri van Suchtelen, 2016; Kupisch & Rothman, 2016; Otheguy, 2016; Otheguy & Zentella, 2012; Putnam & Sánchez, 2013) and show, in contrast, that the outcome of reduced exposure and production of a minority language in simultaneous bilingual acquisition indeed reflects the incomplete acquisition of some aspects of the input language. I argue that incompleteness is not a mechanism, but an acquisitional outcome or a stage in language development. Incompleteness involves the lack of aspects, elements, or features in a grammatical domain that are present in the learner's input language (cf., Meisel, 2014). This lack may also be reflected in a marked increase in the rate of use of a construction in the speech of bilinguals compared with its rate in the speech of input providers, or in the use of constructions not used by or not acceptable to the speakers of the input model.

I consider the issue of incompleteness by comparing bilingual children's developing grammars, the more or less changed bilingual systems of adult second-generation immigrants, and the grammars of the input providers. In previous work on English-Spanish bilingualism (Silva-Corvalán, 2014) I have shown that parallels between bilingual children's linguistic behavior and that of adult heritage speakers imply that some aspects of the heritage language of adults are the outcome of an interrupted process of acquisition of this language between the ages of 3;0 and 5;0, when more intensive exposure to English reduces exposure to the heritage language and diminishes the opportunities to use it.

I do not argue that adult heritage speakers have acquired the *entire system* of their heritage *language* incompletely, or that they do not have communicative competence in this language (although some of them don't), but that some heritage speakers evidence reduced or incompletely acquired grammatical domains compared with these domains in the grammars of their adult models in the preceding generation.

At this point, I find it necessary to state explicitly that the high degree of intra-individual and interindividual linguistic variation that characterizes bilingual communities does not make it possible to establish generalizations valid for all members of an immigrant generation. But I hope to contribute a piece of the puzzle in the search for the construction of the complex picture of similarities and differences across generations and the factors that may lead to the various results.

Critique of incomplete acquisition

Quite appropriately, Otheguy (2016: p. 307) affirms that “the claim of incompleteness relies crucially on misapprehensions regarding four highly problematic notions: the *native monolingual control*, the *experimental subject*, the *heritage speaker*, and the *Spanish language*. Once the flaws in these concepts are exposed, the underlying proposal regarding incomplete acquisition loses much of its appeal.” I would add that the answer to the question whether heritage languages are acquired incompletely depends as well on a definition of “heritage language” and on what is understood by “incompletely acquired.” Regrettably, definitions vary and are not always very precise, including my own attempt in the previous section.

The assumption that second-generation bilinguals (HSs) acquire their HL incompletely has been forcefully criticized by Otheguy (2016). He proposes an appealing alternative to the notion of incomplete acquisition, “that of a differently evolved rather than an incomplete grammar” (303). As pointed out by Otheguy, psycholinguistic experiments and data from sociolinguistic studies that have claimed the incomplete acquisition of a heritage language may have suffered from methodological shortcomings. Nevertheless, the fact that more than a few studies employing different methodologies arrive at similar results validates at least some of the outcomes that point to unfinished acquisition of a HL under pressure from a predominant language. Obviously, it is not reasonable to postulate that second-generation HSs acquire an incomplete **grammar**; rather, they acquire some grammatical **aspects** incompletely compared with the same grammatical aspects in the speech of the input providers. This comparison is crucial; it is mistaken to establish incompleteness with respect to “the researcher’s familiar E-language Spanish,” as Otheguy well points out (Otheguy, 2016: p. 313).

Incompleteness is possible, however. Imagine the copy of a geometric figure, say, a triangle. If only two sides of the triangle are copied, it would be difficult to consider it a complete triangle, i.e., the drawing is incomplete; if the drawing is discontinued, the goal of copying a complete triangle will remain unfulfilled. Similarly, children repeatedly exposed to their caretakers’ language develop a system of communication closely similar to the language input they receive. This development is gradual and at first evidences gaps or incompleteness of aspects of the system underlying the input. If the necessary language input is reduced and use of the language is diminished, then these aspects will remain incomplete relative to the input system.

In my view, then, language acquisition is to some extent comparable to the copying of the triangle.¹ Children develop “a language triangle” guided by the

1. I qualify that this is “to some extent” because language acquisition entails much more than just copying the input.

interactions they enter into with the adults who surround them. If these interactions are interrupted in one language or are not sufficiently rich, the language triangle may be left unfinished. Indeed, previous studies have shown that children unconsciously replicate the language of the adults, that children's use of language is highly sensitive to their mothers' and other caretakers' uses to the extent that even the frequencies of some constructions in their speech reflect the frequencies of the adults' use (e.g., verbs, copulas, grammatical structures) (De Villiers, 1985; Naigles & Hoff-Ginsberg, 1998; Silva-Corvalán & Montanari, 2008). "Statistical learning" is part of the children's capacity for language (O'Grady, 2005).

Otheguy (2016) further argues that the gaps identified in the linguistic behavior of second-generation bilingual Latinos are not in fact errors "but rather points of divergence, dialectal differences if you will, between their Spanish and that of the previous generation, due to normal intergenerational language change accelerated by conditions of language contact" (302). Certainly, normal intergenerational language change does occur, but this change occurs slowly, gradually, across several generations, not abruptly as attested in situations of intensive language contact. In a basically monolingual environment, a language change may take many generations to be completed, e.g., the fixing of clitic position in Spanish (Enrique-Arias, 2004); the development of modal auxiliaries in English (Trask, 1996: pp. 156–158); the gradual lexical routinization of the subjunctive (Torres Cacoullas, LaCasse, Johns, & De La Rosa Yacomelo, 2017). Furthermore, not all language features absent in HLs are the result of acceleration of a change already present in the previous generation.

Other authors have also written explicitly against the concept of incomplete acquisition. Kupisch and Rothman (2016) (KR), among others, stated that "simultaneous bilingual acquisition raises serious questions for the construct of *incomplete acquisition*, a term broadly used in heritage language acquisition studies to describe almost any difference heritage speakers display from baseline controls." (1) Their discontent with the notion appears to be based on treatments of incomplete acquisition as a *causal factor* in variable competence outcomes. Correctly, rather than incompleteness, KR consider the quality of the input and the lack of formal education possible causal factors. They miss an important cause, however: reduced exposure to and use of the HL, which have been shown to be crucial in determining the outcome of bilingual development (Silva-Corvalán, 2014; Unsworth, 2016, among others). But contrary to what KP maintain, I will defend that incomplete acquisition is a suitable term to describe a stage in the development of any language. Indeed, a monolingual child may have also reached a stage of incomplete acquisition of a language feature by age 4;0, for instance, the subjunctive in Spanish, which as an adult the monolingual will use without perceptible differences from the use of the preceding generation. KP wrongly view "incomplete acquisition" as

an evaluative label, which the terms “imperfect,” “deficient,” “defective” (also used in the literature) certainly are, but in my opinion “incomplete” should be viewed as the opposite of “completed” or “concluded,” with no value judgment attached to it. Thus, if a speaker does not use Spanish verbs in the preterite when this tense is expected, it is accurate to say that this speaker has not yet completed the acquisition of the constraints on the use of the preterite tense in Spanish.

In their study of preposition stranding (p-stranding) in heritage Spanish, Pascual y Cabo and Gómez Soler (2015) (PG) also criticize conceptualizing incompleteness as a causal factor. P-stranding is not allowed in Spanish not in contact with English (**El amigo que Pedro vino con.* ‘The friend that Pedro came with.’)

To examine p-stranding and pied-piping (The friend with whom Pedro came.), PG (2015) apply comprehension and production tests to three groups: simultaneous, sequential, and late Spanish-English bilinguals. Only this last group categorically rejects p-stranding in Spanish; the other two groups, and especially the simultaneous bilinguals, evidence variability in their acceptance of p-stranding. Without defining complete and incomplete acquisition, PG conclude that this aspect of the Spanish grammar was “completely acquired” by the simultaneous and sequential bilinguals, albeit differently from the late bilinguals in that they “have acquired a grammar that is English-like and that allows for both options, yet has a clear preference for pied-piping” (p. 202). PG appropriately observe that the cause of the different paths of acquisition is not attrition or incomplete acquisition, but the time of exposure to English. They maintain that “HS competence differences [are] the outcome of a process of acquisition that is – in essence – different” (p. 188). But note that time of exposure to English is related to amount of exposure to the HL. Since both languages are in competition for time, longer exposure to English results in less amount of exposure to Spanish. Early bilinguals with longer exposure to English are more vulnerable to influence from this language and thus do not block p-stranding in their Spanish. HS competence differences, then, may result from the fact that the process of acquisition of the relative clause structure provided in the Spanish input has not become sufficiently entrenched to reach complete acquisition and stabilization.

There are valid criticisms, however, of some of the studies exploring the speech of HSs that have relied on the notion of incomplete acquisition. For instance, HSs are frequently compared with “native speakers” and their language is deemed to be “non-native.” These statements overlook the fact that simultaneous bilinguals acquire two (or more) languages as native languages, i.e., from birth. Many of these bilinguals continue to develop the HL at home, in the community, or in school, and attain levels of competence in the HL that are, to the non-specialist, indistinguishable from those of the first immigrant generation. It may be problematic to characterize “heritage speaker” or “second-generation bilingual” as a category

with shared language behavior given that there is much inter- and intra-speaker variation. Indeed, HSs may be arranged along an oral proficiency continuum inasmuch as one can identify a series of lects which range from standard or unrestricted Spanish to an *emblematic* use of Spanish and, vice versa, from unrestricted to emblematic English. The interruption of the process of acquisition of the HL is expected to affect those who are not at the higher end of the continuum. This observation has justified measuring the language proficiency of the subjects included in a number of studies that appeal to the concept of incomplete acquisition. I discuss some of these in what follows.

Benmamoun, Montrul, and Polinsky (2013: p. 129) (BMP) define HSs as “bilingual speakers of an ethnic or immigrant minority language, whose first language often does not reach native-like attainment in adulthood.” Clearly, this definition includes speakers of an immigrant minority language, but leaves outside the realm of heritage language and heritage speakers those who learnt two languages as first languages, i.e., who have two native languages, regardless of how well-developed these languages may be, and also leaves out bilingual individuals whose home language develops “at age appropriate levels.” These may not be the majority of second-generation bilinguals, but they do exist under favorable sociolinguistic conditions. But although BMP acknowledge that HSs vary widely in the degree of their receptive and productive command of the HL, they do not take back the inclusion of the outcome of the process of language development in the definition of HS (Kupisch, 2013).

The first part of BMP’s definition appropriately identifies Arabic speakers as HSs in the US. BMP cite Benmamoun, Albirini, Montrul, and Saadah (2014), who report that these speakers have “incomplete knowledge of the notion of the root,” which is critical in establishing lexical relations in Arabic (and other Semitic languages). “Unlike native speakers, heritage Arabic speakers struggle with word formation processes.” (p. 141) A number of other examples of differences between HLs and their baseline are cited, many of these apparently as the result of attrition, although incomplete acquisition is also considered a cause: “Syntactic knowledge, [...] appears to be more resilient to incomplete acquisition under reduced input conditions than inflectional morphology is.” (p. 148). According to BMP, incomplete acquisition and attrition are some of the factors that play a role in shaping heritage grammars. I will offer evidence that some features of heritage Spanish suggest incomplete acquisition rather than attrition, while others reflect the expansion of changes developing in the input. I concur with BMP that the source of structural differences across first and second immigrant generations requires the careful documentation of the first generation’s language variety to evaluate if this generation, the input providers, has played any role in prompting such differences. Indeed, this is what I have done in the research discussed below.

