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Expanding the Uniformitarian Principle: A New Model for Diachronic Analysis

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Expanding the Uniformitarian Principle: A New Model for Diachronic Analysis

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ABSTRACT

Diachronic linguistics has long been a field of competing and contradictory claims, most recently with source-based theories and lineage-specific generalizations opposed to explanations for change based on more universal principles, and with various single-factor theories often staking out uncompromising positions that demand discarding research done in other areas of the field. While this is not new, and bold positions have been held at all points in the past of historical linguistics and other diachronic disciplines within linguistics, diachronic linguistics has a unique benefit compared to other subdisciplines of linguistics. It sits at the nexus of many, or even most fields of linguistics. Linguistic typology, historical linguistics, sociolinguistics, and numerous other fields all contribute to this discipline. The problem to date has been how best to capitalize on this advantage. It should be possible to bring the research of all these interconnected disciplines together to speak to diachronic issues rather than leave estranged the findings of each from the others.

Historical linguistics has traditionally operated under an assumption that the languages of today operate under the same rules and limitations as the languages of the past. Known as the Uniformitarian Principle, this premise has been relatively uncontroversial in historical linguistics and linguistic typology but has been underutilized in the field. I will argue here that if we systematically exploit the consequences of the Uniformitarian Principle to their fullest, it presents the answer to the problem laid out above. The Uniformitarian Principle can be leveraged to allow synchronic and diachronic research to speak to the same questions. With this in mind, the full heft of synchronic linguistics can apply across diachronic studies — research into efficiency or typology for example can naturally be expected to have findings applicable to

understanding how languages change. Just as importantly, the Uniformitarian Principle implies that the findings made within the whole scope of linguistics are by their very nature generalizable to language change.

In this dissertation, I will lay out how, by fully leveraging the Uniformitarian Principle, a comprehensive model of language change can be constructed, where synchronic and diachronic data are all treated equally. I will offer a critique of the broad state of the field of diachronic linguistics, in the manner of a philosophy of science analysis. In doing so, I formulate a novel typology of linguistic research as it applies to diachronic analysis and demonstrate how this new understanding can be leveraged to break down conflicts in the field, and in doing so develop a new multi-factor model for understanding language change as a complex system reacting to differential pressures. Where pressures for change align or are unopposed, we should be able to predict such changes in the system in order to alleviate the relevant pressures and bring the system into alignment with them. Where pressures conflict with each other, we predict synchronic variation in general and either no change from the current language state or change into an alternative state within the range of synchronic variants predicted by the opposing forces. This more complex modeling approach allows us to avoid overly reducing the scope of the problem and to acknowledge that the individual strands of research already conducted on language and language change are not necessarily wrong, but rather waiting to be joined with one another and combined into a bigger picture analysis. This advances diachronic analysis beyond the era of single-factor modeling and allows a more cohesive manner of integrating findings from the many disciplines of linguistics.

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GLOSSING CONVENTIONS

1	1st person
2	2nd person
3	3rd person
ACC	accusative
AOR	aorist
ADP	adposition
ASSOC	associative
CNC	concord marker
DAT	dative
DEF	definitive
GEN	genitive
INSTR	instrumental
LOC	locative
NOM	nominative
OBJ	object
PAST	past tense
PL	plural
POSS	possessive
PRS	present
PRT	particle
SG	singular

CHAPTER 1

INTRODUCTION

The Study of Language Change and Addressing the Schisms

1.1 The Challenge of Language Change as a Field of Study

The causes of language change have been a difficult topic for the field of linguistics. We have the problem that if language change has a causal force, the simple existence of that force should in theory make the change predictable. Causal factors are not hard to postulate, and some researchers have cast about for explanations in a sort of wild scattershot without much discrimination between them. Take, for example, Archibald Henry Sayce's list of attributive causes for Grimm's Law:

> "These phonetic changes have, it is true, been brought about by the influence of climate, food, laziness or the reverse, analogy, and fashion" (Sayce 1874 as cited in Campbell 2013, p322)

This kind of summary is an acceptance that we cannot identify the root causes of change. Sayce was a prominent scholar in the late 19th century (as can be seen in his contributions to history and philology to the Encyclopedia Britannica), not one to be easily dismissed out of hand (Important contributors, n.d.). He was writing in an era where the early triumphs of historical linguistics such as Grimm's Law (Grimm 1822) and Rask's Law (Rask 1818) were still fresh, and the premise of evolution in biology (cf. Darwin 1859) was seeping over to other fields, linguistics included (cf. Jastrow 1886). Sayce's listing of causes so disparate and uncertain grants the possible existence of causal factors in

language change, but at the same time functions as a concession that we truly have no understanding of what they might be.

Sayce at his core, despite writing well over a century ago though, is not much of an outlier when compared to the corpus of modern linguistics. The possible explanations as presented above are comical when viewed through the lens of today's scholarship, however the weight of his statement is not the causes, but rather the lack of ability to discern anything between them. Leonard Bloomfield, writing about phonological change in 1933, claimed

> "Although many sound changes shorten linguistic forms, simplify the phonetic system, or in some other way lessen the labor of utterance, yet no student has succeeded in establishing a correlation between sound change and any antecedent phenomenon: the causes of sound change are unknown" (as cited in Labov 1986, p524).

In 1958, Martin Joos stridently claimed that it is "perverse and childish to demand explanation" of language change when we have been so fortunate as to succeed in identifying the details of the changes (cited in Campbell 2013, p322). Lyle Campbell further describes the common position in historical linguistics that change at its core has too many possible causal interactions to be able to predict or explain change meaningfully (2013, p333).

This perspective on change as fundamentally unpredictable (whether formulated explicitly or implicitly) has been a persistent presence in the field for well over a century. These assessments on the causal and predictable nature of language change are dangerous in that they risk denying the functional pressures upon which such change might be

predicated. Furthermore, it can serve to entrench an ascientific approach to language research where a problem seems so inscrutable as to not be worth testing or investigating. Even as recently as 2002, writing specifically about the evolution of creoles, Sarah Thomason was forced to advocate for the radical position that the hypotheses of language change should be tested and validated against actual languages and available data (Thomason 2002). For the study of language change to progress forward, the reaction to a complex problem needs to be a willful leap into the breach and engaging with that problem with an assumption that greater understanding can be achieved.

1.2 Harmonizing Schisms in the Field

The previous section paints a grim portrait of the field of linguistics as a whole, but it would be unfair to leave it so short. As much as the field of linguistics has struggled with the mere concept of causal forces in language change and the predictions which they might enable, it has struggled even more with schisms in the literature. And when some linguists have attempted to bridge these divides, too often those efforts go unheeded or are swept up in greater pugilistic struggles. Frederick Newmeyer published an entire monograph focused solely on addressing and hopefully mending one such schism — that of the generativists and functionalists (Newmeyer 2000). I cannot quantify the effect that his work had in bridging the opposing camps, but even the superficial metric of the conversations I have overheard at conferences would suggest that this divide still exists. Furthermore, these schisms are prone to creating supposed divides even within mostly aligned camps. Looking again at Newmeyer's work, in his discussion of functionalism and formalism, he classified Tony Kroch as functionalist.

When I had the occasion to speak to Tony in 2017, he instead described how his efforts to study historical linguistics were shunned in the generative circles within which he swam because one could not field grammaticality judgements from native speakers when a language was no longer spoken. This sort of limitation serves to be a dogmatic reason to object to one's classification within a certain group (in this case, the generativists) but does not seem to be a distinction with great utility in furthering understanding — I would argue that a review of Kroch's work would suggest that he tends to work within the theoretical framework of generative approaches to syntax. It is important to understand however that encroachments such as this of ideological academic squabbles into a work of reconciliation are not indicators of a failed effort or of poor work. They are guaranteed to happen whenever an author writes with the intention to bring disagreeing voices into harmony because those involved often also disagree on which subjects have disagreement. Newmeyer's work was a valiant effort — the sides he sought to bring together were as far apart as the field has ever seen; indeed, he cites Johanna Nichols approvingly when she claims that what distinguishes functionalism from formalism was the focus on evaluating the complete communication phenomenon rather than narrowly focusing only on an ideal of language structure absent context (Newmeyer 2000; Nichols 1984 as cited in Newmeyer 2000, p11). Newmeyer's warring factions disagreed on everything from how to analyze language, to what an analysis was, to what was covered within the concept of "language". When the field is split like this, entire academic careers are prone to being wasted in avoidable darkness when collaborators or parallel inquiries are available and could be drawn on but are never discovered.

My own goal in this work is to tread in the footsteps of Newmeyer and others like him who have worked to rebuild shattered bridges and mediate renewed discussion of on complex issues across the dividing lines of the field. I confront a slightly different problem from Newmeyer though: while some scholars of language change are aware of each other and vehemently disagree with what they believe the other to be saying just like the generativists and functionalists of Newmeyer's work, there is also a greater problem of many scholars not even being aware of work that is relevant to their own inquiries of study. This leads to two schisms: one represented by antagonist factions and one represented by silence. The analysis and theory of language change is fraught with many schisms of both types. I have on occasion described my work as modern-day scholasticism, hearkening back to the old educational process of early medieval theology where students had to search for authority in the works of previous significant authors and theorists (cf. Pieper 2020). While I have always said this as a tongue-in-cheek joke, it is not wholly inaccurate. Indeed, the premise of this manuscript is that there are many researchers making valuable contributions to the totality of knowledge on language change or which are somehow relevant to language change, but a plethora of small schisms, either silent or hostile, have stood in the way of a broader understanding. In this work, I seek to call attention to how these schisms can be bridged and in doing so bring the field as a whole closer to examining and understanding causal forces in language change. Unlike the early scholastic method based in theology, I will not hold these positions infallible, but rather I will work to bring them in harmony by showing how the larger picture can be built by combining allegedly conflicting work and findings. This outcome of understanding how and why languages change is not a simple one — as will

be clear in the body of this work, it is not a small field and could reasonably be said to encompass all that the study of linguistics represents, perhaps with a number of adjacent fields added in. In hubris or triumph, this could be compared to searching for a theory of everything, and that rightfully has been likened to questing for the holy grail (McMahon 2003). My hope is that I avoid the follies of this noble quest but manage to lay the groundwork for a broader theoretical approach of the big picture by composing a forest from the trees of valuable work which we already have available to bring to bear.

1.3 The Uniformitarianism Principle

Harmonizing the theories and approaches of language change towards one more cohesive approach is not a simple objective and this goal cannot be accomplished from a perfectly theory-agnostic position. Most of the positions I take will not assume a specific theory, approach, or maxim to be correct, but there is one exception which is fundamental to all diachronic study of language. This exception is typically referred to as the Uniformitarianism Principle and will be axiomatic for the entirety of this work.

The Uniformitarianism Principle is the postulate that that languages of the past and languages of the present are not meaningfully different from each other — that is, that the qualities, features, and restrictions of languages in the present and the past are the same (i.a. Roberts 2017). This principle is a fundamental one for the study of historical linguistics, it is assumed implicitly in the field as a whole and sometimes it is formulated explicitly, but its application in historical linguistics has not in fact always been consistent, and its application to *explanations* of language change requires us to refine the expectations of what the uniformitarian principle must signify.

The Uniformitarianism Principle forces us to evaluate historical languages (so long as the time depths considered are not so extreme as to demand accounting for the evolution of humans as a species) on equal terms with the languages of today. In the early days of historical linguistics, even while breakthroughs were being made on the analysis of Proto-Germanic and Proto Indo-European, the causes proposed for historical language change were disconnected from what might be proposed for forces acting on contemporary speakers. For example, we can look back again to the previously referenced comments by Archibald Sayce in 1874 on Grimm's Law: "these phonetic changes have, it is true, been brought about by the influence of climate, food, laziness or the reverse, analogy, and fashion" (Sayce 1874 as cited in Campbell 2013, p322). When uniformitarianism stipulates that languages in the past and present are not meaningfully different, that requires an acceptance that the forces on languages then and now must in principle be the same. For any modern researcher to propose a motivation for a change in language historically, they must be ready for that same motivation to apply to languages in the present. This does *not* mean however that all languages and dialects are always experiencing identical pressures and will respond in identical ways (i.a. Greenberg 1978). To propose this would lead to the problem where all languages might be conceived of as manifesting universal pressures and identical responses to those pressures (such as described and critiqued in Weinreich, Labov, and Herzog 1968). Weinreich, Labov, and Herzog describe a paradox in linguistics as seen by Ferdinand de Saussure, Hermann Paul, and others since then as being forced to choose between the synchronic or diachronic study of language, because theories for how languages work today would create problems if applied diachronically. They frame this in particular as being a natural

extension of Paul's and other 19th century linguists' search for commonalities and universals in languages. The problem was that a psychological motivation for language, if true, would beg the question of why all languages did not respond the same way over time. Thus, divergent changes in language evolution would have to be interpreted as proof of a lack of uniformitarianism and as an example of that synchronic-diachronic split in the field laid out by Weinreich, Labov and Herzog.

The real issue here is not a problem with uniformitarianism, but rather a misapplication of typological principles to the matter. Uniformitarianism, when applied diachronically (rather than in independent synchronic snapshots), is not an assertion that all language change follows the same universals in the sense of there being a single common template for change, but rather it is a claim that the forces which can act on languages and the forms of responses that languages can have to those forces will be the same throughout the history of human language and regardless of language family (while not originally addressed to uniformitarianism, this idea was already laid out in Greenberg 1978). I would suggest that another reasonable way to think of this (in much simplified terms) is as a hill with a low-lying creek on the left side and a higher meadow on the right. With gravity being the source of pressure on the system, a ball starting at slightly different positions on the top of the hill might roll just a short distance to the right and not lose much elevation, but a second ball could tumble down the left and into the creek bed far below. A single universal system was at play, but the two balls ended at different elevations and different distances from their starting points. As we analyze language change, there will be many such disparate outcomes, but they do not imply an

ungeneralizable system, but rather one which requires a greater understanding from which we can generalize properly.

The Uniformitarian Principle actually allows us to draw an important corollary, and the application of this corollary will be one of the main contributions of my work herein. By starting with the assumption that languages today are like languages in the past, we are making the claim that the principles of variation today can be projected into the past. What is impossible today is impossible in the past. What we identify as a cross-linguistically preferred form or structure today should be a preferred form or structure in the past. What we can demonstrate as efficient today should assumed efficient in the past. Accordingly, we are already stating the following: First, we can say that rare forms in language today should be rare historically. If synchronic typology identifies certain features as being apparently dispreferred, those features should not be expected to be common in historical languages, whether documented or reconstructed. That those features *may* be discovered in historical languages remains true, but each discovery of a rare feature is inherently unexpected in the way it would be unexpected in a language documented today. The second thing we can say is that rare changes today should be rare in the past. These two points imply at least one of the following, and I would suggest that both are likely to be true: either changes which yield rare forms should be rare themselves, and/or that rare language forms should be diachronically unstable.

Diachronic support for this conformity between past and present, and for the Uniformitarian Principle and its general predictions, can be found in such work as that of Ilja Seržant and Dariya Rafiyenko (2021). In their analysis of Ancient Greek, Seržant and Rafiyenko (to be discussed in greater detail in Chapter 5) demonstrate the

development of harmonic head-first ordering in Postclassical Greek, despite Classical Greek showing no order preference. This fits in line with modern typological patterns of head ordering, and shows that the change in the past yielded a preferred end structure. Their analysis goes even a step further however and demonstrates that the non-harmonic orderings which were well attested in Classical Greek disappeared over time, again matching with our claim that a dispreferred form should also be diachronically unstable (prone to loss or change).

Further evidence can be seen with the relative instability of ergative languages over time relative to nominative-accusative languages, where ergative languages are far more likely to change case alignment in a given timeframe than nominative accusative languages. This is a common observation (such as commented on in Nichols 1993), but is also well supported by possible motivations in what typologists tend to see as preferred forms. Looking only at recent publications, Bickel et al. (2015) demonstrate how even when a language has ergative case alignment common in certain structures, the listener is still primed for the first mentioned and unmarked form to be the agent. This aligns well with nominative-accusative systems where a transitive subject agent appears before a patient but is not always in alignment with the ergative systems they tested in which the patients of transitive clauses often appeared before agents.

There are also considerations of logic which are relevant here. First, if changes that create rare forms out of common language forms were frequently occurring, we would expect rare forms not to be rare. Similarly, if rare language forms were *more* stable (less prone to change) than common forms, their greater persistence temporally should be reflected in an increased presence in any synchronic analysis of language

typology. This reasoning is what yields the important corollary to the Uniformitarian Principle: the synchronic typologies of languages and the diachronic typologies of language change and of the drivers of language change should be describable and explainable in the same terms. If a feature is rare synchronically, we can postulate a diachronic pressure against the development of that feature or a diachronic pressure against the maintenance of that feature. Thus, when investigating language change and the causes associated with it, we need not be beholden only to the data and findings of diachronic work. Instead, synchronic typology can be utilized and treated as directly comparable to the work done in diachrony and perhaps even interchangeable with diachronic work. This greatly expands the breadth and depth of the compiled knowledge which is available for us to drawn on in the analysis of language change.

1.4 Objectives to this Work

My core objective to this work is to try and bridge disciplines and disagreements in linguistics with the ultimate aim of providing a framework in which different fields of research can be integrated into a greater understanding of language change — I hope to lay the groundwork for an overarching and more unified theory of language change. This is fundamentally a theoretical goal rather than a more empirical or experimental one, but it should provide a general context in which empirical findings, both synchronic and diachronic, and also experimental results synchronically can be leveraged to greater effect. To achieve these aims, I will do the following: First, I will apply the Uniformitarian Principle in a novel and more aggressive way, treating synchronic and diachronic data as equally applicable to diachronic questions. The principle is already

generally accepted in historical linguistics, but the significance of its claims is not leveraged to the full extent — fuller application of the Uniformitarian Principle should result in a wholesale application of the breakthroughs of linguistic typology to language change, and this has not really happened to date. This proposed equivalence of synchronic and diachronic data in understanding language change cannot be overemphasized — it represents a substantial expansion in the application of the Uniformitarian Principle. The integration of many different studies and research findings into one narrative also entails a second claim, that the system is complex and best fit by a multi-factor model. While it is logical that a phenomenon which has supported as much research as language has without reaching a clear "solution" might be a complex multi-factor system, it is worth addressing this aspect of language more directly since some claims made about grammars and change in the literature run counter to it. A multi-factor model is one of the founding premises of the utility of this work: it seeks to demonstrate and illustrate the need for multiple factors when explaining language change and to integrate these factors where appropriate when discussing individual changes and their causes. Third, I will break down the field of linguistic description and explanation into a typology of research approaches. Different segments and scholars in the field of linguistics ask different questions and supply different types of answers, and this metatypy of research is useful to for trying to understand and mend the schisms in the field. Research can be broken down into approaches that evaluate language at the levels of the individual, society, or the structure of language itself, and each mode of research is important to our understanding of the greater whole. I will evaluate the utility and

application of each type of research and apply them to a bigger picture understanding of how languages change and evolve over time.

Lastly, in the course of addressing my main objectives, I will seek to address some of the more common single-factor approaches and evaluate their contribution to our understanding, especially focusing on the increasingly popular school of Source-Based Theory in historical linguistics. This school is a fast-growing faction in modern diachronic scholarship and is on the verge of creating more deep schisms. It also poses the question of whether language change should be considered the product of drift or of functional pressures and motivation, and any development of a unified theory of language requires addressing this question directly. Related to source-based theories are claims of "lineage-specific" effects in diachrony, as in Dunn et al. (2011), which are opposed to more general and universally-based explanations for cross-linguistic patterns and distributions, and these will also be critiqued.

In short, this work will argue for and illustrate an interdisciplinary multi-factor approach to language change which fully leverages the Uniformitarian Principle and provides a framework in which research from many disciplines can be applied to the solution of problems in language change, often to an extent beyond the bounds of the original research's goals. Furthermore, by accepting the Uniformitarian Principle, I would argue that we are necessarily brought to a multi-factor understanding of language change which incorporates typological universals and general synchronic principles rather than one in which the primary motivations for change are discussed in terms of single factors, the ungeneralizable sources of specific changes, and lineage-specific effects.