Albirini, Benmamoun, and Chakrani (2013) (ABC) explore aspects of the grammars of Egyptian and Palestinian heritage speakers in the United States to identify areas of more or less vulnerability within colloquial Arabic heritage speech. They collect data mainly through picture descriptions and elicited narratives. The authors expect that “the heritage speakers have incomplete knowledge of verbal and nominal agreement compared to their monolingual counterparts.” (p. 5) They assign the incomplete knowledge evidenced by the adult bilinguals they study to attrition or fossilization. Incompleteness more likely underlies fossilization since even though agreement morphology emerges early in the grammars of Arabic-speaking children, it remains unstable to after the fourth year of age. This suggests that when Arabic-English bilingual children are becoming dominant in English they have not yet acquired the agreement system of their input providers, i.e., acquisition has not been completed. Without sufficient exposure to and use of Arabic, what ABC find is fossilized or interrupted development of the agreement system in the adults’ Arabic.

Egyptian and Palestinian heritage speakers in the United States are also studied by Albirini and Benmamoun (2014), who focus on plural and dual morphology, possessive constructions, and restrictive relative clauses. Their “findings show that heritage speakers have various gaps in their knowledge” of the areas examined (p. 244). For instance, they surmise that HSs dominant in English before the age of 5 “may not have completely acquired all of the Arabic broken plural forms” (p. 268), i.e., those that are not formed by the process of attaching a plural morpheme to a singular base (e.g., *mudarris* ‘teacher’ → *mudarrisiin* ‘teachers, masculine’), but rather by other more complex strategies, including infixation, reduplication, etc. (e.g., *kitaab* ‘book’ → *kutub* ‘books’). The speakers in their study show notable inaccuracies with regard to broken plural formation. Thus, ABC and Albirini and Benmamoun agree that in conditions of reduced exposure and use, adult Arabic-English bilinguals evidence the incomplete acquisition of some aspects of the grammar of their HL.

Polinsky (2006) sets as her main goals to describe the language of “incomplete learners” of American Russian,² and to identify differences between Full Russian and American Russian, i.e., “a full language and an incompletely learned language” (p. 191). It may be justly argued that this is not an acceptable comparison to substantiate incomplete learning without having shown first that the input to American Russian was indeed a variety of what Polinsky calls Full Russian. Nonetheless, she identified important gaps in American Russian that differentiate this variety from the one spoken by the input providers. These gaps are evident in the use of case

2. The expression “incomplete learners” is not opportune since learners are not incomplete. Rather, a grammatical aspect may be so.

markers, in phonological processes, in gender marking, and in the verbal system (e.g., some tenses are absent, a number of analytical expressions replace synthetic aspectual verbs, and the reflexive is lost).

Ten years later, Polinsky (2016) shows the current dislike for the term “incomplete acquisition,” which though factual, is considered to be misguided and insensitive (Kupisch & Rothman, 2016). In this article, her goal is to discuss four possible scenarios of correspondence across the languages spoken by three groups: child bilinguals, “adult unbalanced bilinguals (heritage speakers),” and “bilingual native speakers of their home language (baseline)” (1). Polinsky does not use the expression “incomplete acquisition” in this article, but she implies it when she states that “a structure that is **not fully learned by child speakers** may be reanalyzed by adult heritage speakers” (1) and “In the HL of child learners, a relatively late age of acquisition for certain phenomena or structures may mean that those **structures are never acquired perfectly** and remain weak in the adult grammar.” (p. 13) (my emphasis)

In sum, not all researchers agree about the role that the concept of incomplete acquisition may play in bilingual development. Those who uphold the term appear to view it as a causal factor. By contrast, I conceptualize incomplete acquisition as a stage in language development, or in the case of adult bilinguals, as an acquisitional outcome of the reduced exposure and production of a minority language, among other causes, that results in the lack of aspects, elements, or features in a grammatical domain that are present in the input language. Support for my views is presented in what follows.

Language contact and change

Without doubt some features undergoing historical change appear to expand in the language of HSs. One such case is the extension of the copula *estar* ‘to be’ to contexts previously limited to *ser* ‘to be,’ which in conditions of “normal intergenerational language change” has taken centuries to extend to an increasing number of adjectives (most recently to *feliz* ‘happy,’ Fernández, 1964; Silva-Corvalán, 1994: p. 98). So, even though this is an ongoing change in Spanish, a comparison of two generations in a bilingual environment shows expansion of *estar* to more new contexts, and justifies suggesting that the second generation has not completed the acquisition of constraints that were valid in the preceding generation. The acceleration from first- to second-generation bilinguals is drastic (Silva-Corvalán: p. 1994); it enables the use of *estar* in general descriptions of inanimate entities, e.g., *Mi casa está amarilla* ‘My house is yellow’ (V21, G2), *La mía [nariz] está chistosa* ‘Mine [nose] is funny’ (A20, G2).

I have said that acquisition has not been completed when a grammatical domain lacks elements or features present in the learner's input language, and that this lack may also be reflected in a marked increase in the rate of use of a construction in the speech of bilinguals compared with its rate in the speech of input providers, or in the use of constructions not acceptable to the speakers of the input model. Thus, I maintain that the task of deciding whether a grammatical aspect has been completely or incompletely acquired must be done with reference to the grammar of the input, viewed as the complete model, and not to what may be possible in order for "a human grammar to be complete" (cf., Otheguy, 2016: p. 313).

Following this methodology,³ I have identified (Silva-Corvalán, 1994, 2014) features that are categorical in the input yet are lacking in the speech of second-generation bilinguals,⁴ e.g., use of imperfect tense with stative verbs when the preterite is required, absence of imperfect subjunctive and compound tenses, weakening of the constraint on the expression of coreferential subjects. I have shown that parallels between bilingual children's linguistic behavior and that of adult heritage speakers suggest that some aspects of the heritage language of adults may be the outcome of an interrupted process of acquisition of this language between the ages of 3;0 and 5;0, when more intensive exposure to English reduces exposure to the heritage language and diminishes the opportunities to use it. I proceed to discuss some of these studies.

The data

I have examined data from bilingual children and from adults mainly in two corpora consisting of recordings of sociolinguistic conversations:

1. Corpus East Los Angeles (ELA): the data from the adults has been selected from recordings of men and women living in the eastern section of greater Los Angeles, all Mexican-Americans of different ages classified into three immigrant generations according to the length of time that the speakers' families have lived in the USA (Silva-Corvalán, 1994).⁵ Generation 1 includes speakers born in Mexico, who immigrated to the US after the age of eleven. Generation 2 encompasses speakers

3. Cuza and Miller (2015) also consider the grammar of the input in their study of child bilingualism.

4. Bearing in mind that second-generation bilinguals do not constitute a homogeneous class.

5. These data are now available to the public at the University of Southern California Digital Library. The link to the data is: <http://digitallibrary.usc.edu/cdm/landingpage/collection/p15799coll22>

born in the US or who immigrated from Mexico before the age of six.⁶ Generation 3 includes speakers with at least one parent belonging to the second generation.

2. Longitudinal corpus: bilingual acquisition is examined primarily in a corpus of longitudinal data obtained during the first six years of life of two English-Spanish developing bilingual siblings, my grandsons Nico and Brennan (see Silva-Corvalán, 2014 for further details). Nico and Brennan acquired greater proficiency in English and use this language significantly more than Spanish. They grew up in a dual-language home: the mother speaks to them exclusively in English; the father used Spanish with Nico almost exclusively until the child was three and a half years old, but use of Spanish decreased from that age on. Brennan, who is two years and nine months younger than Nico, thus heard relatively less Spanish from his father, but in child-directed speech, the father used Spanish almost exclusively with Brennan as well, until he was about 3;0 years old. I spoke with the children almost exclusively in Spanish; they used English very infrequently with me. The children spoke almost only English with each other, occasionally responded in English to their father, but addressed him in English more and more frequently after age 3;0. The siblings' father came to the USA when he was 10 years old; his schooling has been in English from the fifth grade on.

I observed and recorded the siblings regularly using Spanish, English, or both in a variety of natural and uncontrolled discourse contexts and with different interlocutors. In addition, I kept detailed diary notes up to age 3;0 for Nico.

The children's degree of proficiency in English by age 6;0 is comparable to that of monolinguals. By contrast, their developing proficiency in Spanish is unequal. I suggest that this inequality results from differences in the amount of Spanish language input and production, more reduced for the younger sibling.

Overall, Nico was exposed to Spanish about one-third of his waking time; Brennan, less than one-third (about 25%). From about age 4;0, exposure to and use of Spanish are further reduced for both children to at most a quarter of the time. But despite such limited input, the children are able to develop conversational proficiency in Spanish, i.e., despite evidencing incompletely acquired grammatical aspects, the siblings have successfully developed a system of communication that is appropriate to their needs.

6. I set up the age of 11 for generation 1 assuming that, by this age, the individual has completed elementary education in Mexico and has become literate in Spanish. Immigration to the US before the age of six for inclusion in generation 2 is based on the assumption that the individual has done all its schooling in English.

Subject realization

In the acquisition of Spanish, the child needs to learn the semantic and discourse-pragmatic constraints that regulate the variable expression of subjects in this language. One of these constraints is *coreferentiality*. Overt subjects are favored probabilistically, among other variables, by subject switch reference, as in Example (1a)–(1b), where the subject of the clause in a (*tu hermano*) is not coreferential with that in b (*yo*).

- | | | | | |
|-----|----|-----------------------------------|----|-------------------------------|
| (1) | a. | Tu hermano quería saber | b. | si (yo) enseño en USC. |
| | a. | ‘Your brother wanted to know | b. | if [I] teach at USC: |
| | | | | [yo ‘I’ is “optional”] |
| (2) | a. | Pepe es un escritor muy conocido. | b. | 0 Ha escrito muchos libros. |
| | a. | ‘Pepe is a well-known writer. | b. | [He] has written many books.’ |

Continuity of reference (or *coreferentiality*), as in (2a)–(2b), has consistently been shown to disfavor overt subjects. An overt subject *él* in 2b is pragmatically questionable. On average, in various Spanish dialects, over 40% of non-coreferential pronominal subjects are overt, while only about 25% of coreferential subjects are expressed (Shin & Cairns, 2012).⁷

I have examined about 5,000 of the siblings’ declarative utterances containing a verb that had or could have had a subject. The utterances were coded for several factors, including the only conditions that require an overt subject: 1) when it is focal, either because it is new information or the focus of contrast, and 2) when it is needed to identify its referent. Here, I discuss only coreferentiality because this is the factor that more clearly and objectively shows that second-generation bilinguals have not acquired the same subject expression constraints as those in the input language. Indeed, use of subjects in Spanish appears to be affected as exposure to English increases and exposure to and production of Spanish become more reduced. By age 6;0, Nico expresses 42% of pronominal subjects (428/1030) and Brennan a high 69% (411/598). By contrast, in speech directed to the children, I express only 27% (119/445) of pronominal subjects in three recordings: when Nico was 6;1, 6;2 and 8;1, and when Brennan was 3;3, 3;4 and 5;4 (recordings #108, #110, #129).

As English patterns become more entrenched, the siblings may subconsciously replicate the [subject + verb] pattern of English in their Spanish. But Brennan surpasses his brother’s rate of expression by 27 percentage points between the ages of 4;0 and 6;0, thus showing a more intense effect from reduced exposure to Spanish.

7. Other linguistic factors that have been argued to affect subject expression include priming, discourse genre, verb tense; see, e.g., several in Carvalho, Orozco, and Shin (2015).

It is, of course, possible that the high rate of overt subjects might be justified, but the contexts where a subject is clearly the new information or required to identify a referent in the children's data are very rare, so this high rate could not be explained by these factors. The reason appears to be the violation of the coreferentiality constraint.

The frequent expression of coreferential subjects not justified by another favoring factor, as in Examples (3) and (4), is interpreted as redundant by monolingual speakers.

- (3) Él tiene esos [antenas] que son tan, tan largas que está en *outer space* y él te tira para *outer space*, en tu auto.
 [Brennan: 4;1, talking about making a robot when he grows up]
 'He has these [antennas] that are so, so long they reach outer space and he pulls you to outer space, in your car.'
- (4) En el RV, yo a las 5 yo despertó y después yo fui arriba donde mi papá y mi mamá duermen y dormí allí. [Brennan: 5;3]
 'In the RV, I, I woke up at 5 and afterwards I went upstairs where my dad and my mom sleep and (I) slept there.'

A quantitative analysis of coreferentiality in the last age period, when the siblings evidence the highest rates of subject pronouns, shows that this constraint was weak in Nico's speech, and absent in Brennan's (see Table 1). Nico expresses 38.5% of coreferential subjects, more frequently than the 25% average in adult speech, but he replicates the monolingual trend by expressing switch reference subjects even more frequently, 44.5%. Brennan evidences absence of this constraint: he does not differentiate coreferential from non-coreferential subjects with respect to frequency of expression.⁸

Table 1. Subject pronoun realization by coreferentiality (4;0 to 6;0)

	Nico		Brennan	
	Overt/Overt + Null		Overt/Overt + Null	
	N	%	N	%
Coreferential	200/520	38.5	194/285	68.1
Non-coreferential	205/461	44.5	217/313	69.3
	(p < .05)		(p < .74)	

8. Based on the Pearson Chi-Square statistic.

In sum, although both children are simultaneous bilinguals in Spanish and English from birth, by age 6;0 the child with less exposure to Spanish reveals that acquisition of the coreferentiality constraint in his HL has not been completed. Interestingly, his behavior shows some similarity to that of adult second-generation New York speakers, in whose Spanish Shin and Otheguy (2009) identify an important decrease in sensitivity to continuity of reference. This intergenerational difference is not attested between two generations in monolingual communities.

The subsequent section discusses some gaps in the acquisition of tenses in Spanish.

Tense acquisition

The speech of the siblings by age 6;0 and of second-generation adult bilinguals in Los Angeles suggests incomplete acquisition in childhood of some components of the Spanish verb system: the perfective –imperfective (preterite –imperfect) aspectual opposition, some subjunctive tenses, and some compound tenses.

Table 2 presents information about the verb tenses used by the children during the last age period studied: 5;0 to 6;0 years of age. Besides Nico and Brennan, this table includes Daisy and Mike, two Mexican-American bilingual children living

Table 2. Spanish TMA usage compared across bilingual children, a (near) monolingual child (Daisy), and two adults. Children's age range is 5;0 to 6;0

	Sil	Daisy	Mike	Nico	Brennan	Vim
	G1	Sp	Bil	Bil	Bil	G2
<i>Indicative mood:</i>						
Preterite	+	+	*	*	*	*
Imperfect	+	+	@	+	@	+
Present Perfect	+	+	+	+		+
Pluperfect	+					
<i>Subjunctive mood:</i>						
Present	+	+	+	+		0
Imperfect	+	+		+		0
Present Perfect	+					
Pluperfect	+					

“+” tense form is used according to the norms of general spoken Spanish;

“*” closed list of stative verbs used with imperfect morphology in preterite –perfective contexts;

“@” some preterites instead of imperfects;

“0” form has failed to occur in a high number of obligatory contexts; blank: form is not part of the system underlying the speaker's spontaneous use of Spanish.

in Los Angeles. The data reported in Table 2 are based on recordings done when all the children except Brennan were attending kindergarten; Brennan was in pre-school. For purposes of comparison across the children, Table 2 includes data corresponding to the same age range: 5;0 to 6;0. The table also includes an adult from generation (G) 1, Sil (female, 25 years old) to facilitate reference to an unreduced verb system, and an adult from generation 2, Vim (female, 18 years old), who was exposed to Spanish and English from birth, but has from an early age preferred to speak English. The information in this table is based on a minimum of three hours of recording of each speaker.

The G1 speaker, Sil, does not lack any of the tenses listed in Table 2. She and Daisy, who is G2 but speaks only Spanish at home, are the only ones who use stative verbs with Preterite morphology consistently in perfective contexts. The other children and the G2 bilingual use a closed list of stative verbs in the Imperfect instead of the Preterite, as illustrated in 5–7. The TMA system of the children and the adult heritage speaker match in this respect (see Silva-Corvalán, 1994, Chapter 2).

- (5) R: ¿Y cuándo es tu cumpleaños? [Researcher = R]
 M: Ya **era**. [**fue** expected] [R: ¿Sí?] **Era** el 2. [**fue** expected] (Mike, 5;5, G2)
 R: ‘And when is your birthday?’
 M: It was already (Imp; Pret expected). [R: Yeah?] It was on the 2nd (Imp; Pret expected).’
- (6) N: Y Amanda le dio un puñete en, en, en el estómago. (N = Nico, 5;7)
 R: ¿A quién?
 N: A Edwin, un, un compañero mío. [R: ¡Oooh!] Y ellos, te – **tenían** que poner su cabeza en, en su, en su escritorio. [**tuvieron** expected]
 N: ‘And Amanda hit him in, in, in the stomach.’
 R: Whom?
 N: Edwin, one, one of my classmates. [R: Oooh!] And they ha- **had** (Imp; Pret expected) to put their head on, on, on their desk.’
- (7) [Beginning of a narrative; perfective context]
 Ahhmm, pues una vez – a ver, **era** con un muchacho que pues a mí no me agradaba nada. (**fue** expected) (H22,m21,G2)
 ‘Uhhh, well one time – let’s see, it was (Imp; Pret expected) with a guy that, well, I didn’t like at all.’

Stative verbs (e.g., *ser* ‘to be,’ *estar* ‘to be,’ *tener* ‘to have,’ *haber* ‘there to be,’ *poder* ‘can’) occur more frequently in imperfective contexts in the data from G1, but in perfective contexts, G1 speakers use them appropriately in the Preterite (Example 8). This usage rule was not completely acquired by the simultaneous bilinguals, even though it was categorical at least in the siblings’ input, as illustrated by the examples from their father (9) and me, their grandmother (10). The example from G1 speaker A9

(Example 8) further suggests this usage rule is widely attested among speakers who arrive in the U.S. after age eleven.

- (8) Manuel absorbió mucho más el español, ¿verdad? Fue (Pret) el primero. Entonces él **pudo** (Pret) hablar puro español. (A9,f60,G1)
 ‘Manuel learned Spanish better, right? He **was** (Pret) the first one. So he **was able** (Pret) to speak only Spanish.’
- (9) Father: ¿Cómo **estuvo** (Pret) la escuela, mi amor? Kiko, ¿qué pasó en la escuela hoy día, qué hiciste? [Kiko is Nico’s nickname]
 ‘How **was** (Pret) school, my son? Kiko, what happened in school today, what did you do?’
- (10) C: Hoy yo no **pude** (Pret) traer ninguna banana a la escuela; no tenía ninguna, ninguna banana.
 ‘Today I **couldn’t** (Pret) bring a banana to school; I didn’t have a single banana.’

When the process of normal acquisition is interrupted at around age 6;0, the children do not reach the final stage, i.e., the marking of statives with perfective morphology. This stage of incomplete acquisition of the Preterite – Imperfect opposition with stative verbs characterizes the speech of eight of the twelve G2 adults I studied in Los Angeles (Silva-Corvalán, 1994).

Table 2 also shows that the adult HS and Brennan do not have full control of any of the Subjunctive tenses, even though these tenses are attested in the speech of their input providers. Since I am the main provider of Spanish for Brennan, and secondarily his father, I can verify that his TMA input is unreduced. Any gaps in their data reflect incomplete acquisition of the input model. As further illustration of an intergenerational difference in the Spanish verb system, Table 3 compares Vim (f18) and her sister Rit (f,21) with their parents. Examples 11–15 illustrate utterances that miss the tense target.

- (11) Yo le dije ojalá que se **va**. [expected: *vaya* ‘would leave / left’] (Rit,f21,G1)
 ‘I told him I wish he **leaves**.’
- (12) Y estábamos esperando a mi ‘amá – porque ella **fue** a llevar mi hermano a la dentista. (expected: *había ido*) (Vim,f18,G2)
 ‘And we were waiting for my mom – because she **had gone** (Lit.: *went*) to take my brother to the dentist.’
- (13) R: ¿Qué le **hubiera pasado** [if the water had fallen over him]?
 M: el agua **se cayó** donde él. [expected: *Se le hubiera caído* el agua encima / *Se hubiera mojado*.] (Mike,5;5,G2)
 R: ‘What **would have happened** to him?’
 M: ‘Water **fell** over him.’ [expected: *The water would have fallen* over him / *He would have gotten wet*.]

Table 3. Tense usage compared across two generations: parents and daughters (18 and 16 years old)

	Generation 1		Generation 2	
	Rae	Jim	Rit	Vim
	[parents]		[daughters]	
<i>Indicative mood:</i>				
Preterite	+	+	*	*
Imperfect	+	+	@	+
Present Perfect	+	+	+	+
Pluperfect	+	+	+	
<i>Subjunctive mood:</i>				
Present Subj	+	+	0	0
Imperfect Subj	+	+	0	0
Pluperfect Subj	+	+		
PresPerfect Subj	x	+	n	n

Note: “x” form fails to occur in some obligatory contexts;

“n” no pragmatic context for the use of a form is identified in the data examined.

- (14) N: Porque él quería que la gente mala encontrara-*ImpSub* [on target] pedazos de él para que ellos ~*hacía-**Imp*** más robots. [*ImpSub* required: *hicieran*] (Nico,5;6)
 ‘Because he wanted the bad people to find pieces of him so that they **made** more robots.’ [expected: *could make*]
- (15) B: ~No cuando ya *tengo-**PresInd*** dieciséis años. [expected: *tenga-**PresSub***] (Brennan, 5;4)
 ‘Not when [I] **am** sixteen years old.’

At a slightly earlier age, Matías (4;6) and Javier (4;11), Chilean boys I recorded in Santiago, Chile, produce simple subjunctive tenses and indicative compound tenses regularly. Examples 16–19 illustrate:

- (16) Matías: Este Renault. No me importa que *tenga* (*PresSub*) ruedas de Citroen; tiene ruedas de Citroen, pero le van a servir al Renault. (4;6)
 ‘This Renault. I don’t care if it has Citroen wheels; it has Citroen wheels, but they’ll work for the Renault.’
- (17) Matías: Pues cuando te *vayas* (*PresSub*) ve, ve si es así pues. (4;6)
 ‘Well when you leave see, see if it’s like this.’

- (18) Matías: Mi mami me **ha dicho** eso. ¡Pucha! ¿Sabes que un día yo iba al colegio en el Renault, y de repente- y de repente nos fuimos saltando?, y la Nancy no sabía lo que pasaba pa' ya, y ¡pum! y era que se **había desinflado** el neumático. (4;6)
 'My mom **has told** me that. Gee! You know that one day I was going to school in the Renault, and all of a sudden- and all of a sudden we started bouncing? And Nancy didn't know what what happening, and pum! And it was that the tire **had deflated**.'
- (19) Javier: ... y despertaron todos porque los **habían hecho** dormir a todos. (4;11)
 '... and everyone woke up because they **had put** everyone to sleep.'

Differences among the bilingual children and adults, and between the bilinguals and monolinguals correlate with home language and consequent amount of exposure to Spanish. The monolingual children have access to the critical mass of input needed to attain the models provided by the preceding generation. At a similar age, the bilingual children have not yet acquired the less frequent tenses. Acquisition of the complete tense system will remain incomplete if exposure continues to be insufficient.

Position of clitics in verbal periphrases

Spanish clitics (*me, te, se, le, lo, la, nos*) are unstressed pronominal elements also referred to as *verbal clitics*, because they must occur either before or after a host verb.