CHAPTER 2

A TRIPARTITE TYPOLOGY OF RESEARCH

2.1 A Typology of Approaches to Language Change

The present state of the field consists of a wide variety of theories and approaches. These approaches are quite heterogeneous, and this makes it difficult for us to engage with them in a principled fashion without first creating a working typology of the different theories. The theories I address will be predominantly those which directly speak to language change and diachronic effects, but where appropriate I will tie in synchronic theories which speak to typological universals in a manner applicable diachronically via the uniformitarianism principle. A detailed survey of the breadth of study in a field such as this would require several volumes, but it is possible to demonstrate the main features of the different approaches with relative brevity.

In broad terms, I will break down language change research to date as falling into three categories, with these categories defined by their theoretical methodologies and by what types of answers are proposed. This categorization of the field as I propose it does not require an actual alignment between the claims of individual researchers within each category. There can be, and indeed are, core disagreements between theoreticians working within each category. Dividing the field in this way, therefore, does not give an outline of opposing camps within the field but rather a breakdown which contributes to my goal of harmonizing between and across theories and to my attempt to integrate them into a common framework. I should mention that categorizing language change research simply according to the traditional fields of linguistics (phonology, syntax, etc.) would not be prudent in this case. Diachronic changes observed in different subdisciplines of

linguistics are very frequently linked together. The clines and patterns of grammaticalization are a classic example of this, where components of syntactic ordering, phonetic erosion, semantic shift, and morphological utility all are packaged into a single narrative (i.a. Heine et al 1991, Lehmann 1995, Hopper & Traugott 1993). A typical example of this phenomenon would be the Spanish future and conditional tenses, which are marked by inflection on the infinitive verb (for example: *comeré* and *comería* respectively for the verb *comer* "to eat"). The forms used to inflect for these two tenses both derive from periphrastic constructions with the verb *haber* much earlier in the language's history. *Haber* would occur inflected next to the verbs and over time that position was calcified, with the postposed position for this construction becoming dominant over the preposed position and then experiencing a reduction in form to the point that a modern Spanish speaker would be unlikely to identify these verb forms as including *haber* (not to mention that the construction which yielded these suffixes is no longer common) (cf. Green 1988). In this one example, we can see how the *haber* of these constructions lost its independent form, was reduced in phonetic segments, and lost its lexical semantic payload, serving now instead only to give information about the tense of the host verb. This linkage of multiple facets of ongoing changes will be relevant for our analysis of any given change: to explain one change, we can expect that there will be several components, and only by integrating them all will we be able to address the phenomenon as a whole.

Accordingly, while there are other divisions one might make, I have devised a tripartite typology of research into language change which will minimize awkward decision points when categorizing interdisciplinary research such as grammaticalization.

This typology of research is unusual in that it groups research not by its theoretical underpinnings, not by the discipline or subfield, and not by methodology, but rather by the sorts of answers and conclusions drawn. This has a number of benefits. First, it forces a reckoning with parallel research — where multiple researchers are developing similar analyses, but are not in dialogue with each other. Second, it actually paves over some of the schisms in the field where substantively similar research claims to be conflicting. Lastly, it serves to bridge disciplines to work towards a unified understanding. In sum, this typology should prove useful and sufficient for the purposes of this work and will preserve the interplay of different disciplines of linguistics in the analysis rather than risk tunnel-visioning unnecessarily on a single discipline.

2.2 General Functional Pressures at the Level of the Individual

The first category includes research focused on strict functional pressures sandboxed from outside influence. I also include here the source-based approach (advanced by those such as Sonia Cristofaro and Anthony Aristar, among others) under this header (i.a. Aristar 1991; Cristofaro & Ramat 2017; Cristofaro 2017, 2019; Collins 2019). Research within this category can be summarized as research which focuses on frequency, minimized effort, perceptual ease, cognitive load, and progressive reanalysis. For many scholars, this grouping will seem like quite an odd mix but it does not matter for the purpose of our typology here whether the researchers and their research which I group together under this umbrella agree with each other. Rather, the essential component is that their research and explanations follow similar precepts. The connecting line between research in this category is the focus on change being driven at

the level of the individual speaker, as opposed to those pressures that are external to the individual (as summarized in the next section), and by incremental and identifiable changes especially if there is some clear motivation. This criteria of clear pressures on the individual will prove essential when comparing the work within this first category to the work of the final category on structural pressures (Section 2.4). It is not enough for a construct to be objectively more efficient, the analyses categorized in this first category should show *how* the change occurs.

Most of the research covered under our first category can be exemplified by the work by Zipf (1929, 1932) and Ohala (1989). Zipf's work on word frequency and economy of form is the heart of the early descriptions of what would eventually become the extensive documentation on grammaticalization despite being only presented as a synchronic observation. Ohala's work is framed in a way specific to phonetics and phonology, but it forms a succinct and detailed breakdown of a surprisingly comprehensive variationist approach to language change. There is also a wing of the field which builds significantly upon the work of Zipf which I will address directly later in this section.

John Ohala's work focuses on the phonetic and phonological changes exhibited in languages over time. His work is significant for our discussion here as an exemplar due to the broad account which he gives of the sources of change. He frames language change as developing from synchronic variation, but with the sources for that variation being found both within the speaker and in the listener (1989). He describes the sound changes that derive from pressures on the speaker as being based on aerodynamic constraints, neuromuscular constraints, and elasto-inertial constraints. These constraints

include limitations such as the oral versus sub-glottal air pressure during voicing and the physical difficulties of greater movements of articulators when speaking quickly. In essence, this can be summarized as the physical limitations and economies of the speaker, often termed not so much as the conscious choice of the speaker, but as a natural byproduct of normal language use. Just like in the psycholinguistic literature on processing limitations and cognitive load (cf. Hawkins & Cutler 1988, Mithun 2003, Lohse et al. 2004, Hawkins 2009, etc.), Ohala is applying the anatomical and physical limitations inherent to a phenomenon in a physical space. Similarly, he extends the same line of reasoning to the listener's experience. He specifically calls attention to the confusion of similar sounds, and imperfections of transmission resulting in hypo- or hyper-correction. The listener exists in the real world and must make do with the input they receive and attempt to reconstruct the source to the best of their ability. Ohala's work here well encapsulates this group of research: there is a focus on change as an organic development from the experience of the speakers and listeners, and the changes that occur are based on the real-world impositions on the execution of language.

George Zipf is most famous for his statistical work on language, which has proven foundational for the field of diachronic linguistics. He identified the correlation of sound change with frequency of use (1929), and of word length with frequency of use, and used these correlations to make the claim that there is an economy of language — not just that the most frequent words happen to be smallest, but that the most frequent words are the smallest because of their high frequency (1949). This simple observation forms the core of the well-developed study of grammaticalization in historical linguistics and diachronic typology today. It is worth our time to delve deeper into grammaticalization

at this point. This is for two reasons. First, it is an integral part of the body of diachronic research as this time. Second, I have included in this group of approaches one in particular which has gained substantial ground in the field in recent years — the source-based (or "source-oriented") approach. This approach draws very heavily upon the study of grammaticalization and a basic overview of grammaticalization is necessary to engage with this section of the field.

Grammaticalization is the transition of a phrase, item, or element of a language to a position of greater relevance in the grammar. This can be from a purely lexical item to purely grammatical one (such as an auxiliary verb), or it can simply be one step of an item moving a small distance on the continuum of grammatical function (Campbell 2001; Heine & Kuteva 2002). These steps also do not need to be universal — in the previous example of *haber* in Spanish, it only became grammaticalized to become a suffix in those specific circumstances but otherwise remained an independent lexical entry. Similarly, we can look to English and spot examples of *want to* and *going to* reducing to *wanna* and *gonna* while seeing that these reduced forms still coexist with their unreduced forms.

Grammaticalization can thus be described as a semantic weakening and syntactic strengthening of lexemes or morphemes through progressive reanalysis (i.a. Heine et al. 1991, p3; Hopper & Traugott 1993; Lehmann 1995). Applied to a single change, this is often described as a semantic bleaching or desemanticization of the word or structure as it is grammaticalized (cf. Heine & Kuteva 2002, Dryer 2013d, Hopper & Traugott 1993, etc). This means that a lexical construct progressively loses its significance as a citation form for a lexical meaning as it takes on its increasingly grammatical role as morpho-syntactic inflection or anaphoric pronoun. The syntactic wax and semantic wane of

grammaticalization are often accompanied by phonetic erosion pushing the grammaticalized item to a simpler form (i.a. Hopper & Traugott 1993, p96; Heine & Kuteva 2002, p3-5) — consider again the example of *gonna* in English as a phonetically reduced form of *going to*. As the item takes on additional syntactic load it is also expected to phonetically reduce to a more uncumbersome form (in line with Zipf 1949). This phonetic lightening can be interpreted as a consequence of frequency: namely that as the item takes on a greater syntactic role in the system, it in turn creeps upward on the Zipfian curve of word frequency, and this higher frequency makes a large number of articulations for the item untenable to maintain (both for ease of communication and due to typical erosive patterns). Thus, one might envision the core process as a sliding scale from semantic weight to syntactic function with consequent phonetic lightening implied. This definition of grammaticalization is entirely language-internal by nature, and it is largely divorced from the precipitating causes, such as laid out in Heine (1993, p23):

> "The things that happen in grammaticalization do so in an orderly fashion which not only predicts what changes can occur but also puts constraints on what synchronic grammatical systems are found"

Note though how this description clearly constricts the set of possibilities, but leaves the why and the when for grammaticalization as unknown or undefined, instead making predictions only in the weakest sense: if a change occurs, then we can predict certain aspects of that change (Heine 2003). This can even be taken a step further (and in the work of some source-based theorists, very much is), where the impetus of the initial change is not explained as a causal relationship, but rather, grammaticalization is a natural symptom of change. There are other more specific claims about the nature of

grammaticalization, but I will engage with those as they become material to the discussion in subsequent chapters.

While grammaticalization enjoys a rich body of research, it is also integrally linked to the growing source-based movement in the field of diachronic study. There is an ongoing debate as to whether the source governs change or if there is some sort of virtue to the result of change. Source-based theorists conclusively find in favor of the source, and much of the discussion of change in the source-based literature comes back to grammaticalization, either as a model for change or as an inherent mechanism of change (e.g., Aristar 1991, Cristofaro 2014, Cristofaro & Ramat 2017). While source-based theorists will often have fellow travelers who propose similar or related ideas (cf. Bybee 2008), I call specific attention to source-based theorists due to the relative orthodoxy present in this segment of the field and because they form one camp in a growing split within the field. This split between strict source-based analysis and the rest of the field of diachronic analysis is one which I will specifically focus on addressing.

2.3 Functional Pressures from Society

Our second category of research is that which focuses on the interface of language with society and the pressures which result from that for language systems and grammars. Now that we are extending our interest to another school of study, we need to contextualize it as an extension of the principles which feed into all diachronic research. A given phenomenon is not necessarily restricted to research within just one of the categories which I am laying out. Instead what matters is the approach of the researcher and the context of the phenomenon. This means for example that the basic mechanics of

grammaticalization can play out in a sphere defined by societal pressures (that have perhaps set the change in motion, or "actuated it", to borrow the term from Weinreich et al 1968, through e.g. language contact or bilingualism), and in such a case, research demonstrating this would fall under this category, not our first category, despite the heavy reliance of much of the research on grammaticalization studies on individual level functional pressures. The reasons for this second category existing are as much a function of old schisms in linguistics as of the nature of the research included here. As mentioned in Darquenes et al. (2019, p3), the study of language change has long struggled with the concept of a division between internal and external motivations. An internal motivation would be a motivation intrinsic to the conduct or structure of language. An external motivation is everything else. Most of the variationist explanations of Zipf and Ohala would fall under the umbrella of internal motivations, as would explanations based on harmonic word orders. Societal interactions and pragmatics on the other hand would be "external". The division of these motivations is not of importance to this work but it is relevant to how scholars have structured their research and explanations. While this internal-external distinction arises in the literature on occasion, I ultimately consider it to be of little use in expanding our understanding of language change — removing the distinction has minimal consequence on the theoretical underpinnings of major research. Furthermore, maintaining this quarantine of explanations is actively harmful: it is distressingly common to see explanations stay fully on one side of the divide, even when a studied phenomenon straddles the schism. This last issue is a substantial contributing factor in societal motivations being classified as a stand-alone category in my typology and serves as a cautionary tale for the dangers of

research typologies when not carefully applied: if a typology does not better inform our understanding of an issue, then it may serve only to limit our understanding instead (it follows then that while my typology here can be useful for understanding the state of the field as it is now and has been in the past, my hope is for it to become unused and irrelevant as the field grows and improves by mending schisms).

Research in this societal pressures category establishes linguistic change (specifically or broadly) as a consequence of the social realities surrounding language use. We will see an example of this in the case study I present on the Spanish *usted* in the next chapter, but this is a common area of study in diachronic linguistics. The most obvious place to which we can turn for examples is a field more frequently associated with sociolinguistics than with historical linguistics: the study of pidgins and creoles (i.a. Thomason 2002). The study of pidgins and creoles is perhaps over-specific however; pidgins and creoles are but a subset of the largest area of research in this category: language contact (i.a. Darquennes et al 2019). We can see language contact as a motivating factor in everything from the grammaticalization of individual linguistic units (Helmbrecht 2015; Mithun 2018, 2020) to larger morphosyntactic constructions (Mithun 2020). Perhaps the broadest realization of this would be in large scale areal effects. When one feature appears broadly in languages of a specific area, that feature's spread is likely due to language contact. This is an incredibly common phenomenon and often results in apparently rare features being heavily represented in certain clusters of languages, even when those languages are not genetically related (i.a. Graham 2016). Such clusters of rare features have long bedeviled the quantitative efforts of typologists, but they are important for any explanation of language change — if a theory cannot

account for these dispreferred features in areal clusters then the theory will be left blind to a large swath of documented diachronic effects. As implied by the discussion of rare features (such as by Graham), language contact is not restricted to simple lexical borrowings. Examples of language contact inducing grammaticalization are common, such as seen in the North American and Californian language regions where parallels in morphology are common even when lexical borrowings appear to have been avoided (Mithun 2020). Mithun describes one pathway for this as an effect of bilingual speakers mirroring noun incorporation strategies from their native language when speaking a second language. This creates parallel constructions in neighboring languages without creating lexical borrowing and without requiring genetic connections. Once created, these constructions are subjected to the normal forces of internal language change and yield the peculiar areal features associated with North American languages even when those features are rare (indeed, Mithun's example of noun-incorporation is presented as the explanation for the development of the unusual suffixal paradigms seen in languages of the Pacific Northwest). This parallel construction phenomenon of language is especially important because it demonstrates a motivation for a change to *start*, not just for it to occur, which is an unusual achievement in the study of language change (cf. Weinreich et al 1968).

Another general area within this category of research involves the study of changes according to pressures from within the language community such as social hierarchy (e.g., Helmbrecht 2015) and taboo avoidance (i.a. Gao 2013, Fleming 2015). Social hierarchy pressures would include the development of the T-V distinction in European languages (discussed in detail in the next chapter) (i.a. Helmbrecht 2015) as

well as the complex politeness distinctions present in East Asian languages such as Japanese (cf. Mogi 2002) and Chinese (cf. Gu 1990). The development and maintenance of these politeness distinctions is controlled by social hiearachy pressures within the community, even if the actual construct or method of construction was borrowed through language contact. Taboo avoidance would include simple lexical avoidance, such as *gosh* being used as an avoidance strategy in English to avoid saying *God* (i.a. Gao 2013), as well as broad avoidance paradigms such as the famous "mother in law" taboo of Australian languages (i.a. Fleming 2015).

The key component to this category of research is that language change and the causal pressures involved therein are contextualized within the broader bounds of society rather than at the individual level. While processes like grammaticalization may still be part of the analysis, the simple variationist drift accounts of our first category will not account for the direct pressure we can witness in the direction of language change when societal pressures are involved, and the research on such phenomena as language contact and sociological effects are often in effect sequestered from more mainstream simple genetic models of language change.

2.4 Structural Pressures

The final category within our typology of diachronic research is that which involves structural pressures. In other commentaries of the field, this is sometimes referred to as teleological explanation (cf. Mithun 2018) or, more rarely, straw-manned as "results-oriented" (i.a. Cristofaro 2019), but these descriptions carry with them substantial baggage. I use the term structural pressures to describe situations in which

there is alleged or demonstrated to be an advantage to the resulting language state after the change has occurred but not necessarily a clear demonstration of how pressures actually yielded that advantaged language state. Much of the research which leans upon this type of explanation has been grounded in synchronic typological studies, but there are well documented historical analyses which can be grouped herein as well (i.a. the harmonic realignment of word order in Heine & Kuteva 2005). This particular category of research was once much more common but is increasingly getting eaten away by research which fits under our first category as we will see below.

Perhaps the best documented example of favored states in language comes from phonology. In work by Lindblom (1986) and Crothers (1978), vowel systems are demonstrated cross-linguistically to favor configurations of specific models. Lindblom demonstrates this as a result of maximized distance between the vowels (with an additional accommodation for the realities of articulation), and Crothers reformulates the same real-world data into a complex implicational hierarchy. An important feature here is the focus on evaluating a system-level virtue. When vowel systems change, there is always movement towards a favored state rather than simple ungoverned drift. Crothers and Lindblom differ however in that Crothers is very focused on the documented phonological universals and their distributions whereas Lindblom's work includes discussion of the motivations for these which would be well at home in our first category and in line with the Ohala variationist approach. Indeed, Lindblom's groundbreaking paper on vowel systems is perhaps best seen as straddling our first and third categories with discussion both of the virtuous states and of the individual level motivations that push towards those states, but this is not itself a problem — the purpose of this typology

is not to create strictly divided zones which must be analyzed separately, but rather to evaluate the types of explanation that researchers and theoreticians turn to when attempting to explain language change. When the motivation and cause proposed is strictly at the level of the structure of the system, then the explanation falls into this third category.

Vowel systems are a well-worn example in linguistic typology where there are very strong consistencies across world languages regardless of genetic or areal factors. At the same time, they are a poor place to let our discussion of structural pressures rest. The reason for this is that while they are already often discussed in these terms in the present state of the field, it would be trivial to redefine them purely within the context of our first typology of diachronic explanations (again relying on Ohala 1989). If this entire category of explanation were so easily reformulated, such reformulation might be a prudent method of harmonizing between conflicting theories and research (I use "conflicting" here with specific reference to the opinions and stances of the researchers, not necessarily as reference to conflict in the underlying data or analyses). Where structural pressures seem to become much more clearly important as the primary source of explanation is in the area of morphosyntax, especially in regards to word order. This is especially true because this is the area where our individual level analyses as currently articulated are often least ready to address the phenomena. Going back at least as far as Sapir (1921), but gaining a real foothold in the field with Greenberg (1963), certain word order correlations have been recognized cross-linguistically. Correlations may be successfully analyzed at the individual level, but correlations in the ordering of constituents (ia Greenberg 1963, Vennemann 1973, Hawkins 1979 & 1983, Dryer 1988)

often prove difficult to demonstrate as a motivating force for change at the individual level. If one takes a modern approach which evaluates efficiency at a synchronic level (such as head ordering in Hawkins 2014 or the ordering of modifiers in Dyer 2017), efficiency can be demonstrated for the most common structures observed synchronically. Hawkins (2014, p102-115) in particular makes a strong argument for the efficiency of harmonic alignment of phrasal heads. Both his analysis and the more conventional generative analysis of Final-Over-Final (Holmberg 2000), which he has argued against, serve to provide the case for a virtuous state for languages. And as a consequence, by instrumentalizing the uniformitarian principle, the synchronic claims have inherent diachronic corollaries and consequences. The problem for diachronic analysis is converting that synchronic efficiency into an articulable diachronic force. When a language state is favored, we can posit that it should be more stable, so once we reach phrasal head alignment within a language, we can reasonably interpret a pressure *not* to change. But when a language has a dispreferred word order typology, such as disharmonic phrasal head ordering, what is the mechanism for pushing change in that language towards a preferred or stable state? Simple drift or natural variation as described in our individual level analyses may not be sufficient or applicable for something categorical like word order (as opposed to gradient phenomena, like semantic or phonetic weight). If a jump must be made by literally moving and repositioning items within the syntax (e.g. Heine & Kuteva 2005 p214) or if an inherited form is doomed to terminal disuse (e.g., Seržant & Rafiyenko 2021) (I will discuss examples of both in Chapter 5), the analysis may support integration of these top level structural efficiencies

into the explanation beyond simple societal or individual level motivating factors or variationist drift.