When clitics refer to an argument of an infinitive or a present participle in a verbal periphrasis with a finite '(semi)-auxiliary' verb, they may **variably occur before or after the finite verb**. Example 20 illustrates:

- (20) *Viene a verte / Te viene a ver*
 comes-3SG to see-you / you comes-3SG to see
 'He's coming to see you'

This phenomenon is constrained by at least two factors: the semantic properties of the matrix or 'semi-auxiliary' verb, and the relative topicality of the subject and the clitic (Myhill, 1988, 1989). The semantics of the matrix verb is crucial. Preverbal placement is strongly favored when this verb retains little or none of its basic meaning and functions rather as a marker of tense, modality, or aspect, as illustrated by a comparison of Examples 21a and 21b.

The clitic could have been placed in pre- or postverbal position in these examples. In 21a, *venir* has its basic meaning of movement. But in 21b, *venir* has the grammaticalized meaning of iteration, the periphrasis is like a single finite verb, and the preverbal placement of the clitic is favored.

- (21a) Matrix verb: *venir* ‘to come’ (basic meaning)
 Pepe *viene* a entrevistarme hoy.
 ‘Pete’s *coming* to interview *me* today’
- (21b) Matrix verb: *venir* ‘keep’ (grammaticalized meaning: iteration, repetition)
 Pepe *me viene* molestando por años ya.
 ‘Pete’s *been/kept* bothering *me* for years’

The preverbal position is preferred when the matrix verb conveys epistemic meanings, progressive aspect, and future tense. This preference for preverbal position in data from four Spanish speaking countries ranges from 62% (in Venezuela) to 70% (in Chile and Spain) and 77% (in Mexico) (Silva-Corvalán, 1994). There is an internally motivated trend to place the clitic in front of the matrix verb related to the gradual grammaticalization of a number of verbs (Torres Cacoullós, 1999).

Table 4 shows that preverbal position is also favored by the children. Besides Nico and Brennan, this table includes M&R, two Spanish-speaking Chilean children recorded in Santiago, Chile, at ages 4;6 (M) and 5;6 (R), and two Mexican-American children, Daisy and Mike. Daisy is from a Spanish-only home; her English is at an incipient stage. Mike speaks both Spanish and English at home, but he is dominant in English. Note that the quantitative results for these four children are very similar. Nico and Brennan, on the other hand, who are from a home where English is clearly prevalent, have a higher overall percentage of postverbal clitics, thus revealing the possibility of some influence from English in this regard.

Table 4. Overall rate of pre- and postverbal clitics in verbal periphrases. Children’s age range is 4;6 to 6;0

	Preverbal		Postverbal	
	N	%	N	%
M&R *	70/75	93.3	5/75	6.7
Daisy *	31/33	94.0	2/33	6.0
Mike *	97/102	95.1	5/102	4.9
Nico	61/75	81.3	14/75	18.7
Brennan	34/45	75.6	11/45	24.4

* Note: M&R are Chilean children recorded in Santiago, Chile, at ages 4;6 (M) and 5;6 (R). Daisy, Mike, Nico, and Brennan are bilingual children recorded in Los Angeles.

Be that as it may, all the children follow the adults' preference for preverbal position when the matrix verb conveys future tense, progressive aspect and epistemic meanings, as shown in Table 5. The variables which simply favor preverbal placement in English-dominant bilinguals appear as categorical or almost categorical contexts for this order in data from M&R, Daisy, and Mike.

Table 5. Rate of preverbal clitic placement in verbal periphrases. Adult and children bilinguals⁹

	<i>Ir a</i>	<i>Estar</i>	<i>Poder</i>	<i>Tener que</i>	<i>Empezar a</i>	<i>Querer</i>	<i>Deber (de)</i>
	'go'	'be'	'may'/'can'	'have to'	'begin to'	'want to'	'must'
	%	%	%	%	%	%	%
G 1	92	91	60	57	73	32	17
<i>Children</i>							
M&R	100	100	100	38	2/2	2/3	
Daisy	100	100	100			22	
Mike	94	92	100			100	
Nico	89	4/4	92	57		46	
Brennan	100	33	59			1/1	

Note: G 1 = 7 speakers. Future (*ir a*), Progressive (*estar*), Epistemic (*poder, tener que*).

Examples 22–26 from the bilingual children, illustrate clitic position with different matrix verbs.

- (22) Yo te voy a, yo te voy a contar un cuento. (Brennan 4;5)
 'I'm going to, I'm going to tell you a story.'
- (23) La *snake* lo estaba agarrando (Mike 5;10, NB-2)
 'The snake was grabbing him.'
- (24) ¿Y yo me lo puedo llevar? (Daisy 5;6, NB-22)
 'And I can take it with me?'
- (25) Yo quiero verlo. [a movie] (Nico 5;9)
 I want to see it. [a movie]
- (26) ¿Quieres, lo quieres mirar? (Nico 5;9)
 Do you want, you want to watch it?

The trend to place the clitic in front of the matrix verb, internally motivated, is related to the gradual grammaticalization of a number of verbs, which is in keeping with the conclusions reached by Gutiérrez (2008) in his study of clitic climbing

9. Percentages are not calculated when there are fewer than 5 cases.

in Houston, and with Shin, Requena, and Kemp's (2017) examination of direct object clitic placement in data from bilingual and monolingual children, and from adults. These authors show that the children attend to probabilistic patterns in the input and closely match the position of clitics in the speech of the adults in their community.

This section has also shown that the Spanish of bilinguals moves in the direction of strengthening the robust Spanish internal trends. The postverbal placement of the corresponding pronouns in English is of no consequence, except perhaps in the case of the slightly higher percentage of postverbal clitics used by Nico and Brennan, in whose home English is clearly prevalent.¹⁰ Clearly, robust internal trends provide the necessary amount of input for the children to replicate them and thus contribute to the complete acquisition of the grammatical aspect in question. This differentiates clitic placement from the two other phenomena examined here: the weakening of coreferentiality and of some verb tenses, which are not tendencies attested in the speech of the bilingual children's Spanish language models.

Conclusion

I have discussed some connections between bilingual acquisition in the early years and some aspects of the grammars of adult heritage speakers which appear to support that insufficient exposure to and use of Spanish contribute to the incomplete acquisition of some grammatical aspects.

The decrease in sensitivity to continuity of reference evidenced by two bilingual children is also demonstrated in a large sample of generation 2 speakers in New York (Shin & Otheguy, 2009), while the weakening of this constraint is not attested in monolingual children and adults. This suggests that the incomplete acquisition of this constraint results from reduced exposure to Spanish in a situation of intense contact with a dominant language.

Lessened exposure is also responsible for the absence of some tenses in the siblings' Spanish and in one more US-born bilingual child. It is clear that by age 5;0 to 6;0, the children, including a near Spanish monolingual child, have not yet acquired the adult system of tense, mood, and aspect in Spanish (Cuza & Miller, 2015). Some features of the children's TMA system are also characteristic of that of the adult second-generation speaker (Vim). Notably, the absence of subjunctive and compound tenses, and the use of the imperfect instead of the preterite with stative verbs are features that attest to a halted process of development rather than attrition of knowledge.

10. Pérez-Leroux, Cuza, and Thomas (2011) also show a diminished preference for proclisis by the bilinguals in their study.

The preferred preverbal position of clitics in verbal periphrases, on the other hand, strongly favored by monolinguals, is matched by the bilingual children. Clearly, the frequency of verbal periphrases, coupled with the robust process of grammaticalization of (semi) auxiliary verbs that promote proclisis, provide the necessary amount of input for the children to acquire the adult patterns. Infrequent and complex phenomena, even if stable in the input (e.g., use of subjunctive, coreferentiality), will have less of a chance to become stabilized in the speech of bilingual children in conditions of reduced exposure.

I have shown that some aspects of the heritage language of adults are most likely the outcome of an interrupted process of acquisition of this language around the age of 5;0, when more intensive exposure to the L2 reduces exposure to the heritage language and diminishes the opportunities to use it.¹¹ The description of this developmental trajectory as deviant or abnormal is certainly inappropriate; by contrast, incomplete or interrupted acquisition of some grammatical aspect does not convey a negative evaluation.

The hindered process of acquisition and development of Spanish underlies incompletely or partially acquired grammatical domains (Montrul, 2008; Polinsky, 2006). I do not argue that adult HSs have acquired the *entire system* of their heritage language incompletely, or that they do not have communicative competence in this language (although those at the lower ends of the bilingual continuum may not, see Silva-Corvalán, 1994), but that some HSs, including many at the higher ends of the continuum, evidence some reduced or incompletely acquired grammatical aspects compared with these (unreduced) aspects in the grammars of monolingual peers and their adult models in the preceding generation. Regardless of the gaps in some grammatical aspects, the outcome in the case of the two bilinguals studied in Silva-Corvalán (2014) is successful inasmuch as the children are able to use their heritage language to fulfill their communicative needs.

Obviously, it is not reasonable to postulate that second-generation HSs acquire an incomplete **grammar**, although the incomplete acquisition of some grammatical **aspects** means that their language is different. The notion of “difference” requires a point of comparison to be able to say in what respect a language variety X is different from Y, and in addition it should be necessary to explain what language features differentiate X from Y. This is normal practice in dialectology.¹² Incompleteness is

11. Meisel (2014) suggests that a period of stabilization up to age 11–12 might be needed to ensure that grammatical knowledge remain permanently accessible.

12. I do not ignore the fact that change may and does occur from one generation (1) to the next (2), but in predominantly monolingual communities and under conditions of normal transmission (Thomason & Kaufman, 1988), generation 2 does not differ from generation 1 to the same extent as in the bilingual communities I have studied in Los Angeles.

not a mechanism, but an acquisitional outcome or a stage in language development. The notion of “incompleteness” as an outcome also requires a comparison with what is “complete.” Thus, if some domains of the grammars of X (generation 1) are “complete,” then in comparison with these grammars, the absence of required subjunctive and compound forms, and the weaker sensitivity to coreferential constraints in Y are an indication of difference but also incompleteness with respect to X.¹³ Furthermore, *incomplete acquisition* prompts more forcefully than simply *difference* the search for possible causes of this stage. I suggest that the causes are complex, they involve both internal (e.g., complexity) and external (e.g., frequency, crosslinguistic influence) factors that contribute to the interrupted development of the heritage language. Diverse degrees of restricted exposure and production of Y account for the range of proficiencies attested among adult heritage speakers.

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13. While many speakers in generation 2 and most in generation 3 never use subjunctive, all speakers in generation 1 never fail to use a subjunctive form in a required context, e.g., in the complement of the verb *querer* ‘want’ (*Es que yo quiero que mis hijos conozcan su idioma* ‘It’s that I want that my children may know their language’).

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An incomplete disquisition against ‘incomplete acquisition’

With particular reference to changes in the distribution
of the subjunctive in Spanish

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This chapter presents a multi-pronged argument, from a generative syntactic perspective, against the notion of ‘incomplete acquisition,’ centered on subjunctive usage by Spanish heritage speakers in New York City. Special attention is paid to the distribution of the subjunctive in futurate *cuando* clauses and in causative and permissive constructions with *hacer* and *dejar*. It is argued that the distribution of the subjunctive in second-generation speakers of Spanish in New York City is different from that seen in monolingual Spanish speakers; but it has not been in completely acquired. The paper also contains relevant discussion of English *let’s* hortatives, and the acquisition of subject–auxiliary inversion in root *wh*-questions by first- and second-language learners of English.