Structural analyses in language change also lend themselves well to the dominant family of linguistic theories of the second half of the 20th century: generative grammar. By their very nature, generative analyses will tend to lean towards this structural typology of research — it is easy to see this in the discussion of optimality theory constraint re-ranking, where change is explained by reprioritizing the constraints of a language's phonological system (eg. Clark 2004), or Anthony Kroch's analysis of grammar replacement (1989) where he focuses on the uniform progression of changes across the entire structure of the language rather than witnessing localized speed differentials. Similarly, generative analysis of language change lends itself well to a parallel inquiry to that of functionalist typologists, where both might look at the interplay of language universals and change, but within a generative context, the constraints of that change are articulable under the concept of universal grammar (e.g. Kiparsky 2008), and it might even be possible to interpret and construe that position as a variation on my strong application of the Uniformitarian Hypothesis in this work. These demonstrate a focus on the effects of change on structure and vice versa, rather than on factors closer to the individual speaker or society. My own analyses tend to lean quite heavily towards a more functionalist approach, but that on its own does not obviate or deprecate the work and findings of a major segment of linguistics. In fact, not discounting such research is an essential component of my approach in this work — theoretical alignment is largely irrelevant to the possible utility of past work (and this open-mindedness will become especially relevant when we turn to the discussion of theories such as source-based

theory). As a more extended example of generative approaches in this diachronic space and how they often fit within this typology, we can look to Paul Kiparsky's authoritative exposition of his understanding of phonological change (published first as Kiparsky 1995, and later with revisions as Kiparsky 2003). Kiparsky's work frames change as a transference of structures. His analysis identifies sound change as a top-down change, rather than bottom up, as proposed by Ohala. According to Kiparsky (2003), when variation exists, its potential for gaining a foothold in the language is dependent on a compatible underlying structure to the language (e.g. ibid p328). The structure of the language (and Universal Grammar beyond it) defines what is possible and what will be accepted. Similarly, when looking at changes such as vowel shifts or analogical change, these changes are to be explained by the extension or diffusion of structural rules rather than letting the rules be defined by the change. By Kiparsky's reasoning: surface forms flow from the underlying structure, and to change the surface you must account for a change of the structure. Per my own predilections, discussion of structural pressures in this category of research (primarily in Chapter 5) will focus more on work based in the tradition of typology rather than on generative scholarship, but this line of analysis within the generative work on language change is a natural direction for future expansion of the application of the theoretical framework I am developing herein — structural pressures are easily the most difficult to demonstrate and the generative body of work, where it is able to be integrated into a predominantly theory-agnostic framework, is likely to be an orchard ripe with research to leverage on the subject. This does not mean that the structural analyses are meant to remain separate from the two other typologies of research I have laid out, but rather that the amount to which structural pressures play a role in the

grand scheme of language change is naturally going to be challenging to determine and describe, making contributions there all the more valuable.

2.5 Summative

Each of the three categories of research which I have laid out above benefits from coordination with the others, and in subsequent chapters, even as I focus on problems specific to one category of research I will be addressing the others, and wherever possible combining them. I have said above that I hope for this typology to lose utility going forward, and that is not a sign of dissatisfaction with this breakdown, but rather a hope that interdisciplinary dialogue will work to obviate it as tool for discriminating between research and instead be useful in a more limited form for describing explanations. This typology is descriptive of what the field has been, not prescriptive of how research in the field should be conducted. In the following chapters I will evaluate case studies which directly demonstrate and address research problems which fall within these categories, and in doing so I hope to bring into focus a more comprehensive approach to the theory of language change and show how different research disciplines can be hybridized and leveraged in this field.

Chapter 3 will focus primarily on how societal pressures can motivate language change, with an inquiry on the development of the T-V distinction in Europe and using its further development in Spanish as a particular case study. In the course of this chapter we will directly address what utility sociolinguistic explanations may have in explaining language change and entertain the question of whether change can become predictable based off of known causes. This chapter should demonstrate to what extent our Societal

Pressure category can address questions of language change, in doing so I also dispose of the common convenient fiction of the language-internal/-external dichotomy, instead treating the complete system inhabited by language as relevant to its form and evolution.

In Chapter 4, we will focus primarily on the Individual Level Pressures category and how this research contributes to understanding language change through a complex multi-factor analysis of the affixation asymmetry commonly discussed in typology. While focusing on this type of research, the most significant part of this chapter will actually be its aggressive use of synchronic data and research to answer a diachronic question, going back to the premise that the Uniformitarian Principle should allow this extension.

In Chapter 5, we will look at the diachronic maintenance of typological universals and give special attention to two languages: Sri Lankan Portuguese and Ancient Greek. This chapter seeks to demonstrate the utility of Structural Pressures in understanding and explaining language change. In the course of this evaluation, we address the question of lineage-specific or genetic effects in language change and we critique a source-based approach to these data.

Taken in total, the following three chapters will demonstrate the virtue of a multi-factor approach to language change and the utility of our typology of the field in highlighting each type of research and its role in understanding change.

CHAPTER 3

A CASE STUDY IN SOCIETAL PRESSURES

The T-V Distinction and the Genesis of Usted

Many European languages over the last two thousand years have manifested a personal/formal split in their second person pronouns. This split, often referred to as the T-V distinction, spans across language families and seems to have spread often by language contact (cf. Helmbrecht 2015). While this is already interesting as an instance of societal forces interacting with language features, the T-V distinction proves even more useful for our purposes in the way which it frames the context around another change. Old Spanish is typical of many European languages in that it developed this distinction. As Spanish continued to evolve however, there were further changes which resulted in the modern Spanish *usted* replacing the original formal pronoun of that T-V distinction. In this chapter, we will discuss the European T-V distinction and then specifically use the Spanish pronoun *usted* as a useful case study for the analysis of societal-functional pressures in a diachronic system and the utility of conventional explanations for the motivations behind language change. As discussed in Chapter 2, this particular motivation for change, within the overall classification of language change is primarily premised on the dual pressures of frequency and pragmatic utility within the context of society. This position is sometimes more controversial than it should be, as the role of society in shaping language has historically not always been embraced within the broader field of linguistics. Language can be thought of as a self-contained system used for communication or it can be thought of as just one part of a larger system of human experience and interaction. Viewing it in the former light means that we must

characterize pressures on language as being intrinsic to language itself, whereas the alternative is to allow for pressures which are extrinsic to language as a construct but material to language in practice. Finally, *usted* and the gradual evolution of T-V distinction in other European languages presents an unusually clear picture for the evaluation of diachronic forces, to the extent that the changes they went through might have been predictable. Accordingly, they serve us a tantalizing glimpse at what diachronic analysis as a whole might be capable of (should the framework I lay out be successful and adopted within the field). In this case study I aim to demonstrate the application of modern theories of language change to a socially-controlled problem and establish that prediction is a viable goal for diachronic studies. I shall also engage with the concept of internal versus external pressures in language change.

3.1: Theoretical Starting Position

The natural position to start with for analyzing change like the T-V distinction would be to focus on language contact and bilingualism. These certainly have a role to play and will be something that we look at as our analysis proceeds. But we might also start with a different initial assumption, one mostly in line with the modern source-based approach advocated by Sonia Cristofaro and others (cf. Aristar 1991; Cristofaro & Ramat 2017; Cristofaro 2017, 2019; Collins 2019). It is not an approach without some merit, but it can run into problems depending on the scope of the analysis (cf. Dryer 2019), and in this case we necessarily will be extending beyond the traditional framework of the source based approach, purely by virtue of how far our analysis of this case study will go. The source-based approach is typically descriptive in nature: certain diachronic processes

are able to be witnessed in various languages and one can logically claim that change flows from the starting point outward, thus making the end result a consequence of the initial position. Source-based research typically aims to document and demonstrate this flow. But this does not mean that a certain starting point determines a certain end result, and the result cannot be based on teleological norms. This makes the prediction of future changes (or the assertion that future changes *should* be predictable) to be a very difficult premise within a conservative implementation of the source-based approach. This lack of predictability in my view makes an especially conservative source-based analysis potentially a radical variationist position — change is drift within a variationist context but fundamentally unmotivated; the patterns are reliable but the course is set by gentle drift rather than forces unseen. Note that all the components of grammaticalization still apply, but the change is unmotivated — we are just able to describe it by such terms. To make this explicit, under this strictest formulation of source-based ideas, we would say that words reduce in phonological form when used frequently, but that frequent word use is not *causing* the change — the change is just what we observe in that context. In this chapter, we will seek to tie changes directly to societal pressures. In doing so, we are inherently focusing on that question of motivation. Accordingly, while the starting position for this analysis can be considered in line with a source-based approach, the nature of the inquiry will extend it past the bounds of its most conservative applications of source-based analysis and instead rely on more liberal implementations of the premise that I interweave with material that might be heterodox to the theory. This is not a criticism of the source-based theory per se, but rather part of the ongoing effort of this

work to reach a comprehensive understanding and approach from the varied research of scholars who are not otherwise in a robust dialogue with each other.

Within the source-based approach, and especially within the most conservative implementations of it (such as the work by Sonia Cristofaro) grammaticalization is primarily discussed as a process which the source undergoes. As mentioned before, in the strictest interpretations, pressure and outcome should be unassociated, because with such an association, the process starts to resemble the "results-based" or "teleological" approach in opposition to which proponents of the source-based proponents have placed themselves. Such a conflict does not need to exist, however. While a later case study in this work will assume a more result-based stance in the analysis, the core of this case study is that the prior state and context control the later state. The caveat of course is that the context which we will observe is a societal paradigm which is exerting pressures on the source and also involves language contact as a component of the analysis.

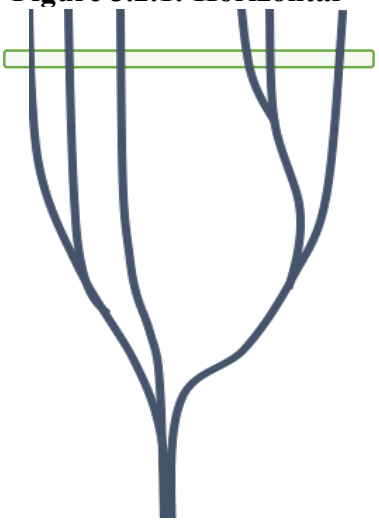
3.2: Why *usted*?

Usted is the 2nd person formal pronoun (2SG.HON) in Spanish and developed in the early 17th century (cf. Pla Cárceles 1923). The genesis of *usted* presents a number of useful qualities which make it ideal as a case study for multiple key problems in language change, but especially the questions of social influence and of causality in language change. The difficulty of demonstrating causal factors in change correlates with the number of factors which can be associated with the phenomenon, so we must attempt to control as many variables as possible, allowing the identification of a specific causal factor without significant confounding variables. With further research in this area it may

be possible to reach the point where a dynamically weighted computational model of linguistic pressures is possible and therefore complex predictions based on that model would be possible. For the present however, we still require careful case studies to identify and demonstrate specific forces, and by extension, predictable responses. With this in mind, *usted* has a number of attractive qualities which align with what we would look for in an ideal case study. I define these qualities below.

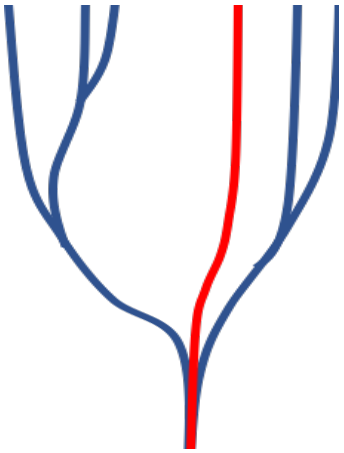
First, we want a scenario where a single style of change occurred across the dialect continuum of a single language or family of languages (the distinction between those two is largely artificial and not important for our purposes). This can be thought of as the "horizontal slice" of the system: the languages/dialects of the target of analysis as sampled at a given moment or window of time. A broad set of closely related languages or dialects of a single language are near-match subjects serve as the closest that we can get to a controlled experiment in this field where we are largely restricted to post-hoc analysis. Each dialect or linguistic region will have a predominantly similar system of forces acting upon them. If we see the same style of change occurring across a horizontal slice, we can identify it unlikely to be random chance. Figure 3.2.1 gives a visualization of the horizontal slice for analysis in a language system, with the lines representing genetic descent in a language family tree and the boxed area being the slice for analysis.

Figure 3.2.1: Horizontal



The second set of characteristics useful in a case study of this type is a discernable pattern in what we will call the "vertical slice". In Figure 3.2.2, we see a large language family, represented in the same manner as in Figure 3.2.1. If we follow the genesis of one branch of that family (the red branch), that branch will constitute a vertical slice. These two modes of analysis are not mutually exclusive and using one mode does not preclude using the other as part of a coordinated analysis. As an example, we can speak of the Romance languages being a viable horizontal slice of analysis in conjunction with the vertical slice of Latin. Similarly, if we take a vertical slice of Spanish, we can still look back to Latin, and we can analyze the broader dialect region of Spanish, but we are no longer including the other daughters of Latin such as French or Romanian. The advantage of the vertical slice is that it allows us to compare a language back against itself. If a language is going through the same set of changes repeatedly, we can see that and incorporate it into our analysis. The same can be said if there is a repeating change that ceases, because that allows us to search for pressures which are no longer present or are no longer unopposed.

Figure 3.2.2: Vertical Slice



3.3: Usted and the T-V distinction in European languages

The transition to *usted* in Spanish follows a sequence of earlier relevant changes in the pronominal system (Briefly: *vos* → *vuestra merced* → *usted*). Many European languages developed a formal distinction in their second person pronouns, following Late Latin's repurposing of the plural *vos* to double

as a singular honorific prior to the 5th century (Helmbrecht 2015). This has broadly been labeled the "T-V" or "V-T" distinction in the literature (cf. Brown & Gilman 1960; Helmbrecht 2015). While termed as an "honorific" or "formal" distinction, it might be best to think of this phenomena as a matter of social distancing — this understanding will scale better across the centuries and across social strata. Despite originating in Latin, this change was not adopted uniformly however, and we can see that the descendent languages of Latin did not necessarily have this distinction at the point where we begin to refer to them as separate languages (cf. Table 3.3.1). This is even to the point that pragmatic borrowing of this formality distinction in the neighboring Old High German pre-dates the documentation of the *tu/vos* formality distinction in Old Spanish (Helmbrecht 2015). Documentation of Old Spanish attests use of the *tu/vos* split of the 2nd person singular from approximately the 12th century onward (Helmbrecht 2015 p320; Penny 2002, p137). Helmbrecht indicates that the *tu/vos* split died out in Spanish around the 16th century, but as we will see, the real story involves a subsequent evolution within the language affecting *vos* and constructions using it. Regardless of these later developments, we can see that Spanish, much like Latin's original formation of the distinction earlier in the vertical slice of Spanish, had the familiar *tú* and the formal *vos* as singular terms of address (with *vos* maintaining its use as the plural without the implications of formality).

Table 3.3.1: Attestation of 2PL > 2SG.HON in some European Languages

Language	Century
Late Latin	3rd-5th
German (OHG)	9th-18th
French (Old)	11th-20th
Spanish (Old)	12th-16th
Dutch (Middle)	12th-16th
English (Middle)	12th-17th
Norwegian (Old)	13th-17th
Italian	13th-17th
Danish (Old)	14th-18th
Czech	15th-20th
Polish	15th-17th
Icelandic	17th-20th
Russian	18th-20th
Serbian	19th-20th
Bulgarian	19th-20th

(Helmbrecht 2015)

The dual use of *vos* as both a singular and plural pronoun presumably precipitated the formation of the *vos* + *otros* ("you others") compound as a way to help differentiate between the honorific usage and the original plural meaning (Penny 2002, p138) — this is not unlike the pragmatic variations seen in various English dialects that give a more explicit plural for "you" such as "y'all" or "yinz" (Galiano 2020). Just as the original *vos* formation was initially used for reference to the Emperor in Rome (akin to the "royal 'we'" in English) (Penny 2002, p138) and then generalized to use across the empire and its contacted cultures (Helmbrecht 2015; Penny 2002), Old Spanish saw *vos* spread beyond the original specific use. The spread of formal *vos* beyond just applying to royalty can be interpreted as a kind of leveling — the mere expansion of reference beyond the original single royal entity means it has been expanded to apply to less prestigious contexts and people. Accordingly, the formal meaning of the word logically loses some of its prestige as well. This then leads to the creation of another more formal construction such as *vuestra merced* (literally, "Your Mercy"). This construction uses an evolved genitive of *vos* as part of the larger phrase of anaphoric reference and entered use around 1400 C.E. (Pla Cárceles 1923). The use of vuestra merced started as a novel construction, as seen by the use of competing terms such as *vuestra excellencia* ("Your Excellency"), but eventually became a standardized frozen form along with some of its competitors (ibid).

3.4: Corpus Data and the Forms of *Vuestra Merced*

Over the course of the two centuries following the formation of the *vuestra merced* construction, it underwent very little variation, functioning in practical terms as single unit (not as a novel construction) in parallel to some competitors such as *vuestra excellencia* (Pla Cárceles 1923). However, in the 16th and 17th centuries, a multitude of abbreviated forms develop from the *vuestra merced* construction. José Pla Cárceles identifies written reference to colloquial shortening as early as 1573, and written examples from around 1597 (Pla Cárceles 1923). As demonstrated in Table 3.4.1, these forms all manifested the phonetic lenition expected of grammaticalization, and this parallels the common understanding in the literature that *vuestra merced* had expanded in domain beyond exclusive use for royalty (Pla Cárceles 1923; Penny 2002).

Table 3.4.1: Attested Forms of 2SG.HON in Spanish

Date	Form
1400	vuestra merced
1597	vosasted
1597	vuesa merced
1598	voacé
1603	vosancé (bosancé/boxanxé)
1617	vuasted
1617	vuesance
1619	voaced
1619	vusted
1620	usted (final form)
1621	buersarced
1624	vuessansté
1625	vuesansted
1635	vuested
1635	voarced

(Pla Carceles, 1923; Krotkoff 1963)

We can substantiate and build upon the philological work of Pla Cárceles through the use of corpus data to expand on his representations of the data. Using Pla Cárceles's forms (and Krotkoff's 1963 compilation thereof) as a starting point, I investigated their frequency across three corpora: the *Corpus del Español*, created by Mark Davies; CHARTA (Corpus Hispánico y Americano en la Red: Textos Antiguos) created by Francisco Javier Pueyo and Bautista Horcajada; and CORDE (El Corpus Diacrónico del Español) a historical database maintained by the Real Academia Española. None of these corpora provide data as broad or specific in attested forms as Pla Cárceles's original work (refer

to Table 3.4.1 for a summary of Pla Cárceles's original data and dates), but they do expand our resolution along specific axes.

The *Corpus del Español* is a literature corpus built by combining disparate subcorpora into a single database. This creates a number of limitations that need to be acknowledged. First, there is a massive over-representation of specific authors. As an example, one of the subcorpora within our date range is specifically and solely devoted to the work of Miguel de Cervantes Saavedra, who was extremely prolific. The corpus is also poorly documented for regional origin of the tokens — we do not have an easy way to associate each token with an area of publication. This lopsided nature of the corpus forces us to use it in specific ways. Statistical metrics on this data are inadvisable due to the extreme selection bias, but the corpus still answers specific questions. First, because a literature corpus such as this naturally focuses on the foremost literary figures of the historical discourse, we can discern community penetration of some of these forms. Additionally, the impact of prevalent authors in extending change in this era should not go understated — we can draw analogy to English and see the dramatic impact the writings of William Shakespeare had on the character of Early Modern English (cf. Crystal 2005, p304-305). Another limitation of this corpus is that its subcorpora encode information inconsistently. Of particular interest to us is that the date of a given token is often affected. The most common variance here is that many tokens are dated as an average of the birthdate and death date of the author. Some of these prolific writers however may have been active over decades (many had lifespans greater than sixty years), and as we can see from the Cárceles dates (refer again to Table 3.4.1), a difference of just four decades could place them at a substantially different point in our timeline.