Keywords: incomplete acquisition, subjunctive, *cuando* clauses, causative/permissive constructions, *let’s* hortatives, subject–auxiliary inversion, Spanish, English, generative syntax

1. Primitive prolegomena

Grammatical analysis, regardless of its theoretical predisposition, must avail itself of certain primitives with which to go about its job. But the naming of these primitives is not seldom at the root of terminological miscommunications and controversies between different theoretical persuasions. The Chomskian generative approach takes the vast majority of its primitives from the Western grammatical tradition – adding a degree of abstraction to them that will facilitate their application to languages which do not *prima facie* seem to be good candidates for the use of the primitives in question. Case will serve as good case in point here. The avowedly

universal Case Filter of early principles-and-parameters theory (Chomsky, 1981), which states that (overt, argumental) noun phrases must have Case (customarily spelt with an initial capital), will only be feasible if Case is taken to be an abstract notion: plainly, many languages of the world do not avail themselves of visible case morphology. Abstract primitives such as ‘Case’ are useful if it turns out that they help us understand fundamental properties of the world’s languages. If they turn out, upon closer scrutiny, not to be beneficial, their abolition is called for. Occam’s Razor will always keep the analyst on his toes in this regard.

Even for features that are directly observable in a language or family of languages under investigation, the question of how to name them is by no means a trivial matter, however. For one illustrative case, let us consider the fact that question words, in many of the world’s languages, form a natural class in morphophonological terms: in English and the other Germanic languages, almost all of them are introduced by a labial approximant (English *what*, *where*, *when*, *why*, etc.; *how* /hau/ and *who* /hu:/ are outliers); in the Romance languages, they typically begin with a velar stop (French *qui*, Italian *chi*, Spanish *quien*, Romanian *care* ‘who’).¹ There is a widespread practice in the generative approach to generalize over question words as a group with the help of a feature named [+WH] (its name obviously deriving from the particular form that English question words tend to take), and to take this feature to imbue the sentence with the interpretation of a question (i.e., [+WH] is an ‘interpretable formal feature,’ in the terminology of Chomsky, 1995). The problem here is not the anglocentric nomenclature. Rather, the trouble is that there really is no straightforward link between this [+WH] feature and question-wordhood or interrogative interpretation. Even for English (which is by no means the world’s most versatile user of *wh*-expressions), this is easy to demonstrate: the word *what* occurs in constituent questions (*what are you eating?*) but also in free relatives (*what you are eating looks delicious*) and exclamatives (*what a delicious pie!*). Morpho(phono-)logically, these three occurrences of *what* are transparently identical. And syntactically, they share the fact that they must all occur in a position at the left edge of the clause. So from a syntactic point of view as well, we would like to have the opportunity to tie them all together. We can certainly use [+WH] for the purpose of generalizing over all occurrences of *what*. But we must abandon the use of this feature to make specific reference to *questions*.

That this is necessary is clear not just from the considerations just reviewed but also from the fact that questions are not systematically introduced by an element

1. Recognizing question words on the basis of their morphophonology is often by no means easy: thus, one will be hard pressed distilling ‘the mark of a question word’ from the following three Hungarian forms: *ki* ‘who’, *melyik* ‘which’, and *hol* ‘where.’

visibly/audibly bearing the feature [+WH]: yes/no-questions, and also constituent questions involving so-called 'wh-drop' (see Dutch *heb je nou gedaan?* '(lit.) have you done now, i.e., what have you done now?') are a clear testament to the fact that questions frequently are not introduced by an overt operator. Treating such questions with the aid of silent operators is fine; but saying that these operators are covertly specified for the feature [+WH] and are fronted to 'check' a matching [+WH]-feature on a head in the high left periphery of the clause looks suspiciously like a self-fulfilling prophecy. At this juncture, the theory will be better served by a rethinking of the analysis of question formation and interrogative interpretation. If that analysis is to be couched in terms of a feature, [+WH] is not likely to be particularly helpful; the label [Q], already introduced in Katz and Postal's (1964) seminal approach to questions in the generative framework, is indubitably a much better choice.

I will return to *wh*-constructions in Section 7, but at this point I would like to switch to the subjunctive, which is the thread common to most of the earlier sections. In the realm of mood, too, there is a well-established nomenclature, based on the Latin labels 'indicativus' and 'subjunctivus' (or 'coniunctivus'). The latter feature has been linked to a variety of interpretive correlates. The speaker's attitude towards the proposition and reduced control by the subject over the event expressed by it are among them: thus, in English *I prefer that he leave* (with a so-called subjunctive, characterized in lexical verbs by the absence of third-singular -s), the speaker is not convinced that the embedded proposition will materialize and therefore does not wish to assert (using the indicative) that it will, and the embedded subject exerts reduced control over his departure. But we witness these interpretive properties also in *I prefer for him to leave*, with a *to*-infinitive. So the link between the subjunctive and assertiveness and reduced control is hardly biunique. For English, we can plausibly achieve a generalization over the infinitival and so-called subjunctival cases by actually denying that English has a subjunctive, and treating it as an infinitive: the so-called subjunctive of English is systematically morphologically identical with the infinitive, even for the most irregular of verbs (*I prefer that he be fired*, *I prefer for him to be fired*);² analyzing it as an infinitive with a nominative subject (or, put differently, an infinitive embedded under a nominative-assigning finite complementizer) is no different from the widely accepted analysis of *his constantly leaving*

2. In *If I/(s)he were smart, ...*, the form *were* is not a specialized subjunctive either: what we see (optionally) in the apodosis of English conditionals is a suspension of the person/number distinctions in the paradigm of past tense *be*, with *were* being usable with all subjects (incl. first and third person singular, which otherwise combine with *was*). An 'impoverishment' rule in the morphology (confined to *was/were* in apodoses of conditionals) can take care of this.

the room as a verbal gerund embedded under a genitive-assigning determiner head. Then, the feature [+SUBJUNCTIVE] plays no role at all in the morphological system of the language, and we can tie its apparent interpretive properties to the occurrence of an infinitival verb form below verbs of certain kinds. For languages that do have a morphological subjunctive, we want to have this feature at our disposal for the description of the verbal paradigm. The interpretive properties of speaker attitude and reduced control will have a relationship with this morphological property – but they cannot be taken to be in a biunique relation: infinitives in such languages often evince these interpretive properties, too.

So again we see that, although there is a connection between a particular morphological property and an interpretive one, the link is far from inextricable. It will be important to have at our disposal the kind of nomenclature to refer to purely formal properties such as [+SUBJUNCTIVE] or, in the previous exercise, [+WH], as well as the nomenclature to make reference to interpretive properties such as ‘reduced control,’ or ‘questionhood.’ We can occasionally achieve some nomenclatural economy by getting rid of a certain morphological category for a particular language (the English subjunctive offers such an opportunity); but in general, the message is that separate nomenclatures for morphological and interpretive properties are needed, and that it is not a good idea to try to collapse the two – for instance, by introducing the notion of ‘interpretable formal feature,’ as was done in Chomsky (1995). Formal (i.e., morphosyntactic) features are themselves semantically uninterpretable; but they often have a predictable (though not biunique) connection to interpretive notions such as ‘questionhood,’ ‘speaker attitude,’ or ‘reduced control.’ For formal and interpretive notions, we require separate nomenclatures, and ensuring that these nomenclatures are sufficiently discrete will greatly enhance the accuracy and precision of linguistic discourse, as well as communication between different approaches to linguistic analysis.

With these things said, let us now embark on an investigation of the use of the subjunctive in different varieties of Spanish – an investigation which will be centered on the question of whether differences among speakers are sensibly characterized by the label ‘incomplete acquisition,’ another important piece of nomenclature that I would like to place under scrutiny in this piece.

2. Changes in the use of the subjunctive in the Spanish of New York City

The use of indicative and subjunctive mood by heritage speakers of Spanish has been the subject of detailed study for several decades, with Lantolf (1978), Torres (1989), Silva-Corvalán (1994), Lynch (1999), and Montrul (2009) as major landmarks. In the context of a recent investigation of 26 first-generation Latin American

newcomers to New York City and 26 second-generation bilingual New Yorkers, Bookhamer (2013) synthesizes previous scholarship and shows that there is quite a bit of variation in these studies' findings. But on aggregate, the distribution of the subjunctive declines from first- to second- to third-generation speakers of Spanish in non-Spanish-speaking environments.

Bookhamer (2013) finds that out of the nine environments in which the use of the subjunctive is commonly taken to be inescapable in monolingual Spanish, first-generation Spanish-speaking immigrants in New York City use the subjunctive (near-)categorically in only five (including the causative *hacer* + finite clause construction). Second-generation speakers, on the other hand, use the subjunctive (near-)categorically only in the causative *hacer* + finite clause construction.

At the most coarse-grained level of analysis, one could summarize the body of scholarship on the distribution of the subjunctive in heritage speakers' Spanish in the following terms. The subjunctive, still firmly in place in the syntax of monolingual speakers of Spanish (both in Spain and in Latin America), is losing ground among Hispanic immigrants in North America. The acquisition of the subjunctive among first- and, especially, second-generation immigrants is, one might say, 'incomplete': it falls short of the target of the standard language.

But it should be easy to see why such a characterization of the situation concerning the specific case of the Spanish subjunctive completely misses the point. The fact of the matter is quite clearly *not* that first- and second-generation immigrants have a blanket problem acquiring the subjunctive. As Bookhamer (2013) notes, both generations of Spanish speakers in New York City use the subjunctive (near-)categorically in the finite complement of causative *hacer*. Thus, they have evidently acquired the subjunctive itself perfectly. The subjunctive *per se* is unchanged and complete; it is its range of use that has shifted. As Bookhamer (p. 93) puts it in his comparison of first- and second-generation Spanish-speaking immigrants in New York City, "the primary difference regarding ... subjunctive use is that it occurs less frequently [in the speech of second-generation immigrants] when compared to the [first-generation] consultants. The form is still very much in use by the second generation, just as it is with the first-generation Spanish speakers." What makes the heritage speakers differ from monolingual speakers in Spain and Latin America, and among each other, is that they do not use the subjunctive in precisely the same environments to exactly the same degree. The *distribution* of the subjunctive has shifted – not the subjunctive as such.

One might at this point still be inclined to believe that this shift in the distribution of the subjunctive represents 'incomplete acquisition': the immigrant populations fail to hit the mark by not picking up the entire set of syntactic environments in which monolingual Spanish is said to require the subjunctive. But that, too, seems to be a serious misrepresentation of the facts. A particularly revealing glimpse of

what may really be going on is offered by one context where generational differences in the use of the subjunctive are particularly noticeable: futurate adverbial clauses introduced by *cuando*.³

3. The subjunctive in futurate adverbial clauses introduced by *cuando*

In futurate adverbial clauses introduced by *cuando*, which is customarily translated into English as ‘when’ (though this, as I will argue below, is probably not quite accurate), the grammars of monolingual speakers of Spanish on both sides of the Atlantic generally call for the use of the subjunctive, as illustrated in (1a) (taken from Otheguy, 2016):

- (1) a. *hablaremos cuando vengan*
 talk.FUT.1PL when come.SUBJUNC.3PL
 b. **hablaremos cuando vienen*
 talk.FUT.1PL when come.INDIC.3PL
 ‘we will talk when they come’.

As Zagona (2002: p. 39) puts it in her authoritative *Syntax of Spanish*, “adjunct clauses introduced by *cuando* ‘when’ require the subjunctive if the subordinate predicate is interpreted as subsequent to the moment of speech.”

Bookhamer (2013: p. 73) observes that first-generation speakers in New York City still use the subjunctive in 97% of cases – essentially categorically, therefore. But among second-generation speakers, the use of the subjunctive has dropped to a mere 76% of the time, with the indicative being used elsewhere, as in (1b’) (identical with (1b) except for the grammaticality diacritic).

- (1b’) %*hablaremos cuando vienen*
 talk.FUT.1PL when come.INDIC.3PL
 [second-generation Spanish speakers in NYC].

This 21% gap between the use of the subjunctive in *cuando* clauses by first- and second-generation immigrants is obviously indicative of something. But does it indicate ‘incomplete acquisition’ of the grammar of the subjunctive or something else entirely?

3. As Bookhamer (2013: p. 74) puts it, surveying the literature on subjunctive use in Spanish heritage speakers (incl. his own study): “The environment that consistently shows the most cross-generational variability among these studies is the context we refer to as temporal adverbial clause with futurity.” The outlier is Lynch (1999), who found 100% subjunctive use by all three of his groups in the context of futurate *cuando* clauses.

An important clue that points us to a plausible answer comes from Haverkate’s (2002: p. 142) observation that even in monolingual Spanish, “*cuando* clauses oriented towards the future do not necessarily require the use of the subjunctive” – and that futurate *cuando* clauses featuring the indicative have a particular interpretation that lends them precisely to the use of the indicative rather than the subjunctive. Haverkate gives the following as an especially illustrative example (taken from the Spanish newspaper *El Pais*, 24 November 1988):

- (2) *pero también constituye un experimento para un futuro no muy lejano cuando el enemigo de ayer, el imperio soviético, en decadencia, se convertirá^{INDIC} en la nueva Rusia que deberá formar parte de la Europa del futuro*
 ‘but it is also an experiment for the near future when the enemy of yesterday, the Soviet empire, fallen into decline, will become the new Russia and must form part of the Europe of the future’.

Tellingly, Haverkate explains that by using the indicative rather than the subjunctive “the writer of this sentence communicates his conviction that the proposition expressed in the *cuando* clause will come true in the near future” (p. 142). To paraphrase, in 1988 it was not a question of *if and when* the Soviet empire would become the new Russia – *that* it would was certain, and the author was even quite sure *when* it would: in ‘the near future’ (as it happens, the Soviet Union was formally dissolved only in 1991, and whether ‘the new Russia’ forms part of Europe is a matter of opinion).

In light of this, the answer that suggests itself to the question raised below (1b’) is the following. The shift in the use of the subjunctive in *cuando* clauses is symptomatic of a shift in the lexical meaning of futurate *cuando*. For monolingual and first-generation speakers, this *cuando* generally means ‘if and when’ (cf. German *wenn*, etymologically a temporal *wh*-expression, synchronically used both in temporal adjunct clauses, translating as ‘when,’ and in conditional clauses, translating as ‘if’). For second-generation speakers, by contrast, *cuando* can readily just mean ‘when.’ With its ‘if and when’ meaning, the *cuando* of monolingual and first-generation speakers predictably combines only with the subjunctive: the ‘if’ in its meaning introduces a key element of uncertainty, of lack of assertiveness on the part of the speaker, a hallmark of the subjunctive in Spanish (see Lunn, 1989 for important discussion).⁴ On the other hand, *cuando* on its purely temporal ‘when’

4. A reviewer points out that “the closest correspondent of *if* in Spanish (*si*) takes the indicative in future-oriented conditionals like *si vienes, verás* ‘if you come, you will see’ (cf., **si viengas, verás*).” The reviewer’s cautious choice of words (“the closest correspondent of *if*”) is on target: *if* and *si* are distinct in that *if* is a complementizer while *si* is arguably a phrasal element best rendered as ‘so’ (note the use of *zo* in Dutch conditionals: *zo het al werkt, werkt het traag* (lit.) so

reading becomes compatible with the indicative – though even then it remains a perfectly natural candidate for the subjunctive, given that full certainty regarding the future is not to be had.

If this is a sensible answer to the question raised above, what makes first- and second-generation speakers of Spanish in New York City different is the meaning that they assign to a particular lexical item – which has grammatical consequences, but is not in and of itself a grammatical change. The grammar of the subjunctive is acquired perfectly well, by first- and second-generation speakers alike – in fact, it is precisely their knowledge of the subjunctive that leads second-generationers to still use it in three quarters of all futurate *cuando* clauses. The fact that, in *cuando* clauses, the indicative is an option for second-generation speakers but generally not for those who were raised in a monolingual Spanish-speaking environment is a function of a simple shift in the meaning assigned to the word *cuando* – a shift that may well have been influenced by the fact that the English translation equivalent of *cuando*, viz., *when*, lacks the conditional component that, if what I said in the previous paragraph is on target, is a key ingredient of the meaning of *cuando*.

Let me emphasize again that on this diagnosis of the drop in subjunctive use in futurate *cuando* clauses among second-generation immigrants, it does not instantiate ‘incomplete acquisition’ of the *grammar* of the subjunctive: indeed, it seems that the grammar of the subjunctive is perfectly intact for *cuando* clauses. The association of the word *cuando* with the lexical meaning ‘when’ instead of ‘if and when’ itself is not a case of ‘incomplete acquisition’ either: *cuando* ‘when’ is not an incomplete version of *cuando* ‘if and when’; it is just a different word. Lexical meanings are fluid, especially for content words (and it obviously makes no sense to treat *cool* meaning ‘hip’ or ‘great’ as an ‘incomplete acquisition’ of *cool* meaning

it all works it slowly, i.e., if it works at all, it works slowly’). The complementizer *if* and the conditional operator in its specifier conspire to license the subjunctive (*if it be your will*); but futurate conditionals with *si* contain no C_{if} or conditional operator: neither *si* ‘so’ in SpecCP nor the silent C with which it combines is in itself a subjunctive licenser. Gregory and Lunn (2012: p. 335) characterize the distribution of the subjunctive after conditional *si* in Spanish along the following lines, couched in the pragmatic terms of the idea that it is the speaker’s evaluation of information quality that determines the indicative/subjunctive choice: “When it is possible to assert an if–then relationship between the clauses (e.g., *Si tengo tiempo, te llamaré*), the indicative is used. When, in contrast, the hypothetical situation is known to be inoperative, the past subjunctive is used (e.g., *Si tuviera tiempo, te llamaría*).” Speaker evaluation of the information quality of the embedded proposition also seems key in the widespread use in Latin America of the present subjunctive after *no sé si* ‘I don’t know if’ (Butt & Benjamin, 2011: p. 262). (Thanks to the editors for drawing my attention to this.)

'somewhat cold'); but even for function words such as *cuando*, what they lexicalize is not pre-ordained or set in stone. Nor is Spanish *cuando* the only word of its kind for which we see a change in lexical meaning. For words meaning 'when,' 'if and when,' and 'if,' we find quite a bit of lexical variation in Germanic. Thus, German *als* and Dutch *als* are morphophonologically the same, yet the former is temporal and the latter conditional. And German *wenn* is a cognate of English *when*, but the latter is not used conditionally.

As a companion to the case of Spanish *cuando*, consider the change in contemporary Dutch in the lexical meaning of *wanneer* 'when' from purely temporal in the normative standard to the near-equivalent of conditional *als* 'if,' in examples such as the following:

- (3) a. *ik zou het fijn vinden {als/wanneer} je dat zou doen*
 I would it nice find if/when you that would do
 'I would find it nice if you did that'.
- b. *ik zou het fijn vinden {als/wanneer} je daarmee ophield*
 I would it nice find if/when you therewith stopped
 'I would find it nice if you stopped doing that'.
- c. *ik zou het fijn vinden {als/wanneer} je me de*
 I would it nice find if/when you me the
waarheid vertelt
 truth tell
 'I would find it nice if you tell me the truth'.

While all three sentences in (3) are perfect with *als*, in my own Dutch I observe a gradient picture for *wanneer*: when *wanneer* is used, (3a) with *zou* 'would' (the closest thing there is in modern Dutch to a subjunctive) in the subordinate clause is best, simple past (3b) okay, and present tense (3c) rather odd (though sentences of this type are certainly attested with *wanneer*). So *wanneer* used in non-temporal, purely conditional contexts to my ear still tends to want the clause to 'compensate' for the fact that *wanneer* standardly means just 'when' by expressing irrealis on the verb (via the modal *zou* 'would' or the simple past).

Dutch *wanneer* is always fine in temporal adverbial clauses, meaning just 'when.' It is also usable in the standard language with the meaning 'if and when.' And what we see in (3) is that it is now encroaching on the territory of purely conditional *als* 'if.' For Spanish *cuando*, I have postulated a shift in the lexical meaning from 'if and when' to 'when.' Contact with English, whose *when* lacks the conditional component I have ascribed to monolingual *cuando*, is likely a factor in this shift in meaning. Both lexical shifts are subtle. But in the Spanish case, its grammatical consequences for the distribution of the subjunctive are significant.

That the shift in lexical meaning for *cuando* has this distributional effect is a direct consequence of the grammar of the Spanish subjunctive – itself unchanged and completely acquired.⁵

4. The subjunctive in finite clauses embedded under causative *hacer*

Bookhamer (2013) observes that the only context in which monolingual speakers and first- and second-generation heritage speakers of Spanish all behave alike in their categorical use of the subjunctive is the *hacer*-causative construction: when causative *hacer* takes a finite complement clause, it must systematically have a verb in the subjunctive.

- (4) a. *los hacemos venir*
 them make.1PL come.INFIN
 ‘we make them come’.
- b. *hacemos que vengan*
 make.1PL that come.3PL.SUBJUNC
- c. **hacemos que vienen*
 make.1PL that come.3PL.INDIC

The use of the subjunctive in (4b) can plausibly be attributed to the coercive nature of the *hacer*-causative, and the concomitantly low degree of control of the subject of the embedded event over the execution of that event.⁶ In the *hacer* + infinitive

5. Montrul (2007) reports a statistically significant difference between indicatives and subjunctives in the interpretation of *cuando* clauses only for advanced heritage speakers, not for intermediate ones. I will return to this in Section 8. Montrul also finds, on the basis of the results a judgment task, that heritage speakers (whether advanced or intermediate) show no statistically significant difference between the indicative and the subjunctive in their understanding of restrictive relative clauses in intensional contexts and *de manera que* ‘such that’ clauses. This calls for an explanation – which, unfortunately, is not being offered by Montrul herself. I would encourage theory-based research on this.

6. On the importance of control in Romance causatives, see especially Huffman (1997), with particular reference to French. I should make it clear that by ‘control over the event’ I do not mean ‘volitional agency’ and therefore I am not implying that this capacity can only be ascribed to Agent arguments. (In the examples in (4), the subject of the lower clause is the argument of an unaccusative verb, hence not an Agent.) Subjects can exert a certain degree of control over events of which they are not Agents: when it is raining, one can control the extent to which one will get wet (not an agentive predicate) by wearing a raincoat and carrying an umbrella. Conversely, the volitional Agent of an event may not be (fully/autonomously) in control of the execution of that event. Thus, in a conditional such as *if you do this, I will kill you*, the killing event in the apodosis is largely under the control of the subject of the protasis (and not the volitional Agent

construction in (4a), this is especially salient: the subject of *venir* (henceforth, the causee, expressed here as an accusative clitic on the causative verb) has little to no control over the event of coming; it is the causer that is firmly in control; the causee is like a puppet. When *hacer* takes a finite clause as its complement, the causee has somewhat more control over the caused event; but in the *hacer* + subjunctive construction in (4b), the causer is still in command. The use of the indicative in the clause below *hacer* would give the causee so much control over the coming event that it would clash with the strongly coercive nature of the matrix predicate *hacer*. This is arguably why (4c) is ill-formed.

In Section 3, we saw that, likely due to contact with English, the lexical meaning of *cuando* seems to have undergone a shift in meaning among second-generation heritage speakers of Spanish in New York – a shift that asserts itself in the reduced use of the subjunctive in futurate *cuando* clauses. But there is nothing about English *make*, the translation equivalent of *hacer*, that could lead these speakers to assign a different meaning to *hacer*. Indeed, since English *make* does not take a finite complement clause at all (**we make that they leave*), it is extremely unlikely that contact with English would affect (4b). The interpretive correlate of the subjunctive in *hacer*-causatives also seems inherently transparent and stable. So the robustness of the subjunctive in *hacer*-causatives, even among second-generation immigrants, presents no surprises, and does not impinge in any way upon what was said about *cuando* clauses in the previous section.