This is especially problematic because many authors used more than just one form in their writing. We lack the discrimination to tell if their writing changed over time when all works appear as a single date (for example, all writings by Cervantes are dated 1582 in the corpus). This last point does illuminate the unexpected boon from the weaknesses of the *Corpus del Español* however: the overrepresentation of single authors means that we have the data to show that even when looking at the writings of only a single author we can find multiple different forms in use. This may be due to regional accent characterizations or due to progressive changes in the then current language. Due to the lack of arealization of the corpus data however, our conclusions drawn here do not differentiate regional dialects or accents from our horizontal slice.

CORDE is the official historical corpus maintained by the Real Academia Española. While it has a broader set of texts than the *Corpus del Español*, it still does not manage to capture the breadth of forms documented in the manual philological review by Pla Cárceles. CORDE also lacks the depth which the Davies corpus gains by virtue of the same specialty subcorpora which prevent quantitative analysis. CORDE is superior however in its curation. Because it is one corpus assembled from the ground up, its entries are much more uniform in quality and the metadata is more robust. Accordingly, in Table 3.4.2, the CORDE dates should always be assumed to be superior than the Davies corpus dates, although both are presented for completeness.

Table 3.4.2: Pla Cárceles with Corpus del Español & CORDE

Forms and Attested Dates:	**Cárceles Date[1,2]**	Earliest Author Born[3]	Latest Author Died[3]	Earliest Corpus Date	Latest Corpus Date
vuestra merced	**1400*** **	1200[3]	1900s**	1140[4], 1300[3]	1978[3]**
vuesa merced	**1597**	1449	1700s	1445[4], 1500[3]	1613[4], 1700s[3]
vosasted	**1597**	<>	<>	<>	<>
voacé	**1598**	1547	1645	1580[4], 1582[3]	1673[4], 1609[3]
vosancé (bosancé/boxanxé)	**1603**	1562	1644	1613[4], 1598[3]	1674[4], 1609[3]*
vuasted	**1617**	<>	<>	<>	<>
vuesance	**1617**	<>	<>	<>	<>
vusted	**1619**	1574	1728	1597[4], 1609[3]	1682[4], 1689[3]
voaced	**1619**	<>	<>	<>	<>
usted	**1620**	1562	N/A	1572[4] ,1598[3]	Current
buersarced	**1621**	<>	<>	<>	<>
vuessansté	**1624**	<>	<>	<>	<>
vuesansted	**1625**	<>	<>	<>	<>
vusted	**1635**	1574	1695	1605[4]	1670[4], 1673[3]
voarced	**1635**	<>	<>	1605[4]	1630[4]

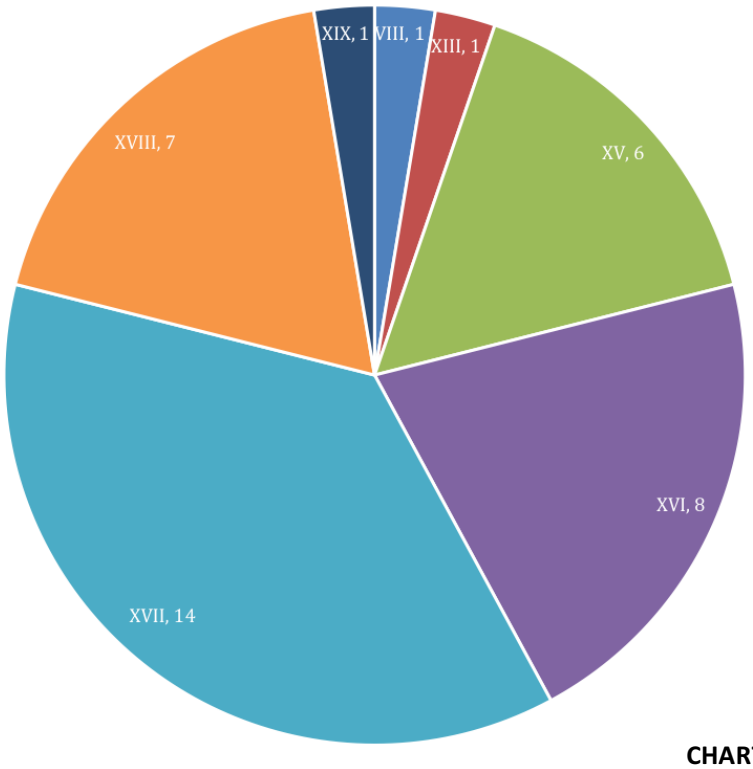
*Dummy value calculated per corpus practice.
**Value represents use which is eccentric or within period fiction.
***Value represents a judgement call as to when it was first used not as an honorific

[1]Pla Cárceles 1923 [2]Krotkoff 1963 [3]Davies 2002- [4]CORDE

CHARTA on the other hand gives us excellent detail on the date, the location, and document metadata, but it is a much smaller corpus and has a much narrower scope. CHARTA is composed of manuscripts, such as decrees, certificates, contracts, legislation, and other handwritten documents, generally with complete scans for each and dual transcriptions: one transcription intended to be faithful to the original spelling (labeled in the corpus as the "paleographic form") and one with a "corrected" (modern) spelling, allowing the ability to search for both unusual written forms and more general assumed intended forms. I searched for each entry in the Pla Cárceles list by written form first and then by the general form, investigating any additional examples revealed by this process for potential miscategorization. Perhaps due to the generally more formal context, CHARTA gives excellent data on the more standard unreduced form and very poor data on the (presumably more colloquial) reduced forms.

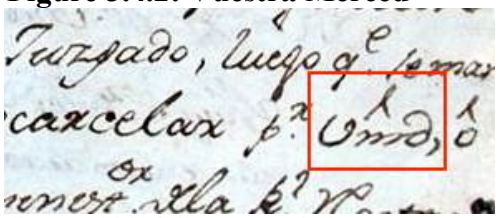
Figure 3.4.1 shows the spread of CHARTA documents with *vuestra merced* by century. The 8th century example is limited to one example, but likely is used there as a novel construction rather than a frozen expression (This is

Figure 3.4.1: Vuestra Merced Documents by Century



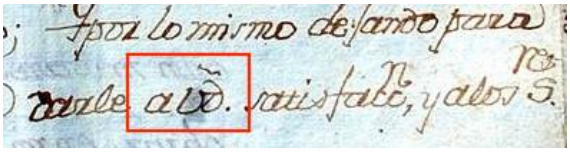
reinforced by the multi-century gap between documents). While CHARTA's coverage of the various forms is quite limited (38 documents with *vuestra merced*, 1 with *vuesa merced*, and 1 with *usted*), and in a generally more static setting, the more precise date information is useful for seeing the preservation of *vuestra merced* at the most formal registers. Lastly, CHARTA calls attention to some of the judgements being made in these datings. For *vuestra merced* and *usted* especially, textual examples will often be abbreviated forms (See Figures 3.4.2 & 3.4.3 for *vuestra merced* and *usted* respectively — both from Venezuela, 1783). This is problematic when differentiating between near forms. In Figure 3.4.2, for example, we would be unable to distinguish if this were actually representing *vuesa merced*. Similarly, the handwriting analysis by the archive on Figure 3.4.3 lists the paleographic form here as *vsted*, but we would be unable to discern if this was representative of *vusted*. Lastly, we are faced with a question which I will not try to answer here: could the abbreviations of the written forms have played a role in the evolution of the spoken form?

Figure 3.4.2: Vuestra Merced



CDHM-0019 | GLH-ULA | CDHM

Figure 3.4.3: Usted

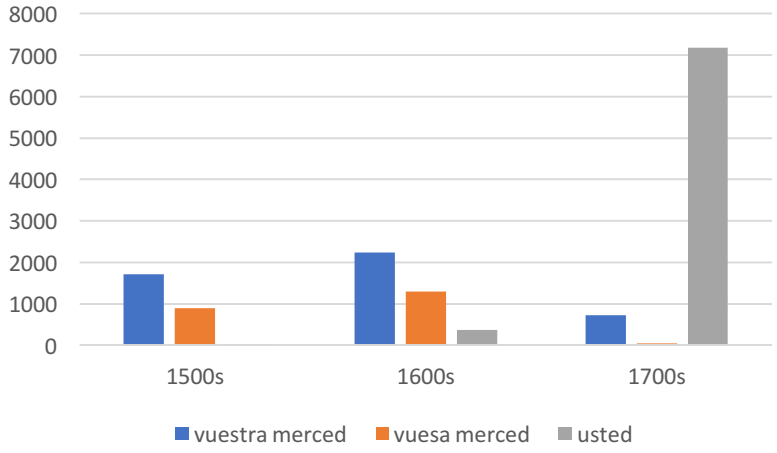


CDHM-0014 | GLH-ULA | CDHM

These corpus sources serve to augment the original Pla Cárceles work, but none of them gives us as complete of a list as his original work. This is in part due to the old philological tradition in which he conducted his work: manually recording not just the instances captured in Spanish literature (as most of the Davies corpus and CORDE do) but also the contemporary

commentary from linguistically related regions, especially Italy. CORDE and the *Corpus del Español* grant us independent and robust corroborating data points on many of Pla Cárceles's documented forms and the information that various authors used multiple, or even many of them. We also gain some measure of sense of frequency, but this must be taken with a grain of salt due to the nature of the corpus data. With that said, Figure 3.4.4 shows the raw counts of the three most common forms over time. CHARTA both substantiates the continued use of the full form in very formal settings (this is backed up too by CORDE and the *Corpus de Español* showing continued waning use through the 18th century) and demonstrates the limitations in extending this to handwritten manuscripts due to the conventions of abbreviation.

Figure 3.4.4: Major Forms Over Time



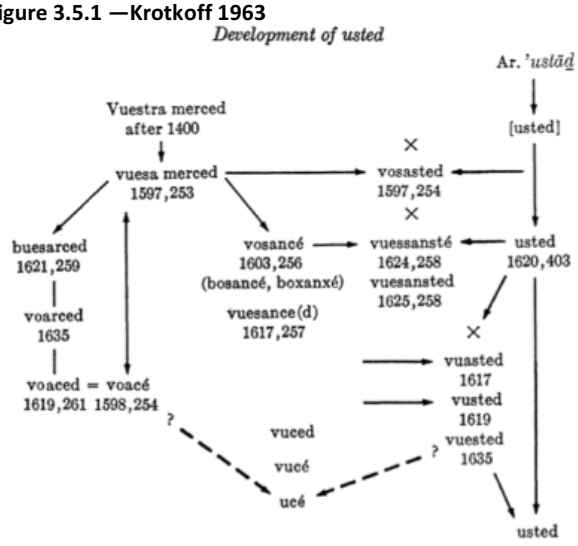
3.5 Discussion

Helmbrecht's original breakdown of the T-V distinction showed a propagation of the distinction across languages and language family divides (2015). The mechanism for this is known as pragmatic borrowing and is itself a mechanism of socially motivated change (cf. Mithun 2007, 2015, 2020). Mithun has written extensively on this subject in regards to Californian languages and typologically unusual agglutinative affixes and constructions which are well attested in that areal context but not elsewhere. In this case, where individuals are

bilingual and interact in more than one speech community, they may borrow the constructions from another known language without borrowing the words. With the T-V distinction, language communities had a social need for maintaining social distance or demonstrating formality and through language contact or bilingualism had a model (extend use of a plural pronoun to include use as a singular formal pronoun). This was common across the European language region, and we saw that it extended to Spanish. Spanish of course loses the strict implementation of this model when it develops *vuestra merced,* and later *usted*, which results in it dropping off of Helmbrecht's list in the 17th century.

Focusing back specifically on our discussion of Spanish, the major forms in the data from the corpora are *vuestra merced*, *vuesa merced*, and *usted*. We can see that while both of the long forms see a bump in the 1600s with the increase in total number of documents, they fall rapidly in the 1700s. More importantly, we can see from the Pla Cárceles list that the numerous shortened competitor forms arose within a fairly short timespan, although it is reasonable to assume that such attested forms would be slow to appear in written record and would likely be underrepresented therein as well (if for no other reason than the pressure and tendency for writing to represent the more conservative "proper" speech). Interestingly, these forms do not all necessarily flow naturally from one another (cf. Krotkoff's derivational diagram in Figure 3.5.1), and indeed this has prompted some to look outside of Spanish for contributions to this process via language contact (cf. Krotkoff 1963). The previous

Figure 3.5.1 —Krotkoff 1963



Moorish occupation of Spain would make an Arabic ingredient highly plausible (as proposed in Krotkoff 1963), but this necessarily assumes a lingering of Arabic influence in peninsular speech. This possibility is especially attractive due to the similarity in form and function of the Arabic *'ustāḏ* (IPA: ʔustɛ:ð; Orthography: أستاذ), now commonly translated as "professor", "mister", or "master".

While this potential cross-linguistic influence is noteworthy enough to mention, it represents at most a bump in our analysis, not a crack in the foundation. This is for a number of reasons. First, even if *usted* was initially sourced from Arabic (as seems plausible due to the strong similarities in form), it is likely that the rise of *usted* as a prominent form in Spanish was assisted by the use of convergent reduced forms of *vuestra merced*. One way of interpreting this is by the verbal agreement with *usted* in Spanish. Despite being a 2nd person pronoun, *usted* inherits the 3rd person verb conjugations which would be expected to accompany the distanced address of "your mercy" rather than what might be expected of a vocative use of "mister" or "master". Second, the form does demonstrate the phonetic lightening compared to *vuestra merced* that we would expect to see both as a result of grammaticalization and in accordance with the Zipfian maxim — that more high frequency words should be smaller. Furthermore, if there was substantial bilingualism, and the Arabic form was in pragmatically similar use the abbreviation conventions of the day could have given opportunity for misanalysis or convergence. Lastly, the second and first person pronouns in particular often derive from lexical entries specifying personal relations (as opposed to third person which more predictably derive from demonstratives) (Lehmann 1995; p40). Pulling *usted* from the Arabic lexical token would be consistent with the general paradigm of grammaticalization of this type of pronoun and would

still be consistent with the analysis presented in the next section on the functional pressure motivating the change when considered in the context of the attested convergent forms.

3.6 Cause and Effect

The *vuestra merced* shift and explosion of derivative forms can best be explained by functional motivations. While *vos* (the original "V" of the T-V distinction in Spanish) was still in use at the point that these numerous forms became attested (Pla Cárceles 1923), its productivity as a formal marker had eroded significantly, collapsing perhaps so far as to the familiarity level of *tú* (Penny 2002, p138). This is significant because it means that our vertical slice shows previous a leveling of the honorific over time. The expansion of reference for *vos* beyond a specific person (beyond the king or emperor) mirrors the later expansion we see for *vuestra merced.* In fact, this creates the very context for the pragmatic domain of *vuestra merced* to broaden (cf. Pla Cárceles 1923) as would be necessary for grammaticalization, since the erosion of *vos* as an honorific creates a gap in the language for a suitable formal (or socially distant) term of address and deictic reference. Due to an apparent societal need for such a term this gap represents a functional pressure on the language. In this case however, we do not see the same option available as before. The European language region's communities clearly showed a broad need for a formal or socially distanced term of address (as seen by the spread of the TV distinction), but here Spanish has that need despite having already exhausted that option for borrowing — the plural has already been repurposed once and eroded to the point of being useless in this role. Accordingly, this pressure created by the societal needs can be seen in the previously mentioned expansion of the use of *vuestra merced*. The problem of course quickly becomes apparent: as the *vuestra merced* construction is expanded to greater and greater use, its

phonetic heft becomes more and more inappropriate for its role in the language. Having started as an obsequious honorific specifically for the king, any expansion beyond that specific limited use is already a large pragmatic widening and semantic weakening. We can see the same thing with the expansion of the original T-V distinction: once used to refer to the emperor of Rome, the use of the plural as an honorific spread to the edges of the empire (Helmbrecht 2015), and that expansion alone is demonstration of a pragmatic widening.

As with the original T-V distinction, the pragmatic widening introduced a new set of pressures on *vuestra merced*, but this time in addition to the semantic shift and erosion, there was a corresponding phonetic lightening. Put simply, four syllables is quite large and the phonetic lenition that we expect to accompany syntactic strengthening does not appear to have occurred in the first two hundred years of the existence of *vuestra merced* as a set phrase (Pla Cárceles 1923). The progressive pragmatic weakening of the formal singular *vos* continually increased the pressure exerted on the language to coopt another term to this role (whether it be a reduced *vuestra merced*, an alternate borrowed term, or a combination of the two) eventually forcing the mass response we saw presented in Tables 3.4.1 & 3.4.2. The important aspect of this is that while the erosion of *vos* and perhaps even the initial extension of *vuestra merced* can be easily interpreted through diachronic language-internal processes and the generic extension of use across social classes, the pressure to grammaticalize a replacement for the formal anaphor is defined more by the social context (including social hierarchy, bilingualism, and language contact) than by the linguistic context present within Spanish. We also see that these forms were in dynamic change throughout this period, with authors using multiple forms throughout their writings. Additionally, the preservation of the full *vuestra merced* in legal documents is unsurprising, since we might expect weaker pressure on the form in these limited contexts due to

the lower use, as well as an increased effort to maintain static and pedigreed language forms in such contexts.

Within the source-based approach, this all looks very familiar and easy to describe — a commonly used word followed a familiar diachronic process. Exactly as this analysis expects, the changes look like a natural follow-through of a change that is within the defined sets of common changes documented in diachronic typology and historical linguistics. The problem however is that while the source-based analysis is correct on all these points, it is strictly descriptive and language-internal. Under a source-based analysis, we see two examples of grammaticalization involving *vos* and *vuestra merced* with strong documentation and both fit perfectly within established models. With the addition of the conception of causal forces in the system laid out here though, certain parts of the sequence become more readily explainable and predictable. Thus, we can tie together the two separate phenomena into one larger picture. The downward pressure on the formality of *vos* earlier in our vertical slice is both indicative of a frequent use of the formal pronoun and of the development of a pragmatic gap — the practical use of the lexical item no longer adequately fulfilled the actual pragmatic need. From this starting point, we can predict that, given the similar social constraints, another formal term of address would likely rise to fill the pragmatic gap because the society speaking this language demonstrated a need for that formality (The nature of that need is where the fields of sociology and anthropology would take over in a broader analysis, but stipulating to the demonstration of that need is sufficient for our purposes here). Note that while the conventionalization of a replacement form was predictable here, the precise descendent construction is not possible to predict from the earlier language state. Once something begins to fill that pragmatic gap, we should expect the same subsequent expansion of reference for that item (in this case, *vuestra*

merced) as was seen with *vos*. This is predictable both from simple analogy to the prior history of the language in our vertical slice and from analysis of the same pressures leading to the conventionalization of the *vuestra merced* construction. The pragmatic gap was created by the use and leveling of *vos* and the societal pressure for an honorific inherently indicates that this honorific will see use. Of course, once conventionalized, *vuestra merced* is in the same position from which we saw the expansion of reference with *vos*. From this last position, we then can expect and predict with fair certainty that phonetic lightening will occur as a consequence of the expansion of *vuestra merced* into more common contexts. Our horizontal slice validates this quite well; again, we cannot predict the end successful form, but we can predict with fair certainty that the *vuestra merced* form would reduce, and we see a large number of competing attested derivative forms in exactly that vein. There of course is one problem here, which is that we cannot say for sure whether *vuestra merced* truly grammaticalized successfully down to *usted* or whether language contact allowed the borrowing of a pragmatically similar term at an opportune time leading to either convergence or even suppletion. This in the end does not matter though, because the pressure for the language to have a suitable formal second person pronoun is still being relieved either way.