5. Changes in the use of the subjunctive in monolingual Spanish: *dejar*

The discussion of the coercive *hacer*-causative in the previous section leads us to the question of what happens in permissive causative constructions, which in Spanish feature the verb *dejar* ‘let’. In standard monolingual Spanish, the subjunctive is once again categorical: (5c) is not well-formed.

of the apodosis) as long as the event expressed in the protasis is, too: as long as the addressee refrains from performing the event in the protasis, the apodosis will not arise. A reviewer asks what this leads us to expect regarding subjunctive use in the apodosis of Spanish conditionals. Much will likely depend on microscopic factors (incl. The question of whether the protasis and apodosis have coreferent subjects or not: compare *if you do this, you will get hurt* with the example given previously). Regarding subjunctive use in the apodosis of conditionals by Spanish heritage speakers, the fact of the matter seems complex: Bookhamer (2013: p. 79–80) notes that some researchers (Lynch, Silva-Corvalán) report a steady increase in the use of the subjunctive from first- to third-generation speakers while he himself found a decrease. I do not clearly understand the factors (potentially) at work here, and will leave the matter aside here.

- (5) a. *los dejamos venir*
 them let.1PL come.INFIN
 ‘we let them come’.
- b. *dejamos que vengan*
 let.1PL that come.3PL.SUBJUNC
- c. **dejamos que vienen*
 let.1PL that come.3PL.INDIC

I have no information about the stability of the subjunctive in the speech of first- and second-generation Hispanic immigrants in New York City. But if what I argued in the previous section is correct, it is not likely that a major shift towards the indicative will be found with *dejar*-permissives. Though in (5) the subject of the embedded event is not under any obligation to come, it is just as true for *dejar*-permissives as it is for *hacer*-causatives that the lower subject is not in control of the execution of the coming event: their license to come depends entirely on the matrix subject. With lack of control correlating with the use of the subjunctive, and with nothing about the lexical meaning of *dejar* being likely to change under the influence of contact with English (like causative *make*, permissive *let* does not take finite complement clauses at all in present-day English), the expectation is that (4c) should be firmly rejected even by second-generation speakers of Spanish in New York.

But when *dejar* is used in the imperative (informal *deja* or formal/polite *deje*) and takes a finite complement clause, the use of the subjunctive (as in (6b)) is no longer categorical. As Maldonado (2007) points out, the examples in (7) are grammatical in certain monolingual varieties of Spanish.

- (6) a. *déjame ver si lo tengo*
 let.me see.INF if CL have.1SG
 ‘allow me to see if I have it’.
- b. *deja que vea si lo tengo*
 let that see.1SG.SUBJUNC if CL have.1SG
- (7) a. %*deja que veo si lo tengo* [Castillian Spanish]
 let that see.1SG.INDIC if CL have.1SG
- b. %*déja(me) veo si lo tengo* [Mexican Spanish]
 let.me see.1SG.INDIC if CL have.1SG

What is interesting about (7) is that, unlike in the permissives in (6), the subject of *veo* is in complete control of the event, and the subject of *deja* can only wait: a natural English rendition for (7) would be ‘wait while I see if I have it’ (cf., *deja lo busco* ‘let CL search.1SG.INDIC’ and *espera a que lo busque* ‘wait for me to look for it’ which Maldonado treats as semantically on a par).

Though the ‘wait’-type reading can only be associated with *dejar* in the imperative, it is imaginable that constructions of the type in (7) will set in motion an encroachment of the indicative on the terrain currently firmly held by the subjunctive in *dejar*-permissives of the type in (5b). Whatever may happen, it is important to bear in mind that sentences of the type in (7) are found in *monolingual* Spanish varieties. I take it to be self-evident that one would not want to say that monolingual Spaniards producing (7a) or monolingual Mexicans uttering (7b) have ‘incompletely acquired’ their native language. The sentences in (7) reflect changes in the grammar of Spanish – changes resulting from analyses assigned to *deja*_{IMP} + clause that differ from the one assigned to (6b). Such differences in analysis are the signature of language change, which is part and parcel of the ordinary language acquisition process. Incompleteness does not come into the picture: if anything, the grammars of speakers producing (7a) or (7b) are ‘overcomplete’ in comparison with those lacking these constructions; they certainly are not incomplete.

What exactly the analysis of (7a) and (7b) should look like is a far from simple matter. A major question is whether in (7) the clause containing *veo* is subordinate to *deja*. The prosodic profile of these constructions, often featuring a pause after *deja* or *déjame*, suggests that it might not be (for some discussion, see Maldonado, 2007 and also some of the more interesting contributions to an on-line forum that is about sentences like (7)). Detailed research on the syntax of these constructions would be very welcome. One educated guess would be that we are dealing with clausal parataxis (cf. in this connection the grammaticality of *deja y veo*, with an overt conjunction); another would seek a connection between (7) and English hortatives with *let*’s. Let’s turn to these next.

6. English hortatives with *let*’s

For the imperative in (8b), an interpretation is available in which it is equivalent to the modal indicative in (8a): the speaker wants the referent of the subject of *let* to allow the group that (s)he is a member of to leave. On this reading of (8b), we are dealing here with a garden-variety *let*-permissive.

- (8) a. *you should let us leave*
 b. *let us leave*
 c. *let’s leave*

But there is also a reading for (8b) in which the speaker is not asking someone for permission to leave but instead is exhorting the group to which (s)he belongs to pack up and go. This hortative reading is the only one available when *us* cliticizes onto *let*, producing (8c).

The question of whether hortative (8c) involves the same subordination structure as (8b) is brought to the fore particularly by the negative versions of the hortative in (8c) – especially by the occurrence, in American English varieties, of (9c).

- (9) a. *let's not leave*
 b. *don't let's leave*
 c. *let's don't leave*

In British English, one instead finds (9b). (9b and c) are both found alongside (9a). While (9a) and (9b) are compatible in principle with a subordination analysis (with *not* inside the infinitival complement of *let* in (9a), and *don't* in (9b) heading a negative imperative of *let*), the example in (9c) is not amenable to an analysis in which *let* selects an infinitival (small) clause: *don't* is uniquely found in finite environments. This raises the question of what to do with *let's* in (9c).

Horn (1978: p. 197) identifies *let's* in (9c) “as an illocutionary force marker rather than as an autonomous verb + object.” Weir (2013: p. 281) puts some flesh around the bones of Horn’s suggestion by venturing the hypothesis that *let's* in (9c) might be the exponent of some part of the Jussive phrase that defines the imperative (Zanutini, 2008). Since there can be little doubt that *let's* in (9c) is a head-level unit, it is possible in principle to treat it as the spell-out of the Jussive head itself; but *let's* qua head could also project a phrase occupying the specifier position of the Jussive phrase.

- (10) a. [_{JP} J = *let's* [_{IP} *don't leave*]]
 b. [_{JP} [_{XP} *let's*] [_{J'} J = \emptyset [_{IP} *don't leave*]]]

The rudimentary structures in (10a) and (10b) are two alternative ways of translating Horn’s and Weir’s remarks structurally. (I am obviously leaving a lot of details aside, including the question of how to represent the subject of the imperative. These details are of no concern to us in the present context.) Whether we choose (10a) or (10b), the head of the imperative IP is *don't*, not *let's*; the latter stands outside the clausal core, finding itself in the left periphery of the construction.

The fact that *let's* in (10) stands outside the clausal core and is not the ‘ordinary’ V = *let* (with something glued onto it) suggests that the hortative in (9c) is the result of an analysis of the string in (8c) that makes it very different from the string in (8b). Speakers not accepting (9c) may, for all we know, analyze hortative *let* in the same structural way as permissive *let* in (8b). But speakers who have (9c) have reanalyzed *let's* as something else – a high functional head, or the phrasal occupant of the specifier position of that functional head.

As in the case of Spanish (7), this change took place within a monolingual community of speakers, and represents a perfectly pedestrian case of language change as a result of the assignment, by a subset of the members of the community, of an

analysis to a given string that differs in one or more structural ways from another analysis compatible with that string. Those members of the monolingual community who treat *let's* “as an illocutionary force marker rather than as an autonomous verb + object” (to quote Horn, 1978: p. 197 again) have not acquired their native language in an ‘incomplete’ manner. Again, the notion of ‘incomplete acquisition’ makes no sense.

7. On the acquisition of subject–auxiliary inversion in *wh*-questions

So far we have looked at ‘deviations from the norm’ seen in the language of heritage speakers and of monolingual adults. Such deviations (or ‘errors,’ as they are customarily called) are also common in learners – children acquiring their native language, or adults learning a second/foreign language. Against the background of the discussion of ‘incomplete acquisition’ in the preceding sections, I will briefly look now at one area giving rise to pervasive ‘errors in acquisition’: English *wh*-questions and subject–auxiliary inversion.

Standard adult English root *wh*-questions introduced by an operator other than their subject systematically call for the placement of a finite auxiliary in the second structural position of the sentence, right after the initial (non-subject) *wh*-constituent. The standard derivation of this word order in generative syntax is via an operation called subject–auxiliary inversion (I-to-C movement). In the absence of a modal or finite aspectual auxiliary, the dummy *do* is called upon for this purpose.

- (11) a. *what will you eat?* *what did you eat?*
 b. *when will you eat?* *when did you eat?*
 c. *where will you eat?* *where did you eat?*
 d. *how will you eat?* *how did you eat?*
 e. *who will eat?* **who did eat?*
 e'. *who ate?*

It is a well-documented fact in the literature (in fact, one of the first acquisition studies in the generative framework was precisely on this topic: Klima & Bellugi, 1966) that learners of English produce root *wh*-questions in which the *wh*-expression is not immediately followed by a finite auxiliary. In other words, learners of English produce ‘non-target-like’ word-order patterns in root *wh*-questions, underperforming on subject–auxiliary inversion. (Yes/no-questions typically give rise to significantly higher inversion rates than *wh*-questions; so the problem here seems tied specifically to the ‘Verb Second’ pattern of the latter.) This is true both of children learning English as their first language and of adult second-language learners of English.

In Pozzan (2011), the trouble with subject–aux inversion in root *wh*-questions was confirmed on the basis of a multi-pronged systematic study using (for the first time) the same experimental protocols for both first- and second-language learners of English (with the second-language cohort including native speakers of Spanish and Mandarin Chinese). When she put them under the microscope, she discovered that the types of non-target-like behavior observed in L1 and L2 acquisition of English root *wh*-questions are similar but not identical. Children acquiring English as their first language produce lower subject–aux inversion rates in root questions with non-argumental *wh*-words (*where*, *when*, *how*, *why*) than in those featuring argumental ones (an effect reported in much of the earlier literature as well; see the references in Pozzan, 2011 for details). Learners of English as a second language make the split in a slightly different spot: their inversion rates are lower with *why* than with other *wh*-words. Pozzan notes that in her study this difference might, to some extent, be an artifact of the specific *wh*-words used in the experiments. But a ‘*why*-effect’ has been reported frequently in the literature on L2-learners’ acquisition of English *wh*-questions, so there is good reason to take it to be real.

The fact that *why* behaves differently from other *wh*-expressions (arguments and non-arguments alike) with respect to the propensity of I–to–C movement could, for Spanish L2-learners of English, be chalked up as an L1 transfer effect: Spanish allows ‘*why*’-questions to forgo inversion (V–S order) quite freely (Baauw, 1998), more so than any other *wh*-question type. But Chinese neither fronts any of its *wh*-phrases nor performs I–to–C movement in any *wh*-question; so the difference in subject–aux inversion behavior of *why vis-à-vis* other *wh*-phrases in the English of native speakers of Chinese cannot be straightforwardly accounted for in terms of transfer.