The predictions as discussed here share a common theme. Where we can predict change, our predictions will spring from a mix of past comparable observable data and from an analysis of the functional pressure on the language given its use at that time. The predictions which are made possible are going to be neither precise nor absolute. At no point in this process could we say "this will be the next state of the language"; instead we are restricted to more reserved statements, along the lines of "from this position, we can predict these styles of changes, for these reasons", and from certain states we can confidently predict follow-through changes (when

given sufficient time depth) such as the phonetic lightening of *vuestra merced* with its increased use (and this in turn gets us very close to finally addressing the actuation problem of attempting to determine why or how changes actually start, as discussed in Weinreich et al 1968). This last point bears additional focus: it is the inflection point for the contentious split between results-based and source-based analyses. One might say that the language *must* change to a more harmonious state from the unstable state with *vuestra merced*. While this borders on explanation by teleology, it is not an unreasonable interpretation of the strength of the pressures I have posited to be acting upon *vuestra merced*, and puts the analysis solidly back towards a results-based analysis. This represents the greatest problem that the strictest implementation of the source-based approach tends to find itself facing: as soon as the approach nears a strong generalizable statement capable of prediction, that statement automatically is incompatible with the very analysis which yielded it. The problem arises in two fashions. First, we have the problem discussed above about *vuestra merced* being under pressure to reduce. Source-based theory agrees that its reduction made sense, but our proposal that there is a better form that is motivated or that there is a virtue to the change is not compatible with strict interpretations of the theory. Second, we find the language-internal distinction to be counter-productively interfering with our analysis again. We can see that in a hierarchical society, there would be frequent need for a formal pronoun. This is context beyond the strict confines of the walled garden of language-internal analysis. But these external factors matter if we are to improve our explanatory powers. In a similar vein, we have the pragmatic borrowing of the original T-V distinction and then the open question as to how or if language contact with Arabic (or corresponding bilingualism) was involved in the development of *usted.* Interplay between languages such as this is difficult because it again introduces questions not answered within the

original source. In other cases of grammaticalization there may be language-internal shifts that trigger certain consequences, and source-based theory has less trouble with these scenarios, but as we integrate more data and chase explanations capable of solving problems like actuation, it becomes necessary to give a fuller discussion of the language-external pressures, such as those in this case. Historical linguistics has never before been able to say confidently that a specific change in a language is going to start (ia Weinreich et al 1968), and source-based analyses so far have not been able to break that barrier. But problems like this however are exactly what our interdisciplinary harmonization serves to address: by amalgamating the methods and claims of established theoretical camps with those allegedly unrelated or even in conflict, we are able to broaden the utility and power of the analyses we are able to formulate.

3.7: Conclusion

After the original borrowing of the European T-V model for a 2SG.HON pronoun, we can see that an extended period of pragmatic drift in the Spanish pronominal system created a build-up in pressure for the language to eventually respond to in order to restabilize. The weakening of *vos* as a productive honorific and the extension of the super formal *vuestra merced* into that pragmatic domain created a context where the language was no longer at a balance – the original formal item was no longer reliably formal and the next closest construction was cumbersome. On top of this, the T-V paradigm was not a productive source for a solution, because *vos* was still in use, even in its deprecated state. Despite appearing in the written record in a rather abrupt fashion (an explosion of reduced forms in a relatively tight temporal window), the rebalancing of the language we discussed in Spanish was the result of a gradually widening gap between the expressive needs of the language community and the language's practical application (which by

extension can be interpreted as a gradually increasing functional pressure on the language). The specific mechanics of this change are left uncertain — we discussed substantial possibility for the influence of language contact on the process — but we are able to draw a line from a specific causal pressure to a specific effect in a predictable manner here. While other forces may be at play here as well, at its core this case study demonstrate a social pressure fomenting a grammatical response from the language. This is significant because it represents a clear causal model of a change, giving a toehold for breaking past the limitations of restrained strictly descriptive language-internal analyses and perhaps even confronting the activation problem. At the same time, it also provides a clear and direct example of society itself, a purportedly linguistically extrinsic force, unambiguously yielding ongoing change within a language.

CHAPTER 4

THE AFFIXATION ASYMMETRY

A Case Study in the Interdisciplinary Investigation of Functional Pressures

4.1 Theoretical Background: Approaching Synchronic Phenomena Diachronically

In Chapter 2, I laid out a tripartite typology of research in the field of diachronic linguistics divided up according to the manner in which answers and conclusions were framed and involving pressures at the level of the individual, societal pressures, and structural pressures. These three categories of research map reasonably well onto the analyses advanced within the field as well as onto the directions of research pursued, and can in turn be applied to the discussion of the actual linguistic problems investigated. One challenge as we reformulate our approach to these problems, even when taking cues from a single one of these categories of research, is recognizing how broadly each of these categories should be interpreted. A useful case study to focus on is the affixation asymmetry — this involves the dual observation that there is a tendency across languages as a whole to favor suffixing over prefixing and that the balance of prefixation verses suffixation is correlated with word order typology. This problem is especially useful to us because truly understanding it requires a heavy emphasis on the integration of a broad range of contributory studies, especially those which were originally conducted using purely synchronic methodologies.

The approach taken in this chapter relies especially on a certain theoretical grounding regarding the evolution of languages. The maintenance of a pattern synchronically requires an understanding of the diachronic processes capable of yielding that pattern. For example, most languages change word order over time, whether as an internal process (cf. Harris 1978) or as the

result of areal contact with languages of other word orders (cf. Nichols 1995). The analysis of synchronic patterns is often limited simply to the synchronic, but there is a continuity of language states that lead to what we see today. An implicational universal seen today should not have historically have violations as a common occurrence, and when we see irregularities or violations, those violations should generally serve to reinforce the universals by being part of a single narrative of movement, not disparate processes (cf. Hawkins 1979). This means, that if a rare or violating form arises, we should expect that form to be prone to eroding or otherwise changing until the universal is again met.

Synthesizing the condensed outline above, we can lay out these principles of language universals and their interactions with language change as follows. We will do this first in terms of language change as a phenomenon and then as it relates to synchronic patterns. First, we know that all languages change over time. There is a body of literature for changes across nearly every aspect of language (cf. Joseph & Janda 2008). Second, these changes are often interlinked. The clines and patterns of grammaticalization, as previously discussed in Section 2.2, are a classic example of this, where components of syntactic ordering, phonetic erosion, semantic shift, and morphological development all are packaged into a single narrative (cf. Heine et al 1991, Lehmann 1995, Hopper & Traugott 1993). In our analysis of the Spanish *usted* in Chapter 3, we saw a subset of these with the interconnectivity of the pragmatics, semantics, and phonetic manifestation. This linkage of ongoing changes is relevant to our analysis of any given change: to explain one change, there will be multiple components, and only by the integration of all of them can we address the phenomenon as a whole.

These changes also should not be considered random. While, as mentioned in Chapter 1, the field has struggled with strong claims ranging from treating language change as an example

of Darwinian evolution (cf. Jastrow 1886) to equally strong claims alleging complete unpredictability (cf. Joos 1958 as cited in Campbell 2013, p322), the patterns we see in the languages today should give strong credence to the idea that changes in the system are at a minimum not random vectors in all directions, but instead follow common pathways — perhaps by functional pressures, perhaps by the very nature of erosive effects, perhaps by a mixture of these and other factors. Put in a different way, we can say that to explain any modern pattern, we have to be able to give an accompanying explanation for how we reach the present scenario — doubly so if our explanation is founded on a premise of cognitive advantage (Mithun 2003). Additionally, any universal (whether strict or probabilistic) or motivation which we propose for languages today should also hold across languages in the past under the Uniformitarian Principle as discussed in Chapter 1 (cf. Labov 1972; Hawkins 1979, 1983, etc.). As will be relevant with the present analysis of the affixation asymmetry (elaborated on below), this means that if we find a correlation of two phenomena, we must be able to explain why the ongoing change and variance of one does not wipe out the correlation of the two features. Based on this, we can predict that when two features strongly correlate, they also correlate in change over time — either both respond to the same functional pressure or to linked pressures, or one will have a causal relationship with the other.

Bringing us back to the uniformitarian principle, in practical terms, this establishes two broad criteria which any theory of synchronic typological patterning should meet:

1. Any proposition based in historical derivation needs to be able to explain how the pattern is maintained even with word order drift in languages and explain why some features remain intact while the rest of the language changes around them.

2. Any proposition based on a synchronic state should also have a viable derivational path to reach the current state from former states.

Both of these criteria essentially say that any analysis seeking to explain the phenomenon needs to be robust enough to handle ongoing change in the system. For example, per the first criterion, we need to be able to explain why languages shifting from SVO (Subject-Verb-Object) to SOV (Subject-Object-Verb) will not erase the differences between those two word order typologies when it comes to typological correlates such as preferred affixation strategy. Similarly, per the second criterion, if we propose that affixation strategy is dependent on the synchronic state of the language, we need to have a diachronic means of reaching that point — we cannot simply say for example that a language being OV today inherently makes it a predominantly suffixing language.

4.2 The Suffixing Preference and Empirical Support

Languages across the world demonstrate an asymmetry in the frequency of prefixation relative to suffixation (ia. Sapir 1921, Greenberg 1963, etc). Suffixation is by far the most common form of affixation, as shown repeatedly in language sampling (cf. the aggregation of samples conducted in Greenberg 1963, Hawkins & Gilligan 1988, and Bybee et al 1990), and in this section we will review the pattern as documented across some of the language samples leveraged in past studies. As we will see, the phenomenon is not evenly manifested across languages. Languages with typical Object-Verb word order have a predominance of suffixation relative to prefixation, sometimes almost to the point of exclusivity (cf. Lehmann 1978, Hawkins & Gilligan 1988, Bybee et al 1990). The strength of this statement varies with the specific languages in a sample, the categories of affixes being investigated, and the specific methodologies employed.

First, let us look at the sample used by Bybee, Pagliuca, and Perkins (1990). This was a seventy-one-language sample created as part of the GRAMCATS database. As with the Gilligan sample which we will discuss later, this sample aims to control for both family relations and areal effects. The GRAMCATS sample however has a number of quirks to its methodology, and this will impact how we engage with the data. First, the sample distinguishes between bound and non-bound grams (grammatical items), but this distinction is not in the traditional linguistic sense of bound and unbound morphemes. Instead, the quality of being bound or not bound is defined by the traditional orthographic convention utilized in the reference material for the languages — this is not an uncommon or negligent practice: it is common for the expedience of language sampling in typology but it is important just to note that this is a heuristic-based sampling which will naturally carry over some idiosyncrasies from the orthographies. We should not interpret this as a definitive statement of whether a grammatical element is affixed or unaffixed, but instead as a general heuristic. This is not a problem for our analysis per se, but it does mean that the data from the Bybee sample is essentially labeled according to a unique convention not maintained elsewhere and which may not demonstrate quite the same distribution on errors in its data as other sample sets.

Due to the relative frequencies of the different word order types across the globe, the resolution of patterns in rarer word orders is quite poor. The discussion of the Bybee sample in Bybee, Pagliuca, and Perkins follows the same aggregation as other authors analyzing prior samples. Languages are grouped according to their general word order, but these word orders are not treated equally. The two most prevalent word orders (SOV and SVO) are treated as individual categories, and then VSO and VOS are categorized together. OSV and OVS are both so uncommon that they are unlikely to show up in multiples (or even as singletons), even in

weighted samples. Bybee, Pagliuca, and Perkins justify the grouping of VSO and VOS as separate from SVO due to a difference in the relative frequencies of affixes of SVO compared to the other types of VO languages (1990). I should mention that they call SVO "V-Medial" in their discussion, but these labels are synonymous as there were no non-SVO V-medial languages in the set and the same is true for SOV and "V-Final" (Data presented herein from Bybee et al. converts the V-[Location] terminology to more conventional word order labels so as to be consistent with our discussion of other data). An additional comment that should be made on the methodology of the Bybee, Pagliuca, and Perkins treatment of the GRAMCATS sample is that they aggregate all languages within each word order type together. This means that the total number of items across all languages of a given word order type in their sample will be summed and this is the number reported in their aggregated data. Consequently, heavily affixing languages strongly weight the sample and lightly-affixing (and non-affixing) languages have essentially no impact on the results — even if the affix distribution within a lightly affixing language is starkly biased in favor of one affix type. This stands in contrast to the more conventional aggregations we see elsewhere and has the drawback that strongly affixing languages will have disproportionate sway over the sample. The discussion in other studies such as Greenberg (1963) as well as Hawkins and Gilligan (1988) and Hawkins and Cutler (1988) takes a different approach and aggregates languages as units, which gives a greater resolution as to the tendency across languages as a whole to be prefixing or suffixing rather than weighting weakly affixing languages out of the analysis.

The simple tabulation of the affixation asymmetry as seen in the GRAMCATS sample is presented in Table 4.2.1. It contains verbal affix positions (relative to the root), aggregated from

the work of Bybee, Pagliuca, and Perkins (1990) using their bound/non-bound classification cross-referenced with the preposed or postposed position.

Table 4.2.1: Verbal Grammatical Item Location (Bybee et al 1990)

	Affix Location	Non-bound	Bound	All
All languages (71)	Preposed	48% (386)	52% (426)	34% (812)
	Postposed	20% (316)	80% (1236)	66% (1552)
SOV (32)	Preposed	32% (75)	68% (158)	18% (233)
	Postposed	20% (205)	80% (813)	82% (1018)
SVO (31)	Preposed	60% (298)	40% (200)	54% (498)
	Postposed	19% (82)	81% (341)	46% (423)
VSO & VOS (8)	Preposed	16% (13)	84% (68)	42% (81)
	Postposed	26% (29)	74% (82)	58% (111)

The breakdown in Table 4.2.1 shows the raw data, but with subsequent manipulation and processing, the disparate rates become even more marked; for example, SOV reaches a 93% suffixation rate for grams in the language sample above with the removal of outlier languages (Bybee et al 1990). The term "outlier" here is perhaps misleading however. In the original discussion of the data, Bybee et al. discuss how eight "exceptional" languages in their sample violate the pattern we would expect for V-final languages. Eight outlier, or "exceptional", languages is a substantial proportion of their seventy-one language sample, especially when we consider that these eight are solely from the V-final subset of 32 languages. The classification of these eight as exceptional is not entirely appropriate though, because half of them are languages with either nearly no affixation or a lower prevalence of bound grams — this is the issue that arises with their aggregation methodology discussed above: languages with little affixation essentially do not contribute to the aggregated affix totals and disproportionately impact the percentage of unaffixed grams. The remaining unusual languages represent a much more palatable number — four unusual languages is within the range of what we might expect for

sample volatility. Even with these foibles of the data aggregation methodology, this bound/non-bound distinction in the data reveals something that is obscured in the other data aggregation strategies which we will discuss later on. Instead of demonstrating typological tendencies on a language-by-language basis, this shows the relative resistance to or propensity for affixation for grammatical items in preposed versus postposed positions. For example, we can see that across the entire GRAMCATS sample, roughly 80% of all postposed grammatical items are affixed, and only in the rarest category, the V-initial languages (VOS and VSO), do preposed grammatical elements have a greater affixation tendency than postposed elements.

The basic correlations we have seen between word orders and morpheme orders in the GRAMCATS sample are also shown in the other major samples that have been conducted for this purpose. Joseph Greenberg was the first to leverage a sampling methodology to document this phenomenon in concrete terms, with his sample of thirty languages (Greenberg 1963). He used a simple categorization of languages according to criteria (word order? has prefixes? has suffixes? etc.) and aggregated the data by this language-count approach. Later studies have also applied the basics of his language-count approach (in contrast with the item-count approach of Bybee et al), but with a greater emphasis on probabilistic patterning rather than exceptionless patterns (Greenberg discussed both, but exceptionless universals are prone to being disproven by a single counterexample whereas probabilistic universals are more robust). Due to the amount of data available today, there has been a split between sampling and bulk analysis with this methodology. The *World Atlas of Language Structures* database (Dryer & Haspelmath 2013) has been used by Jacques (2013) toward this end, and is useful in dialogue with the Bybee et al. results because it allows us to look at the strength of affixation across language types. Table 1

from Jacques (2013) is reproduced below as Table 4.2.2 and then transformed and presented again in Table 4.2.3.

Table 4.2.2: Correlation between Word Order and Inflectional Morphology (Jacques 2013)

	OV		VO		No dominant order		Total	
	N	%	N	%	N	%	N	%
Little affixation	35	(25.0)	100	(71.4)	5	(3.6)	140	(14.8)
Strongly suffixing	269	(68.6)	93	(23.7)	30	(7.7)	392	(41.5)
Weakly suffixing	70	(57.9)	44	(36.4)	7	(5.8)	121	(12.8)
Equal prefixing and suffixing	49	(34.5)	78	(54.9)	15	(10.6)	142	(15)
Weakly prefixing	23	(25.0)	61	(66.3)	8	(8.7)	92	(9.7)
Strongly prefixing	6	(10.3)	52	(89.7)	0	(0.0)	58	(6.1)
Total	452	(47.8)	428	(45.3)	65	(6.9)	945	(100)

Percentages in each row sum to 100%

Table 4.2.3: Correlation between Word Order and Inflectional Morphology (Jacques 2013)

	Little Affixation		Strongly Suffixing		Weakly Suffixing		Equal Prefixing & Suffixing		Weakly Prefixing		Strongly Prefixing		Total	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%
OV	35	(7.7)	269	(59.5)	70	(15.5)	49	(10.8)	23	(5.1)	6	(1.3)	452	(100)
VO	100	(23.4)	93	(21.7)	44	(10.3)	78	(18.2)	61	(14.3)	52	(12.1)	428	(100)
NDO*	5	(7.7)	30	(46.2)	7	(10.8)	15	(23.1)	8	(12.3)	0	(0.0)	65	(100)
Total	140	(14.8)	392	(41.5)	121	(12.8)	142	(15.0)	92	(9.7)	58	(6.1)	945	(100)

**NDO = No Dominant Order* *Percentages in each row sum to 100%*

We can see from Jacques' presentation of the WALS data in Table 4.2.2 that not only are the suffixing languages dominated by OV languages, but also that the languages with little affixation are predominantly VO. This informs us as to the weighting we should expect from an item-count methodology such as that of Bybee et al. and reinforces some of the benefits of a Greenbergian language-count methodology in identifying the big picture correlations across languages. When this table is transformed to measure affixing tendencies by word order typology (Table 4.2.3), we also see that, in addition to certain affixing strategies being dominated by different word orders, each broad word order category has tendencies for certain morpheme

orders. For example, OV languages are less likely to be predominantly suffixing and are less likely than VO languages to have little or no affixation. Despite collapsing the categories of VSO and VOS with that of SVO (unlike the other categorizations discussed in this chapter), this population-level data affirms the trends seen in the language samples: the majority of suffixing languages are OV (Table 4.2.2) and the majority of OV languages are suffixing (Table 4.2.3).

With this in mind, we can turn to the data from the Gilligan language sample presented in Hawkins and Gilligan (1988, Appendix III). Table 4.2.4 and Table 4.2.5 demonstrate the broad patterns of extant affixing strategies per language, in comparison to the data from Table 4.2.1 which breaks it down by item frequency within the data set.

Table 4.2.4: Affixing Strategies Relative to Adposition Order (Hawkins & Gilligan 1988)

	Prefix Only	Both	Suffix Only	None
All languages	2	32	15	1
Prepositional	2	22	5	1
Postpositional	0	10	10	0

Table 4.2.5: Affixing Strategies Relative to Word Order (Hawkins & Gilligan 1988)

	Prefix Only	Both	Suffix Only	None
All languages	3	32	15	0
SOV	0	8	11	0
SVO	3	13	4	0
VOS & VSO	0	11	0	0

From the above tables, it is clear that there is a correlation between affixing strategy and word order, with prepositional and postpositional being mostly serviceable as a proxy variable for general word order, both as seen by the same general pattern in Tables 4.2.4 and 4.2.5, and based on the typological correlation between adpositions and word order (cf. Dryer 2013c).

SVO is separated from VOS and VSO here as in the Bybee et al. analysis due to the difference in patterning compared to SVO (Hawkins & Gilligan 1988, Bybee et al 1990), but in subsequent discussion we will condense these into one category (VO) and contrast it with SOV (OV).

The difference in SVO and VSO/VOS patterning can be seen most clearly when affixes are distinguished according to their grammatical and semantic types. Not all affixes follow the same tendencies, but these tendencies are also split across language types. Hawkins and Gilligan show this quite clearly with the following breakdown in their data (1988, Appendix III). Table 4.2.6 shows their aggregated results for affixation on the verb.