In their study of acquisition of English by children bilingual in English and Cantonese, Yip and Matthews (2007) note that once *why* appears (soon after the age of 3), root *why*-questions start emerging in which *why* is followed by the subject rather than the finite auxiliary – the typical pattern, as we have seen. But Yip & Matthews find a decidedly non-English pattern at the immediately preceding acquisitional stage: a pattern in which, instead of *why*, the bilingual child produces *what are doing* – as in *what are doing hurt?* ‘*why* are you hurting me?’ or *what are doing go up?* ‘*why* go up?’ (both produced at age 2;10;18). Yip and Matthews (p. 112) write that the phrase *what are doing* “is evidently calqued on the Cantonese equivalent,” which literally corresponds to ‘do what,’ and they point to a parallel with the form of ‘*why*’-questions in Igbo and Yoruba and in creoles with a West-African substrate, where the equivalent of ‘what it makes’ is used for this purpose (*wa-mek* ‘what makes’ in the English-based Guyanese and Jamaican creoles, *kife* ‘qui fait, who/what makes’ in French-based ones). This parallel may be plausible. But to me these patterns suggest a deeper truth about ‘*why*’-questions cross-linguistically, one

that will serve us very well in understanding the high propensity for such questions to forgo subject–aux inversion in L2 acquisition: the fact that 'why'-questions are readily posed in a *biclausal* form.

This can also be observed in adult standard English: while *wh*-questions based on *it*-clefts have a decidedly marked information structure (requiring exhaustification) for all other *wh*-expressions, with *why* the use of an *it*-cleft is perfectly natural and neutral. Thus, while *when/where/how is it that they say this?* may be usable in specific contexts but is otherwise quite awkward, there is nothing special about *why is it that they say this?*. Imagine now that the learner of English is aware of the liberal occurrence of *why is it that S?*-type questions – biclausal structures, with subject–aux inversion in the copular clause but not in the clause following *that*. Imagine next that, because of their meaninglessness, the copula *is*, the 'expletive' *it*, and the complementizer *that* show a tendency to remain unpronounced, especially under duress. With the substrings *is it that* not spelled out, we get (for our earlier example) *why they say this?*, which is precisely the pattern that sets *why*-questions apart from other root *wh*-questions in the behavior of L2-learners of English. Viewed this way, then, *why they say this?* represents a 'covert' biclausal structure, with *say* in a subordinate clause. The absence of *do*-support is now entirely expected: the inflectional head of a subordinate clause does not invert with the subject of that clause, so the dummy *do* is not called for.

To be sure, there is something about the biclausal syntax of *why they say this?* that is not fully on target: perfect production of this syntax would have delivered *is*, *it*, and (at least optionally) the complementizer *that* as well. But though not completely pronounced, the syntax of this biclausal structure as such is not incomplete. And L2-learners, regardless of whether their first language does or does not feature biclausal 'why'-questions, have clearly registered the fact that in English the use of an *it*-cleft for the formation of a *wh*-question is natural only for *why* and not for any of the other *wh*-expressions of the language. We are not dealing here with 'incomplete acquisition': the learners' behavior suggests a very sophisticated degree of mastery of English syntax.

Let us turn now to the argument/non-argument distinction exhibited by L1 learners in the distribution of subject–aux inversion in their root *wh*-questions.⁷

7. It should be pointed out that Pozzan's L1-study, confined as it was to the argumental *wh*-expressions *what* and *which* and the non-argument *wh*'s *when* and *why*, did not investigate the behavior of the manner adverbial *how* relative to other non-argumental *wh*-expressions. If her interpretation of the L1-acquisition facts is on target, a follow-up experiment will be needed to verify whether *how* behaves differently from *when* and *why*: one would expect so, given that in neutral sentences, placement of a manner adverbial in sentence-initial position is much less easy than similar placement of temporal, locative, or rationale adverbials.

The account suggested by Pozzan (2011: p. 318) runs as follows. Adverbial expressions, in their non-*wh* guises, can easily occur in IP-initial position in an English neutral sentence (i.e., without any special information-structural effect, such as a topic or focus reading). This makes it possible in principle for non-argument *wh*-questions to be structured entirely analogously to their non-*wh* counterparts: as IPs with an adverbial in initial position. The requirement that a [+WH] expression in an English *wh*-question must take scope over the clause as a whole can straightforwardly be satisfied by placement of an adverbial *wh*-phrase in an IP-adjoined position – and putting the *wh*-adverbial there provides a particularly economical way of constructing the question, without recourse to the CP layer of the structure, and, concomitantly, without the need for I-to-C movement ever arising.

Viewed this way, children's behavior with regard to subject-auxiliary inversion is the reflex of the complete acquisition of a system of *wh*-question formation in which it is of paramount importance (just as in the grammar of adult standard English) that the *wh*-expression be placed in a position in which it scopes over the entire sentence, but in which the scopal position of the *wh*-expression is not (necessarily) identified as SpecCP: an IP-adjoined position will work perfectly well for non-argument *wh*'s, given that it can be independently verified that an IP-adjoined position is generally admissible in neutral declarative sentences for temporal, locative, and rationale adverbials.

Children acquiring English as their native language will eventually converge on a grammar for (non-subject) root *wh*-questions in which the *wh*-expression is systematically in SpecCP and I must raise to C: adult standard English has *wh*-expressions which take their scope from SpecCP, in root and non-root questions alike, and in root *wh*-questions, the language insists (in a nod to its history) on a 'Verb Second' pattern which has otherwise fallen out of grace. But for some time, children work with a Universal Grammar-compliant grammar in which *wh*-expressions take scope over the entire clause by being in a position commanding the clause, not necessarily in SpecCP. This grammar is not an 'incompletely acquired' adult grammar. Children at the relevant acquisitional stage clearly have acquired CP. They use it accurately in the formation of argument *wh*-questions as well as for subordinate *wh*-questions. But for root non-argument *wh*-questions, they initially resort to an IP-based analysis that is just as good as – and structurally and derivationally more economical than – the 'CP across the board' approach. The realization that it is not the acquisition target is something that more attention to the input will eventually instill.

So Pozzan (2011: p. 345) is right when she concludes that "difficulties with word order in English main questions are best characterized in terms of differences in representation, rather than differences in implementation of target procedures."

As in the case studies in the previous sections of this paper, what we see is not 'incomplete acquisition' but acquisition of a grammatical pattern different from that of the monolingual adult standard, and not deriving directly from the input.

8. Closing remarks on 'incomplete acquisition'

Both Bookhamer (2013: p. 108) and Otheguy (2016) reject the notion of 'incomplete acquisition' because (in Bookhamer's words) it "alludes to the idea that there somehow exists a complete grammar *de facto*; a conception framed by an ideological projection of what embodies completeness." Bookhamer prefers the term 'fragmented grammar,' asserting that this improves on 'incomplete acquisition' because it "indicates possible underdevelopment in certain syntactic areas of the mood grammar" (p. 113). The improvement here does not strike me as real, quite frankly – nor do I think that the problem with the notion of 'incomplete acquisition' lies there where Bookhamer and Otheguy locate it (i.e., in the "ideological projection of what embodies completeness"). To my mind, what is wrong with the terms 'incomplete acquisition' and 'fragmented grammar' alike is that they misconstrue what 'acquisition' and 'grammar' mean.

Let me clarify this against the background of Montrul's (2009, pp. 241–42) description of what she means by 'incomplete acquisition': "[w]hen input to one of the languages in bilingual children is not sufficiently rich and abundant during the period of language development, a language runs the risk of not reaching its full potential." What does it mean to say that a language does not reach its full potential? Or, asking the question in a somewhat more effective way, what could the product of 'incomplete acquisition' be? Would it be an 'incomplete grammar'? There can be no such thing. We can think of a grammar as a machine that generates and processes grammatical linguistic expressions. A machine with one or more cogwheels missing is not just incomplete: it is not a machine; it does not work. Of course, if the set of parts out of which one is to assemble a machine happens to contain a couple of cogwheels less than the set of parts for the machine of one's neighbor, one can still build a perfectly functioning machine. That machine will be able to do a lot of the things that the neighbor's machine can also do (and might even be more efficient at some of its tasks, and do some jobs that the neighbor's machine is not equipped to do). But it is a complete and fully functioning machine. It is different from the neighbor's machine, but it is not 'incomplete.' There are no incomplete yet functioning machines. For grammars, the same is true.

The expression 'incomplete acquisition' can only refer to an acquisition process that does not deliver a complete grammar – which is tantamount to saying that

the acquisition process fails. This is not what the users of the term ‘incomplete acquisition’ have in mind. What they usually mean is that the process that they call ‘incomplete acquisition’ by heritage speakers results in a grammar that is different from that of monolingual speakers, but a well-formed and fully functioning machine that has a systematicity to it that one would expect from a representative of Universal Grammar (see Polinsky, 2008 on the point of systematicity). But that is just *normal* acquisition: this is how language variation and language change come about. For a fully functioning heritage-speaker grammar, the predicate ‘incomplete’ should not come into the picture.

Now, there is an aspect of heritage languages which suggests that the well-oiled machine may at some point start showing signs of decay, and might founder. Silva-Corvalán (2003) presents the results of a longitudinal study of two children growing up in an English-speaking home, from age 2;10 to age 5;6, and finds that these Spanish heritage speakers’ tense-mood-aspect system was more reduced at age 5;6 than at around age 3, at which point they still had the present subjunctive. This is language attrition, which may ultimately lead to language loss. Advanced heritage speakers who use and hear their native language frequently naturally stand a better chance of keeping the machine running trouble-free than heritage speakers who have much less exposure to and opportunity to use their native language. Just like what was at some point a well-functioning machine may, through disuse, stop running smoothly and fall apart, what *was* at some point a well-functioning complete grammar (though not necessarily fully identical with the grammar of the environment) may *become* incomplete and faulty. But that would not be an example of ‘incomplete acquisition’ – instead, it would instantiate emerging incompleteness as a result of attrition, which is quite a different matter.⁸

I do not disagree with one of my reviewers that “[l]anguage change in monolingual varieties may be different from language change in bilingual varieties. That is, they can happen for different reasons.” In my presentation I juxtaposed the two instances of language change for cases in which they arguably can and should be juxtaposed. Doubtless much more is going on than what I have had occasion to talk about. What I have argued in this short paper is that both in a monolingual speech community and in a multilingual setting with lots of language contact, language change is a natural product of the human language acquisition process. A product different from the ‘target’ is acquired, one that conforms to the options provided by

8. Montrul (2009) also distinguishes between loss and what she gives the epithet ‘incomplete acquisition.’ As Naomi Shin is right to point out, it may turn out that language attrition and loss actually proceed through stages that each by themselves represent complete grammars, so that ‘incompleteness’ would not come into the picture even here.

Universal Grammar, not one that is incomplete. The term 'incomplete acquisition' misrepresents what we find in the patterns of language acquisition and language change. This term should have no place in the linguistic discourse.

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Across the world, professional linguistic inquiry is in full bloom, largely as result of pioneering thinkers who helped rapidly modernize the study of human language in the last century. As the field continues to move forward, further solidifying its position as a conduit of insight into the human condition, it is essential to take stock of the theoretical primitives that have given linguistics its intellectual foundation. This volume does precisely that, inspecting the load-bearing components of the edifice upon which contemporary linguistics has been constructed. The volume's authors – whose expertise spans the Generativist, Functionalist, and Variationist research traditions – remind us of the need to revisit the conceptual bedrock of the field, clarifying and assessing our primary theoretical moves, including those relating to such elemental components as the 'linguistic sign', 'a language', 'structural relations', 'grammatical category', 'acquisition', 'bilingual', 'competence', and 'sociolinguistic variable'.

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