Table 4.2.6: Affixing Strategies on the Verb Relative to Word Order

(Hawkins & Gilligan 1988)	**Prefix Only**	**Both**	**Suffix Only**	**None**
Tense Affixes (on Verb)				
VOS & VSO	4	0	1	6
SVO	4	1	5	10
SOV	0	0	15	4
Aspect Affixes (on Verb)				
VOS & VSO	4	1	5	1
SVO	4	2	4	10
SOV	2	1	9	7
Mood Affixes (on Verb)				
VOS & VSO	3	0	3	5
SVO	1	0	3	16
SOV	1	0	14	4
Verbal Negation Affixes (on Verb)				
VOS & VSO	4	0	1	6
SVO	5	0	0	15
SOV	3	0	5	11
Subject Person-Marking Affixes (on Verb)				
VOS & VSO	6	1	3	1
SVO	5	2	5	8
SOV	3	1	12	3

As we can see, the correlation of word order with prefixing and suffixing on the verb is not consistent. Verbal negation actually shows a sample-wide preference for prefixation, and even SOV is only weakly balanced towards suffixation for this feature. Tense affixes also are worth mentioning to illustrate the correlations. Tense affixes show the classic VO-OV split quite cleanly with SVO and VOS/VSO tracking closely together with support for prefixation (although SVO is still weakly suffixing) and the SOV languages being exclusively suffixing (among those with any affixation for this feature). It is fair to note however that SVO and VOS/VSO *are* materially different due to SVO being essentially evenly balanced on affixation strategy for tense in contrast to VOS/VSO being seemingly nearly exclusively prefixing.

The same kinds of differences across semantic classes are present with affixation onto the noun, as seen in Table 4.2.7 below, again sourced from Hawkins and Gilligan (1988).

Table 4.2.7: Affixing Strategies on the Noun Relative to Word Order

(Hawkins & Gilligan 1988)	Prefix Only	Both	Suffix Only	None
Definiteness Affixes (on Noun)				
VOS & VSO	2	0	0	9
SVO	2	0	3	15
SOV	1	0	2	16
Possessive Affixes (on Noun)				
VOS & VSO	3	2	2	4
SVO	4	0	5	11
SOV	3	2	7	7
Gender Affixes (on Noun)				
VOS & VSO	3	0	3	5
SVO	2	0	5	13
SOV	0	0	5	14
Plural Affixes (on Noun)				
VOS & VSO	2	1	4	4
SVO	3	1	9	7
SOV	0	1	16	2
Nominalizing Affixes (on Noun)				
VOS & VSO	0	1	5	5
SVO	2	1	12	5
SOV	1	0	13	5
Case Affixes (on Noun)				
VOS & VSO	0	0	2	9
SVO	0	0	3	17
SOV	0	0	15	4

As with the verb data, we can see that across all the categories the VOS/VSO languages trend more towards prefixing than SVO languages and that SOV languages suffix more than the other types. Additionally, we can see from the case affixes data that even when none of the languages prefix, there still is an asymmetry across the languages — SOV is very likely to suffix cases, but

even though the other language types will suffix for cases too, the non-SOV types are likely to instead just not have case affixes at all.

We can see from this data that VO languages generally have both suffixing and prefixing, but they are also the only set in which we witness any exclusive prefixing languages. We can also see that OV languages have a tendency to be exclusively suffixing, but like VO languages, there is a substantial number of OV languages which support both prefixing and suffixing. Lastly, while these statements generally hold across semantic categories of affixes, there is substantial variation between those categories.

4.3 Grammaticalization

Much of the subsequent discussion requires a return to our understanding of grammaticalization. While the formal application of the term and some of the stronger formulations have been disputed at times (cf. Campbell 2001), the general framework of evolutionary clines is reasonably well accepted in the field. Grammaticalization is simply the movement of a lexeme or morpheme from a lesser to a greater role in the grammar of a language (cf. Campbell 2001, Heine & Hünnemeyer 1991, Hopper & Traugott 1993, Lehmann 1995, etc), with our discussion of *vuestra merced* in the preceding chapter offering an excellent in-depth example. This is obviously a very broad category of changes, but the relevance to our interest here is quite specific. We can think of grammaticalization as being a unidirectional process correlating with increased frequency of use. First, with increased frequency, we can expect any token to reduce phonetically (cf. Zipf 1929), and with increased frequency, the semantic domain of that token is similarly likely to drift toward greater functionality (Zipf 1932). Grammaticalization places these phenomena in the grander context, by saying that as an item becomes more heavily used

and moves towards a role in the grammar, it will semantically bleach (lose its significance as a citation of lexical meaning) and phonetically lighten (reduce its empirical form) (cf. Hopper & Traugott 1993, p96; Heine & Kuteva 2002, p3-5; etc). This is a progressive process of erosion, so we can expect that as an item continues to drift towards greater syntactic significance, it will also continue to erode phonetically and semantically. This pattern of erosion has been applied to the analysis of the origin of affixes and clitics to get the following parallel clines (as presented in Hopper & Traugott 1993 p106-108, and based predominantly on work by Christian Lehmann and Joan Bybee):

1) Relational Noun → Secondary Adposition → Primary Adposition → Agglutinative Case Affix → Fusional Case Affix
2) Full Verb → (Vector Verb →) Auxiliary → Clitic → Affix

For context, relational nouns are nouns capable of being interpreted as giving information about the location relative to some other entity, such as "top" or most body extremities. Secondary and primary adpositions are differentiated by whether their role is to define a more definite relational location or if they have expanded in scope to bear a greater grammatical utility. We can see this in more concrete terms by comparing "beside the table" ("beside" is a secondary adposition) to "go to school" ("to" is a primary adposition). Vector verbs are an optional portion of the verbal cline and might be grouped as a possible sub-class of auxiliary where the verb still carries information of tense, aspect, mood, or relational information, but functions as part of a "compound verb" complex with another verb (Hopper & Traugott 1993).

As seen in our previous discussion of diachronic and synchronic typological analyses being closely intertwined, grammaticalization necessarily defines both the synchronic and

diachronic sets of potentialities, limiting what can exist in the present and what kinds of change can exist over time (Heine 1993, p23).

4.4 Approaches from Syntax — Positional Analysis

In this and the following section, I will break down the foundational work done on this problem to allow us to piece together a unified account for the nature of the affixation asymmetry. Most of the research done on this problem has incorporated multiple approaches. I categorize these studies here according to where they have made the most distinctive contributions to the analysis of the suffixing preference. This means that if a hypothetical paper were to make strong claims on syntactic ordering and more general claims about processing which were then expounded upon and further developed by later authors, that paper would be grouped with those focused on syntactic analysis, even if the majority of the paper had been focused on processing (and the later authors would be included in the psycholinguistic grouping). While arbitrary, this distinction should allow us to more easily digest the significant differences of the prior work on this subject and focus on the contributions which remain current.

The syntactic arguments for the affixation asymmetry are well exemplified in the accounts of two studies which we have already discussed in Section 4.2: that of Bybee, Pagliuca, and Perkins (1990) and that of Hawkins and Gilligan (1988). Both supplanted earlier work by Theo Venneman on the "natural serialization" principle which proposed an evolution of languages "toward a consistent order between […] head and dependent" (as cited and characterized in Mithun 2003). This will ring especially familiar when we consider the "Head Ordering Principle" of Hawkins and Gilligan (1988).

The study by Hawkins and Gilligan focused on the comparison of the syntactic typology to the morphological (with an emphasis on inflectional morphology to ease crosslinguistic comparison). Essentially, if we consider an item of interest prior to its conversion to an affix, that item will typically be serving as either an auxiliary or adposition (depending on whether the initial pre-grammaticalization lexeme was a verb or noun, per the clines laid out in the preceding section). Accordingly, we can think of the proto-affix as the syntactic head of the phrase that dominates the eventual host. As the proto-affix continues through the process of grammaticalization, its position remains the same. Therefore, when we compare the position of the affix with the structure of the syntax, we should expect them to line up. Hawkins and Gilligan go a step further in proposing that under a theory of morphology with morphological heads, the affix can remain the head of the resulting construction. Based on this, Hawkins and Gilligan argue that the syntactic headedness of the language directly relates to affixation strategy, under their Head Ordering Principle. The fundamentals of this proposition are reasonable — the serialization of the components should be expected to be governed by the syntax. Based on this, they also focus heavily on the adposition typology of their inspected languages, but this would seem a poor additional explanatory variable due to the high correlation between adposition and word order typologies (cf Dryer 2013c as well as Hawkins & Gilligan 1988). Their analysis especially draws criticism however from the proposition that the affixes remain heads in the morphology (Hall 1988). Hall (1987, as cited in Hall 1988, p325) especially attacks this under the application of this analysis to inflectional morphology, with the counter that not only are many affixes not the heads, but for inflectional affixes, they can *never* be the heads. Hall's criticism presses further, arguing that this is a descriptive correlative analysis without explanatory power, but his criticism incidentally cedes the strength behind Hawkins and

Gilligan's proposal, by terming this correlation a "historical accident" (1988, p325). If the syntactic history of an item predicts its position, that is a meaningful explanation, especially when we are considering the genesis of the item as relevant to the modern synchronic pattern. Furthermore, Hall's critique seems grounded in the synchronic analysis of headedness according to the given language today, which is not necessarily applicable to the historical nature of these phenomena when they grammaticalized — we can easily turn to the Spanish example of Chapter 3 to demonstrate that *vuestra merced* was not originally the same type of linguistic unit as its descendent form *usted*. Accordingly, I suggest that the Head Order Principle can be interpreted to explain the location of an affix at the point that it grammaticalizes sufficiently to affix status, and the specific counter-orderings such as nominalizing affixes which go against word order typology predictions (refer to Table 6) are therefore not a problem for the synchronic state and instead speak to the position of their ancestor forms. Cutler et al. (1997) will make similar commentary on the Head Ordering Principle, although they see it operating in combination with an Order of Computation concept which will be in line with much of the discussion of the next section.

The study by Bybee, Pagliuca, and Perkins (1990) applied a mixed set of methods including semantic, syntactic, phonological and psycholinguistic approaches. We should briefly revisit the methodology employed in this study, as discussed in Section 4.2. The authors used the GRAMCATS sample discussed earlier to test specific hypotheses for viability. This focused exclusively on affixes on the verb (as is visible in Table 1), and as mentioned before, they favored an aggregation of all grammatical items across their language set, rather than keeping the data categorized by languages. They tested multiple hypotheses, ranging from the semantic to the phonological. They argued that the preposed items induced greater phonological

coordination from the stem than postposed items and therefore should be compatible for affixation, but that that affixation is blocked until the preposed item is "semantically appropriate". Under this analysis, we expect a phonological integration of the preposed item and the stem (essentially focusing on the prosodic phrase and the extent of coarticulator effects), whereas postposed items demonstrate less of this effect. This might as well simply be put in terms of the grammaticalization process described in 4.3. Therefore this is more of a statement of viable potentiality than a differentiating force between suffixes and prefixes. Instead, Bybee and her colleagues focus on clausal boundaries as a point where affixation should be predicted. They argue that this, in conjunction with the natural syntactic ordering of the items in a sentence, is what leads to affixal patterns. Drawing this back to the diachronic nature of this process, they offer explicit endorsement for the concept of word order fossilization in affix position. They reject however the concept that there might be a processing difference between the onset and the ending of a word, in contrast with the research to be discussed in Section 4.5 (cf. Hupp et al 2009, Marslen-Wilson & Switserlood 1989, Hawkins & Cutler 1988, Tyler et al 1988). The analysis supporting this rejection is weakly substantiated however and based entirely upon the relative strength of prefixed syllables, meaning that they looked at how prefixes are articulated more strongly and concluded that this meant that the primacy of the onset could not play a role.

As we will see in the Section 4.5, this portion of the Bybee et al. analysis has been thoroughly contradicted by more recent scholarship. This study stands out most significantly on the syntactic axis. Their conclusions in this domain center on a number of correlations, with specific attention to Venneman's Natural Serialization and to the Fossilized Syntax proposition. First, there is a general preference for the postposing of grammatical items (both free-standing and affixed). And second, they draw a close connection between the ordering of the items in a

language and the affix position, thus accounting for the semantic asymmetries of affixes noted in both their study and Hawkins and Gilligan, by considering where the proto-affix would have been placed when it was still a standalone item unattached to a host. This is essentially the same correlation as identified in Hawkins and Gilligan but without the appeal to the abstraction of language headedness, and should be interpreted as fundamentally aligned with the crux of the findings from Hawkins and Gilligan.

4.5 Approaches from Phonology and Psycholinguistics

While ostensibly coming at the problem from two different angles, the phonological analyses and the psycholinguistic analyses have much in common. In contrast with Bybee, Pagliuca, and Perkins' (1990) conclusion against a processing narrative, work by Hupp, Sloutsky, and Culicover (2009) and Himmelmann (2014) provides a strong defense for the integration of processing into the analysis of the affixation asymmetry. Whether the analysis is based on gating with the onset (Hawkins & Cutler 1988, with significant reliance on Grosjean 1980), or on the simple prosodic features of the utterance (Himmelman 2014), the general basis of the analysis is on the nature of the manner in which the word is produced by the speaker or received and processed by the listener. This often relies on the phonetic lightening aspect of grammaticalization leading to small and unstressed items prior to their final erosion to affix status (cf. Himmelmann 2014). Accordingly, we can aggregate these approaches under a single processing narrative fairly easily. Listeners rely significantly on prosody for detecting word boundaries (cf. Cutler et al 1997), but the story for prosody needs to be paired with an inclusion of the temporal asymmetry of the sound stream. Essentially, one can take the basics of a Bybeean approach to morphology (Bybee 1988), with regards to storage and recall, where

content is stored according to overlap in speech streams, but that approach then needs subsequent modification due to the significance of sequencing (cf. the "left-to-right access" discussed in Hawkins & Cutler 1988 and the importance of onsets discussed in Marslen-Wilson and Zwitserlood 1989). The studies by Himmelmann (2014) and Hupp, Sloutsky, and Culicover (2009) are defining works in this respect, especially when put in dialogue with work by Marslen-Wilson and colleagues (Tyler et al 1988, Marslen-Wilson and Zwitserlood 1989).

The work of Grosjean (1980) demonstrated increased salience of the onset for recall, but Hupp, Sloutsky, and Culicover (2009) showed that the primacy of the onset extends much further. Based in part on the asymmetric acquisition of affixes, where prefixes are acquired more slowly and less easily than suffixes (Kuczaj 1979, Clark 1998), and on the typological patterns discussed above, Hupp et al. conducted a study with a set of experiments evaluating the tendency of subjects to categorize sameness by the onset or coda, using syllables, images, and musical tones via a forced choice task for similarity (the gist of the task can be illustrated thus: "Is <*be-ta-te*> or <*ta-te-be*> most similar to <*ta-te*>?"). While their interest was in the affixing asymmetry in language, their study demonstrated that the primacy of the onset actually extended beyond the domain of just language, holding true for non-language audio and visual input as well. Their study indicates that in a sound sequence, the final sound will be inherently more likely to be perceived as more similar to the initial sound and less consequential on the meaning.

The studies by Marslen-Wilson and Zwitserlood (1989) and Tyler et al. (1988) investigated the significance of the onset in the process of word recognition. Similar to the Hupp et al. study, the Marslen-Wilson and Zwitserlood study focused on the salience of the onset for word recognition, comparing onset versus rhyme primes for word recognition by association with a related visual probe (for example, if the test item partially overlapped in form with the

word “honey”, the probe might be the word “bee”). Onset primes tended to be associated with a related visual probe quite strongly, even if the rhyme broke the connection. Rhyme primes were much less effective. Tyler, Marslen-Wilson, Rentoul, and Hanney (1988) also investigated the effect of the onset on parsing, but instead focused on derivational prefixes in the speech stream through three experiments. All three experiments tested the word-recognition point of prefixed and non-prefixed stems. The goal here was to investigate whether the mind processes the stem in the same way regardless of the presence of prefixes — essentially stripping the prefix off and parsing the stem independently. If the stripping analysis were correct, the word recognition point for “count” and “recount” would be the same (from a segmental perspective) or the same plus the duration of the prefix (if approached as a simple time duration). The prefix-stripping option was counter-indicated by results from all three experiments however, with the word recognition point shifting forward with the addition of the prefix, meaning that the word was recognized even without reaching what the recognition point would have been for the bare stem. Both of these are relevant to the investigation of the affixation asymmetry because they indicate that the word-level parsing of prefixes is inclined toward interpreting prefixed words in the same way as if they were unprefixed — the word-level processing begins at word-start and does not sandbox interpretation of the prefix separate from the stem.

Himmelmann (2014) approaches the affixation asymmetry from a mixed phonological and psycholinguistic approach, with an unusual leveraging of production failures in his analysis. He focuses on the phrasal prosody of the utterance and the possible generalizations from this perspective. The core hypothesis is that function words are most likely to affix onto a host word if they are within the same prosodic word or phrase (rather than the same syntactic phrase). This means that even if a function word occurs adjacent to a prospective stem with high frequency,

further grammaticalization to the point of affixation is unlikely if the two words happen to fall on opposite sides of a prosodic boundary. First, when speaker disfluencies occur, they normally group unstressed function words with the preceding item, regardless of the actual syntactic structure, essentially demonstrating a lack of mandatory parallelism between the phonology and morphosyntax at the phrasal level. The cause of this lack of parallelism may be due to processing, but need not necessarily be so — a viable alternative or additive hypothesis proposed by Himmelmann is that it may be a social strategy to hold the floor as the overt incompleteness of the clause demonstrates the incompleteness of the utterance. This foments asymmetry because while both preposed and postposed function words will indicate a commitment to further speech, a preposed function word will break the prosodic unit, while a postposed function word already represents the end of the prosodic unit of it and its phrasal host. Second, Himmelmann discusses how the categorization of clitics (eg. enclitic versus proclitic) differs based on whether one is asking the question from a syntactic position or a phonological position. This difference is manifested by a peculiar patterning of ditropic clitics (clitics which show both enclitic and proclitic features), where syntactic proclitics are often realized as phonological enclitics in actual spoken practice. The unstressed nature of these function words essentially pushes them to be encapsulated within the prosodic contour of the preceding item. Keeping in mind the position of clitics on our grammaticalization clines, this means that the candidate proto-affixes are commonly associating with a preceding host, even if that host does not have logical ownership of the clitic. Himmelmann attributes the affixation imbalance to these asymmetries in preposed and postposed items in regards to the prosodic chunking of utterances, with the potential additional factor of an advantage in signaling that the speaker is holding the floor in turn-taking scenarios.

Himmelmann's prosody account joins with the psycholinguistic family of accounts to demonstrate an inherent bias in both production and perception to associate a trailing item with the preceding. To put it in context, it is worth mentioning explicitly that across the studies discussed, there has been a broad spectrum of psycholinguistic components demonstrated as applicable to this phenomenon at opposite ends of language use: Himmelmann's disfluency data demonstrates relevance in production, the work by Hupp, Sloutsky, and Culicover shows the same in perception, and the studies by Marslen-Wilson et al. and Tyler et al. demonstrate that the prefixed onsets are processed linearly without an inherent stripping of the prefix to parse the stem independently (meaning that in real-time processing, the word is not interpreted as a prefix plus a stem). It is worth noting however while reviewing these studies, that while they make strong arguments for a preference towards suffixation, or for an inherent processing repercussion for prefixation, they do not tend to differentiate well between the affixation patterns in different languages and the correlations with word order typology.

4.6 A Unified Analysis

The word order analyses, the prosodic analyses, and the processing analyses all bring increased clarity to the problem, but each lacks something that is provided in the others. The answer is to give a combination of all three, with a component about ongoing language change occurrring in situ. The attachment of proto-affixes to prospective hosts is predicated on word order and that word order obviously has a close relationship with syntactic headedness. In this way, we can see that Bybee, Pagliuca, and Perkins generally agree with Hawkins and Gilligan. The Head Ordering Principle (in the weakened form I proposed) and the more generic Bybee asymmetry together give a strong account for initial placement of the lexical item and its correlation with the

end placement as an affix in the morphology of the language. We can also expect that the asymmetry of affixation by semantic category of the affix is a consequence of this original serialization (based on the common proto-constructions which lead to each semantic class of affix — as per Hopper & Traugott 1993, and shown in Section 4.3). Keeping all of this in mind, we can analyze the evenness of SVO languages and the one-sidedness of the SOV languages as a case of aligned or competing pressures.

In OV languages, the head ordering, the prosodic packaging, and the psychological processing all are pushing in the same direction — the item undergoing grammaticalization is likely to be serving as the phrasal head during its erosion and is therefore in the postposed position of a suffix when it degrades enough to need to attach to a host. Additionally, it naturally falls into the patterns predicted by processing and prosody. The primacy of the onset and the natural parsing of the word are aligned with the bulk of the information payload of the affixed words (due to the semantic bleaching of a grammaticalized item, the host stem carries a greater amount of meaning at this point).

However, in VO languages, the analysis gets more complex. First, the word order should favor prefixing, and in the data (especially that of Bybee, Pagliuca, and Perkins) there is evidenced that *some* VO languages are prefix-dominant. However, these examples are few and most language samples have demonstrated a near-even split on prefixation and suffixation in VO languages. This split however meshes with the contrasting pressures being exerted on the language. While the availability of candidate items is predominantly preposed, the *maintenance* of productive affixation solidly favors the post-posed items due to prosodic and processing advantages. This is in addition to the inherent advantage that post-posed items may have for affixation in the abstract per Himmelmann. This means that we should expect VO languages to

have greater opportunity for prefixes to develop relative to suffixes due to the Head Ordering Principle and general word ordering, but still have the ability to develop suffixes due both to gram postposition as outlined by Bybee, Pagliuca, and Perkins (1990) and to the prosodic and processing advantages inherent to that position.

This brings us to the biggest problem with the suffixing preference: if languages can change word order, why do we not see OV languages with prefixing? Here we partially fall back on a proposal advanced by Givón (1979). Givón asserts affixation patterns can be left in place as a language continues to change. A very strong formulation of this would assert that all languages were originally OV and then posit a historical narrative for the bias witnessed today (and indeed Givón makes this claim). However, this strong formulation begins to fall apart when we consider the data supporting word order as an areal effect (cf. Dryer 2013a) and by work such as that of Harris (1978) indicating that languages may go through word order cycles over time (Harris's work centered on French and Romance languages and posits a two-stage oscillating cycle wherein Romance languages over time repeatedly transition from SOV to SVO and back again). Instead, we take the initial supposition from Givón that historical change could put affix patterns out of place, but we apply it towards prefixes instead of suffixes. The important question to consider is the maintenance of prefixes in the context of the pressures of language use. After their initial development in line with the Head Ordering Principle, syntactic position is no longer a relevant feature for prefixes or suffixes — they already are locked in place and we have no evidence or cause to predict something radical like extant prefixes jumping position to be reintegrated as suffixes to better align with the syntax should the language's syntax change. However, we can see that the advantages and pressures we find in processing and prosody are not forces which go away with affixation. They remain very much in play.

Speakers will still perceive sound with an onset bias as demonstrated in by Hupp et al. (2009), and they will still recognize the word by working from the actual onset, not the onset of the stem as demonstrated by Marslen-Wilson et al. (1989) and Tyler et al. (1988). Accordingly, we should expect an erosion of prefixes once in place, and this erosion should result in the lopsided distribution of suffixes relative to prefixes, because only some languages are even in the position to develop prefixes to a significant degree. This erosion of prefixes however should not be the same kind of erosion that we saw in the proposed grammaticalization cline. Due to the primacy of the onset, we should predict that the erosion of the prefix is not eroding the form per se, but rather its productivity, leading to frozen forms maintained in the lexicon as whole lexemes.

There is evidence to support this, for example, in the current ongoing loss of productivity for prefixes in English (as discussed in Bauer 2003). English is typical of SVO languages in that it demonstrates both prefixation and suffixation, but while the word-level morphology of English is eroding substantially, the productivity of prefixes is eroding to a faster degree than that of suffixes. Bauer attributes this to a potential typological shift, and her analysis focuses on two patterns: on the re-lexicalization of certain prefixes (such as *pro-* and *anti-*) and the loss or decline of productivity of others (such as *be-* and *a-* in the words *bespectacled* and *alight*). She attributes this loss of productivity to the death of the prefix, but her findings also show maintenance of specific frozen constructions. This phenomenon therefore fits exactly with what we should predict from the forces demonstrated on affixes. Because of the primacy of the onset, the pressure on the affix is for it to be processed with the stem together as a single lexical item, and additional phonological fortification (in the form of stress, or the non-reduction of vowels, for example) is necessary to maintain the independence of the affix. This strengthening of the phonological form effectively pushes the affix back up the cline towards clitic status, as has

happened with *pro-* and *anti-* in English (cf. *pro-American*, *anti-American,* and "I am *pro-*backing up your computer daily"). Accordingly, the relexicalization of the productive affixes and the loss of productivity and creation of frozen forms with the remainder essentially show the two paths we should expect for affixes under this processing effect — either they erode and fuse to the stem or they are expressly emphasized and no longer fit the word-level prosodic contour. Note how this actually reinforces the primacy of the onset in parsing — the phonological buttressing of the prefix which pushes it back towards functioning like a clitic is essentially resetting the onset and giving two separate streams of sounds to be processed.

In a similar vein, going now beyond Bauer's work, those same pressures can create new words by misanalysis. Recent research demonstrates that in Northern Mao, affirmative and subject prefixes fused and detached from the verb, eventually becoming pronouns which are not cognate with the pronouns of other languages in the Mao family (Ahland 2013). Just like in Bauer's example with English, what we see is that there is a perceptual salience to the beginning of the word (the primacy of the onset) and that prefixes might be especially at risk for becoming frozen. In this case, the affirmative prefix and the subject prefixes fused, as we would expect, given the prior discussion — the productivity of prefixes is under constant pressure. But while they did fuse, their perceptual salience resulted in an unusual outcome, where instead of becoming frozen on the verb, they instead became frozen forms which degrammaticalized to independent lexemes which became suppletive forms within the pronominal system. This of course does not remove or preclude prefixes as a phenomenon, but it gives an asymmetrical pressure for the loss of prefixes over time. Thus, the imbalance in affixation can be maintained and explained throughout multiple stages of a language's evolution or cyclic movement based on

circumstances of the genesis of the affixes in contrast to the circumstances of the maintenance of the affixes.

As a caveat, I would be remiss not to call attention to something left unaddressed in this analysis. While I have no reason to doubt the theory advanced by Hawkins and Cutler (1988) that infixing is so rare as a feature across languages due to a dispreference to inhibit recall by creating disfluencies in the basic units of the mental lexicon, it does leave an unsatisfactory note in that it does not speak to how such languages come to develop. This unfortunately is mirrored in my own analysis. The combined analysis of prosody, processing, and ordering/headedness gives a clean framework for the development of prefixes and suffixes and the regularity of the typology of their occurrence today, but just as in many past analyses, this has been by an implicit reduction to a binary pair of affixes with the analysis focused only on the two most prevalent types.

4.7 Discussion

The suffixing preference can best be thought of as a set of competing opportunities and pressures in the ongoing evolution of a language. We can see that the word order typology of the language defines and constrains the formation of affixes according to syntactic headedness and serialization, with suffixes being advantaged to form in OV languages and prefixes being advantaged to form in VO languages, all else being equal. This advantage however is not symmetrical, because we can see that VO languages are less likely to productively affix in general. Part of that may be due to a prosodic blocking effect in line with Himmelmann's phonological phrasal boundaries (also visible in the affix "flirting" proposal of Hall 1988), but this can be better conceived of as multiple pressures acting independently, sometimes in

alignment and sometimes in opposition. Based on the syntactic positioning and ordering of a language, we can predict a certain affixing strategy will be advantaged to arise, and that in OV languages the syntactic and prosodic pressures are aligned, leading to strong suffixation (both in comparison to prefixing and to not affixing). In VO languages, the two pressures are in opposition, with the prosodic factors pushing against prefixation while the syntax leaves the greatest opportunity for prefixation, with the balance coming out either against affixing or in prefixation's favor. But this only speaks to the genesis of the affixes. Over time the psycholinguistic factors relating to perception, recognition, and production all advantage the maintenance of suffixes as opposed to prefixes, regardless of the word order typology of the language. Marslen-Wilson et al. (1989), Tyler et al. (1988), and Hupp et al. (2009) each give strong evidence for the prefix's integration into the host stem in terms of perception and processing. Prefixes are therefore under erosive pressure and are likely to see their productivity decline until they remain frozen and treated as part of the root. While this process can be stymied by dedicated emphasis on the prefix this in reality functions to disjoin the prefix from the stem for parsing and pushes the prefix back towards clitic status and relexicalization. This for example may be the case in negation examples, where negating particles might be incentivized to be preposed under linear ordering theories such as Hawkins' Maximize Online Processing (2014). But we can already see how this creates a vulnerability as Bauer's work (2003) has shown negative morphemes like "anti-" relexifying, perhaps even due to the very need to emphasize that negation. In turn, this strong erosive pressure on prefixes serves to preserve the imbalance we see across word order typologies, because even a change in word order will not result in a substantial violation of the correlation — while we might expect some counter-indicated affixes to remain extant for a while (such as prefixes in a language that

recently became OV), the constant erosive pressures on the language will over time lead the language to conform to the expected patterning (in coordination with the lopsidedness in affix genesis). This more complex analysis of the suffixing preference as an amalgam of multiple functional pressures acting in concert gives us an overarching analysis which both explains the asymmetry in the phenomenon and the maintenance of this asymmetry over time and successive language changes.

The analysis advanced here places heavy emphasis on the pressures and advantages experienced directly by the individual, as we would expect for our first category of research (functional pressures at the level of the individual) mentioned in Chapter 2's tripartite typology of research, but even so, there are still features which draw in the other pressures on change as seen for example in Himmelman's appeal to the pragmatics of turn-taking in communication. Furthermore, most of the work integrated into addressing this problem was based on synchronic data, but that synchronic work was able to be synthesized into diachronic claims. The integration of synchronic research into diachronic analyses represents one of the major rifts in the field of diachronic linguistics — when one is so entrenched within a given research or methodology within one's speciality, research allegedly targeting a different type of question is prone to being overlooked. While everything fed into the final analysis demonstrating a complex combination and interplay of pressures acting on the individual and yielding the broad typological pattern seen across the languages of the world, the components were from many disciplines: syntax, phonology, psycholinguistics, historical linguistics, and typology. Approach the problem from any one of those subdisciplines alone and we would not have been able to improve on our understanding — the greatest breakthroughs to be made in understanding

language change, I believe, will come from putting these fields in dialogue with one another, as I have tried to illustrate in this chapter.

CHAPTER 5

GREENBERGIAN WORD ORDER CORRELATIONS AND WORD ORDER CHANGE

5.1 Introducing Word Order Correlations

The word order typology of languages has long been a challenging feature for diachronic studies. Word order is one of the most salient features of a given language when analyzed, but it has been misused in past research in conjunction with other morphosyntactic features to draw erroneous conclusions about language genetics, such as the much-maligned Altaic hypothesis which groups Korean, Japanese, Tungusic, and Turkish into a single language family of common origin (cf. Clauson 1956, Vovin 2009). Similarly, word order typology has been the source of some of the boldest claims applicable to language change and evolution: Givón (1979), for example, made the claim that all languages are derived from a common SOV word order, and this claim was reprised by Gell-mann and Ruhlen (2011). Givón's hypothesis is useful for us to linger on because it frames some of the core issues that concern us in this chapter. The central claim of Givón, Gell-mann, and Ruhlen is that it is possible to enumerate the possible pathways of word order change and the enumeration of these pathways can allow for claims about the origin of language. Using the more current work by Gell-Mann and Ruhlen (2011), as the exemplar, they make the claim that there are language-internal pathways away from SOV, but none that lead back to it. They instead assign all new diachronic movements toward SOV to be the consequence of language contact and areal effects (the nature of such forces we will discuss later in this chapter). This hypothesis is radical, but further testing remains for it to be confirmed and substantiated, and indeed I would consider affirming their claim to be a massive undertaking

for future research. It is worth mentioning two features of the claims by Givón, Gell-Mann, and Ruhlen which are material to our interests in this chapter.

First, Givón's original hypothesis grew out of a specific set of observations: in his analysis of Bantu, Givón noticed that the word-internal ordering of linguistic components was out of alignment with the syntactic order of the sentence (1971). Givón used this as evidence that the present form of a language may hold artifacts of past configurations of the language. Second, Gell-Mann and Ruhlen (2011) make the claim that the pathways for change are not limitless — there is a finite range of changes that a language can make and some types of change can be ruled out. In the case studies of this chapter (Section 5.4), we will rely on both of these concepts. As we move past these less well born out or yet-to-be born out hypotheses however, we can direct our attention to some of the strongest breakthroughs in the field of typology and the challenges that they pose for diachronic linguistic theory.

As mentioned in Chapter 4, the patterns of affixation strategy correlate strongly with a language's word order typology. The most basic word order typology involves the six permutations of Subject (S), Object (O), and Verb (V). More specialized implementations may sometimes also incorporate other features such as Adpositional Phrases (X) (cf. Dryer, 1991, Graham 2016). When we describe a language with SOV word order, we can make a very good (albeit not definitive) guess as to which affixation order (re: prefixes versus suffixes) will be dominant in the language (cf. Greenberg 1963, Hawkins & Gilligan 1988, Bybee et al 1990). While asymmetries in the dominant affixation order have been documented at least as far back as the work of Sapir (1921), for our purposes, it is situated within a much larger group of observations made by Greenberg (1963) where he cites broad typological tendencies for all languages; of these, many of the universal tendencies he describes specifically correlate with the

word order type of each language. In this chapter, we will break down the problems posed by these Greenbergian word order correlations when approached diachronically and discuss how they might be addressed within the theoretical approach this dissertation seeks to advance. Specifically, the questions to be addressed will be how word order correlations can be maintained when languages are constantly changing, since such changes may affect word order which is a prominent component of many correlated feature pairs; how well each of our three categories of diachronic explanation (functional pressures and motivation at the level of the individual speaker, at the level of society, and at the level of linguistic structure) can address the maintenance of these correlations; and to what extent these correlations might be maintained by a simple predictable follow-through from a given source, as advocated by source-based theorists (cf. Aristar 1991; Cristofaro & Ramat 2017; Cristofaro 2017, 2019; Collins 2019).

5.2 Greenbergian Word Order Correlations

Greenberg (1963) laid out a number of very strong correlations with word order (relevant universals are presented in Table 5.2.1). His complete list included 45 universal tendencies across the world's languages, and while they are labeled as universal correlations associated with "the order of meaningful elements", only a subset actually apply to typological word order and correlated features, and some, such as Universal 37 ("A language never has more gender categories in nonsingular numbers than in the singular"), are wholly unconnected to word order (or ordering of any kind) at all. These universals were foundational for the field of typology and still fuel research over half a century later, but for these universals to be useful, they need to be kept in the proper context. First, these are all *tendencies*, not absolute laws. We should expect that there will be exceptions to all universals laid out by Greenberg, and most of them are already

worded to allow for this possibility. That being said: an overwhelming trend across world languages is sufficient for most meaningful characterizations of language attributes. Given our goal of outlining the approach one might take to analyzing diachronic universals, relying on trends of the scale to be labeled "universal" will be sufficient for our purposes. Second, these are explicitly laid out as correlations. We cannot infer a causal link within any of these correlations. The biggest impediment here is that causality in language evolution is already a holy grail for diachronic linguistics, but it is also important to remember that these were posed as *synchronic* universals. As mentioned in Chapter 1, and demonstrated in Chapter 4, the uniformitarian principle allows us to leverage synchronic findings towards answering diachronic questions, but as we will find in this chapter, the application of these universals to diachrony is far from simple and to imply that Greenberg's work demonstrated a causal diachronic relationship would be to substantially overextend and misrepresent his claims.

Turning back to the universals themselves, of interest to us will be those word order correlations which cleanly relate to typological word order. Specifically, Universals 2-5, 12, 16, 17, 21, and 22 deal with the position of elements in the syntax of the language as a function of the word order typology. Universal 27 pertains to what we discussed in Chapter 4, and will be addressed again only briefly. While more of the universals do relate to word order in a broader sense, the goal of my undertaking here is to whittle them down into those with which we can reasonably engage, using the data at hand. Word order universals such as Universal 28 ("If both the derivation and inflection follow the root, or they both precede the root, the derivation is always between the root and the inflection.") or Universal 18 ("When the descriptive adjective precedes the noun, the demonstrative and the numeral, with overwhelmingly more than chance frequency, do likewise.") do speak to the ordering of items in the syntax or morphosyntax, but to

include them would require a substantially enlarged scope for our methodology and their inclusion would not meaningfully contribute to our aims herein.

Table 5.2.1 Selected Universals from Greenberg's Word Order Universals* (1963)

2	In languages with prepositions, the genitive almost always follows the governing noun, while in languages with postpositions it almost always precedes.
3	Languages with dominant VSO order are always prepositional.
4	With overwhelmingly greater than chance frequency, languages with normal SOV order are postpositional.
5	If a language has dominant SOV order and the genitive follows the governing noun, then the adjective likewise follows the noun.
12	If a language has dominant order VSO in declarative sentences, it always puts interrogative words or phrases first in interrogative word questions; if it has dominant order SOV in declarative sentences, there is never such an invariant rule.
16	In languages with dominant order VSO, an inflected auxiliary always precedes the main verb. In languages with dominant order SOV, an inflected auxiliary always follows the main verb.
17	With overwhelmingly more than chance frequency, languages with dominant order VSO have the adjective after the noun.
21	If some or all adverbs follow the adjective they modify, then the language is one in which the qualifying adjective follows the noun and the verb precedes its nominal object as the dominant order.
22	If in comparisons of superiority the only order, or one of the alternative orders, is standard-marker-adjective, then the language is postpositional. With overwhelmingly more than chance frequency if the only order is adjective-marker- standard, the language is prepositional.
27	If a language is exclusively suffixing, it is postpositional; if it is exclusively prefixing, it is prepositional.

**Name adapted for context and concision*

The universals provided in Table 5.2.1 above should be taken whole cloth for the purpose of our investigation rather than evaluated one by one. The value to our work will not be in demonstrating how a specific universal is maintained through the course of syntactic change, but rather to investigate and outline the mechanisms and pressures which allow such universals to be maintained. Accordingly, when I address a specific correlation, the intent will be not to exhaustively investigate that specific correlation, but to instead build a bank of explanations which are used to support these correlations. As will become obvious, each explanation applied

to one of these universals tends to easily apply to most or all. This is also useful because not all languages will equally display the features described in these universal correlations and searching for that unicorn case study where every component can be demonstrated would ultimately be detrimental to our goals. One final practical reason for this approach is that the goal of this chapter is to explore a top-level evaluation of a broad phenomenon — as seen in Chapter 4, it is possible to do a deep dive into any one of these Greenbergian Universals, but such a deep dive into any given correlation is likely to yield a specialized analysis which is prone to echoing the analyses of each of the other Greenbergian universals of similar subject matter. Our goal in this chapter is the bigger question, "how and why are such universals able to be maintained even though every language is in a constant state of ongoing change?", rather than the smaller question of "how is *this* universal maintained." Chapter 4 demonstrates how that smaller question can be addressed at the level of system tendencies and this chapter does not aim to stay at that same level of analysis. One final note on methodology: while this chapter is not going to strictly repeat the same approach of Chapter 4 per se, I will however be building on the framework advanced in the discussion of the affixation asymmetry in Chapter 4 where synchronic explanations will be accepted as applicable to diachronic maintenance and development. This equivalence between the synchronic and diachronic will allow a broader and more useful analysis than one which attempts to carefully segregate such explanations.

5.2.1 Justifications of Greenbergian Universals

Let us start where we left off in Chapter 4. We have seen that the world's languages are predominantly suffixing rather than prefixing, but that that trend correlates with word order typology as seen in Universal 27 in Table 5.2.1 above. I will start by recasting what we

suggested in Chapter 4 as stating that the phenomenon of the affixation asymmetry could be thought of as a confluence of two effects: development and destruction. "Development" in this case would be the result of pressures from frequency (eg Zipf 1932, Hopper & Traugott 1993, Heine & Kuteva 2002, etc.), the syntactic and cognitive advantages of stems first (ia. Hawkins & Gilligan 1988, Cutler et al 1997, Bybee et al 1990), prosodic factors (Himmelmann 2014), and an enabling word order (assumed by most, but especially Hall 1988 and Venneman as cited in Mithun 2003). "Destruction" as conceived here would be a partial refutation of Givón's (1971, 1979) premise of the functional maintenance of affixes post word order change and instead be a demonstration of the freezing of stems or degradation of the affixes when in non-ideal configurations. Stem freezing (rendering of the morphology non-productive over time) can be attributed to a variety of causes, and as we saw in Chapter 4, will not apply in an unbiased manner to both prefixes and suffixes (for example, the primacy of the onset described in Hupp et al 2009, Marslen-Wilson et al 1989, and Tyler et al 1988 applies to prefixes through freezing and fusion, and suffixes by phonological erosion). Another example of destruction is the relexification of affixes, thus removing them from the consideration of this correlation (cf. Bauer 2003). These patterns of development and destruction will be an ongoing theme in our discussion of the maintenance of universal word order correlations. The main difference from how we treated them in Chapter 4 will be in how we tie them back more directly to the mechanisms of change it in a broad sense.

I will now turn to the other universals within our set, starting with Universal 2: "In languages with prepositions, the genitive almost always follows the governing noun, while in languages with postpositions it almost always precedes." (Greenberg 1963). One further piece of support for this universal, in addition to the general empirical support for it in typological

samples such as Greenberg's original 30-language sample, comes from language learnability studies. Lupyan and Christiansen (2019) conducted a machine-learning study on case and word order which found that genitive ordering in line with Universal 2 was conducive to the acquisition of word order. But this kind of basic evaluation will only get us so far. Indeed, as we expand to other universals, the explanations are prone to getting repetitive. This is not a criticism of Lupyan and Christiansen — their commentary on Universal 2 was incidental to a much larger study and not the thrust of their paper. Rather, their finding is useful because it shows that studies can directly address a single universal, and this contrasts with what most studies do.

Dryer (1992) and Dunn et al (2011) interpreted Universal 2 in different ways — they divorced the original correlated pair and then tied both halves back to the most basic word order typology (that of the order of Verb and Object). Both the adposition strategy and the genitive ordering could be correlated to the verb-object word order, and this in turn reduced the daisy chain effect that can be seen in Greenberg's original formulation: where X can correlate with Y and Y can correlate with Z but it is not directly stated whether X and Z correlate meaningfully. The advantage of this change in approach should be clear: it redefines the universal correlations to a more parsimonious representation that also mitigates co-correlating variables being treated as confounding variables. On top of this, it collapses much of our universal list above. For example, Universals 2, 3, 4, and 5 can be interpreted as a merged set (although, per Dryer 2009, Universal 5 might be best left aside as an unusually weak correlation). Similarly, Universals 12, 16, 17, and 21 all condense down to being correlates of the basic word order typology as well. This means that whatever explains any one of any of these eight universals can reasonably be applied to the other seven. While validating these broadly applied explanations and conclusions

against the independent universals can still be beneficial for research focused on that close level of analysis, this aggregation is important for our ability to look at the issue more broadly.

Applying this in practice, we can say that a broad model of word order harmony can be applied to all the entire set of Greenbergian Universals reproduced in Table 5.2.1, and we can see examples of this in the Head Ordering Principle (Hawkins & Gilligan 1988) discussed previously in Chapter 4, Dryer's Branching Direction Theory (1992), and the Performance-Grammar Correspondence Hypothesis (Hawkins 2004, 2014). There are multiple such theories focused on this harmonic ordering. Greenberg's paper itself (1963) also makes a substantial nod toward harmonic word orders. But we can also see it represented as an assumed feature in many other works. Culbertson et al (2012) and Culbertson et al (2020) and the commentary on Culbertson et al (2012) by Goldberg (2013) all take harmonic ordering as an assumed premise and then probe its impact on learnability. Just like the work by Lupyan and Christiansen (2019), they find that harmonic ordering plays a role in language acquisition. Culbertson et al (2012) and Culbertson et al. (2020) are useful particularly in concert because in the 2012 work they test the effect of harmonic ordering in second language acquisition on students of a harmonic (in the context of the targeted features) first language (in this case: English) and in 2020 they demonstrate it for students with non-harmonic first languages (specifically: French and Hebrew), demonstrating the same results both times: a learning advantage for languages with harmonic word order. This means that between these studies and Lupyan and Christiansen (2019), we can demonstrate a thorough probing of harmonic word orders as an advantage in learning both at the practical and theoretical level. Additionally, while processing and learning present different and distinct pressures, it is worth mentioning that they are not wholly unconnected. This should be intuitive on a certain level: what is easy to process would plausibly be easier to identify, and easier to

identify may sometimes correlate with ease of learning. This linkage is well demonstrated in Trecca et al (2021) where a restricted phonetic inventory in Danish was demonstrated to impede language acquisition, in part due to ambiguity in detecting word and morpheme boundaries.

Similarly, when we consider Hawkins' work on syntactic efficiency (2014), the pressures he discusses as part of his Performance-Grammar Correspondence Hypothesis all coordinate to improve the identification and integration of information — this was approached from a processing efficiency perspective, but the advantages in processing likely would yield some advantages in acquisition. The Performance-Grammar Correspondence Hypothesis and its component principles of Minimize Domains (briefly: keep items close to that which they modify), Minimize Forms (reduce formal complexity and consolidate meanings), and Maximize Online Processing (streamline meaning assignment) each serve to improve the clean identification and association of meaning, but those principles in actual practice would reasonably be expected to yield some advantages in language acquisition. Keeping this in mind and turning back to Lupyan and Christiansen (2019), we can see that the advantage in learning has the characteristics of a developmental pressure — an advantage in learning both will be an advantage in correctly learning a harmonic word order language and it will be a pressure for change within a non-harmonic word order language, since errors in acquisition can be assumed to err in the consistent direction of demonstrating harmony.

Applying this to our concept of virtues in learnability overlapping with virtues in processing, the improvements in learnability of harmonic word orders can be reasoned to match with the improvements in processing supplied by harmonic word orders — harmonic word orders provide for easier and faster identification of each component's role in the sentence. Simple advantage models without the acquisition pressure, like the Head Ordering Principle

discussed in Hawkins and Gilligan (1988), similarly might provide a bulwark against destruction for harmonic word orders while offering no protection for non-harmonic systems. This accordingly is our starting point: we can see that these selected Greenbergian Universals can be largely boiled down to a characterization of word order harmony and that they have been commonly analyzed in terms of a synchronic advantage once in place — the Head Ordering Principle (ibid.) and, with greater abstraction, the Performance-Grammar Correspondence Hypothesis (Hawkins 2014) — and as a learning advantage for speakers. This picture will become more complicated however as we add more explicitly diachronic data and analysis and evaluate changes in word order.

5.3 Word Order Change

With languages either changing due to contact or due to more "internal" causes, these changes of word order would necessarily place these universals in jeopardy if nothing else were to change, as previous correlations would be frozen as a historical artifact despite no longer being appropriate. This is conceptually related to the "historical artifacts" utilized by those such as Givón (cf. Givon 1971) to propose historical language states (see for example: Givon's demonstration of historical syntactic properties of the Bantu verb phrase based on the modern prefixes and suffixes of Bantu descendent from verbs), but that methodology is based on the fact that these are reliable correlations, thus making historical artifacts stand out as unusual and often deprecated (cf. Hawkins 1983, p262). It is therefore important to an understanding of change, especially as a complete system, to understand how these correlational universals are maintained. Some of these can be maintained as part of the mechanism for word order change (cf. Hawkins 1983, p252 where situational pragmatic fronting of the subject can lead to a paradigmatic shift in

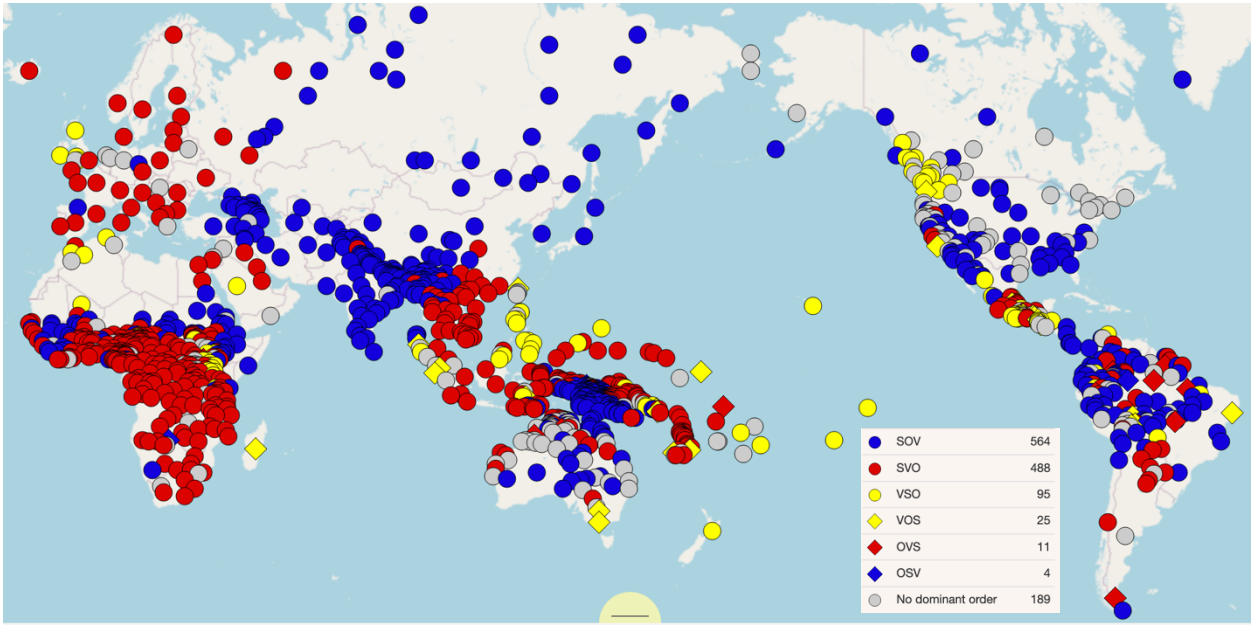
word order), but not all — and indeed, if we turn again to Hawkins 1983, we can see that for many changes, we have been relegated to purely descriptive statements of the phenomena, without having a mechanism for precisely *how* that comes to be (cf. p254). As seen in the maintenance of the affixing asymmetry in Chapter 4, the process involved may require the integration and joining of multiple forces and explanations, but the existence of these correlations as tendencies means that we should expect there to be a mechanism behind their continued maintenance through ongoing changes in word order. With this in mind, we should first turn to phenomena of word order change.

5.3.1 Word Order as a Genetic Phenomenon and Areal Phenomenon

The first reasonable assumption for word order is that it would be a genetically linked phenomenon, that the word order typology would be consistent across languages with a common ancestor language. This makes intuitive sense. We can expect that descendent daughters of a parent language will be similar to the parent unless a change occurred. Indeed, when we look at Figure 5.3.1.1 taken from WALS (Dryer & Haspelmath 2014, Map 81A), we can see the dots for the Romance languages in Europe are all SVO, and when we look to southern India, we can see that the dots for the Dravidian languages are all SOV. Word order and syntax in general has generally not been a good indicator of a genetic relation, however, since the clusterings of red dots and blue dots extend well beyond these individual families and subsume genetically unrelated languages that happen to be in areal contact. This has been recognized in the literature — Campbell's (2003) survey of methodologies for establishing genetic relations does not even mention the use of the syntax, and even those scholars who hope to demonstrate syntax as a genetic indicator are forced to concede that it is not well accepted and that present research does

not support it (cf Ceolin et al 2020). The issue here is that while word order typology may have a greater than chance correlation within a family, as a feature it is prone to change. We can even see this clearly in the Indo-European languages. In Figure 5.3.1.1, we see yellow VSO languages in the British Isles — these are part of the Celtic branch. On the European mainland, we can see languages with no dominant word order in the north — these are West Germanic languages. In Scandinavia, the North Germanic languages are SVO. Moving east to South Asia, we see the Indo-Iranian languages are SOV. All of these, and the Romance languages previously

Figure 5.3.1.1: Word Order Typology (Dryer & Haspelmath 2014, Map 81A)



mentioned are part of Indo-European. And yet we do not see a regular pattern. Even in the Germanic family, the North and Western branches do not track together, and this even deliberately ignores English which is not part of that continental cluster. So, while we can speak generally of a family having a certain word order typology, we cannot rely on that typology being consistent. And when we look at the map more carefully, we notice instead that the most apparent patterning is by region, not by family. The Indo-Iranian languages have the same word order typology as their Dravidian neighbors rather than staying similar to related European

languages. This non-genetic connection is important for the premise that word order correlations are more than lineage-specific trends, and this has been substantiated by theoretical modeling with Markov chains by Jaeger and Wahle (2021) who show, contrary to the claims of Dunn et al. (2011) who used a more limited sample and argued for lineage-specific trends, that the correlations of Greenberg and Dryer are indeed universal and not just lineage-specific.

Language word order typology as an areal effect is a well-known phenomenon (cf. Heine & Kuteva 2005, Dryer 2013, Graham 2016) but it is helpful to pull back to the very basics because word orders in a family may be inconsistent with other languages of the genetic family and lineage, we can say with certainty that they may change over time, and that change is often predicted by what the other languages are nearby. This suggests that languages are influenced by language contact or bilingualism (cf. Heine & Kuteva 2006). Turning back to our map (Figure 5.3.1.1) we see this manifested in the frequent and intense clustering of the same word order typology (Graham 2016). This essentially defines word order as a sociological phenomenon or as at least as a phenonenon affected strongly by sociological forces. For our purposes this means that this phenomenon likely has a substantial non-teleological component despite having a large number of correlated phenomena which might be teleologically motivated. That is to say, there is no inherent good to a language's word order typology which is determining that word order's selection, even if the correlated features might have intrinsic advantages when co-occurring with their correlated word order. I do not mean to say that different word orders might not be more dominant, and there is good evidence that some may be more stable or transmissible than others, as argued by Nichols (2003, p305) where she indicates that while SVO and SOV word order typologies are well represented around the world, V-initial word order typologies are much rarer and seem to require especially fortuitous circumstances to be observed and maintained (such as

reinforcement by neighboring languages) unlike the more dominant word order typologies. For our purposes in this chapter however, these relative advantages which word orders might exhibit over each other will not be of particular interest; we are instead focused on the fact that this feature does change and that the correlated features manage to maintain their correlation.

5.3.2 Types of Word Order Change

We have established that the word order typology of a language may in principle have a genetic component or it may have an areal component. A further possibility for word order change is that it arises through language-internal change. This is not something that we have directly addressed so far. A genetic correlation with word order typology is merely a statement that word orders are likely to stay similar within a family. But in the absence of demonstrated language contact, the only other option is that word order is capable of changing on its own. The boldest possible interpretation of this claim comes from Givón (1979), where he posits that all world languages descend from a historical SOV word order. In such absolute terms, there is no other word order that languages can borrow from, at least initially, so change towards the current diversity comprising SOV, SVO and VSO languages, etc, would *have to be internal,* at least initially. This claim however is stronger than that with which I think we need to concern ourselves. In more conventional scenarios, this kind of explanation for the history of basic word orders is harder to prove, simply due to the overwhelming opportunity for contact, but it is attested within the literature. Harris (1978) proposes an oscillation in the history of French between SVO and SOV with Classical Latin (SOV) → Vulgar Latin (SVO) → Old French (SVO) → Modern Standard French (SVO) → a possible future shift back to SOV. This analysis is made on purely language internal grounds — he hypothesizes that there may be an obscuration

of the semantic motivations of syntactic structures leading to change leading to syntactic drift (in line with the variationist class of analysis discussed in Chapter 2). Another possibility would be through correlated features leading the language to a typological shift (Harris 1981, p186) with non-correlated features developing randomly, and eventually pulling a realignment of the syntax to a form more in line with the Greenbergian correlational universals. And indeed, we should assume that all languages are in a state of change and transition at all times, both at the macro level where dialects are treated as one, and at the micro level where each dialect is treated individually — no language is going to be perfectly consistent across time or even in a given moment (ibid). This means that even where contact is not attested, it's not enough to assume that change simply *can* happen, but that it *is* happening, and the question is only the pace and how.

The form of word order change depends often on the morphosyntactic features of the language. Let us consider one facet of Harris's discussion of French. What does it mean for word order to change between Classical Latin to Vulgar Latin? Classical Latin was richly inflected and could demonstrate many word orders, so Latin, when categorized typologically, has always had a best-fit assigned word order upon which typological claims would then be based (cf. Ullman 1919, Adams 1976, Coleman 2009), with SOV being identified as the least marked form. This means that in the context of Latin and its transition to its descendent Romance languages, a realignment of word order will be manifested by pragmatic choices in word order becoming conventionalized. We can find support for this both in the Ohala-esque pool of variation, and also in the fact that the initial word order conventions were rooted in pragmatic utility to begin with since the words were sufficiently inflected to allow free word order assignment (Coleman 2009, p159). Thus one possible mechanism of internal word order change

is simply pragmatic drift, especially when inflection is rich enough to not necessitate rigid word order.

This same mechanism of pragmatic realignment can be found in less richly inflected languages, but it occurs by different means. In richly inflected languages, we see a drift towards one construction being favored among all valid constructions. As mentioned before, a good example would be Latin's drift towards SVO — Latin was primarily SOV, but was richly inflected enough to allow free word order without changing syntactic relationships, and over time SVO began to become favored within the pool of options, even though it was not originally the most common order (ibid). In less richly inflected languages with stricter word order on the other hand, we might expect to see a syntactic construction with a particular pragmatic function becoming overused to the point of desemanticization. One example of this on a minor scale would be the outgrowth of do-support in Modern English where a pragmatic construction resulted in an overt auxiliary becoming common, and in many cases mandatory, in English (cf. Varga 2005, Rohrbacher 1999). This has been investigated in terms of its effect on V-to-I movement within the family of generative syntactic theories (ibid), but one need not hold those theories to be correct or false to demonstrate the effect on the syntax and the vehicle of change for this development.

Contact-induced change is where multi-lingual speakers or trade connections result in the exposure to new constructions or second language artifacts and errors. This type of word order change is perhaps the easiest to demonstrate and document, but it carries with it some challenges. The first problem is one that we will need to probe in greater depth in the next section: when does language contact motivate change versus when is language contact creating a new entity (like a pidgin or creole). There is no straightforward answer to this problem and it is one which

we will need to engage with delicately primarily because it is a problem of our own creation — by categorizing word orders in terms of "change", we are implicitly engaging with the problem of what qualifies as genetic descent. I will leave the genetic question for later, but simple contact-induced change is extraordinarily common (cf. Hawkins & Filipović 2021). This can be subsumed under their Maximize Common Ground principle where the presence of another language can tip the scales for variation within a language towards a form or structure common to both languages (ibid). This is the case when French is in contact with English — the position of adjectives with regard to the noun changes from mixed Noun-Adjective/Adjective-Noun (but favoring N-Adj) to predominantly Adj-N as is the overwhelming norm in English (Nicoladis 2006). Similarly, when Spanish and English are in contact, the obligatory subject of English interacts with Spanish's flexible overt/implied subject and yields a drift in Spanish towards ever greater conventionalization of overt subjects (cf. Myers-Scotton 2003). Other examples might be better explained under the concept of pragmatic borrowing, as discussed in Chapter 3 with European formal pronouns.

The best example of the genetic problem is when we have more aggressive forms of language contact. When we evaluate English, for example, we have a famously complex lexicon with words from Old French, Latin, Greek, Old English, and Old Norse all in abundance, and yet those last two, the primary Germanic sources for English, are not the dominant sources today for what is universally accepted to be a Germanic language. And the lineage of the syntax is not perfectly clear either, as even the ancestor of Modern English garners some controversy with the occasional contrarian asserting that Middle English is descended from Old Norse rather than Old English (cf. Emonds & Faarlund 2014). Linguists choose to evaluate English as a West Germanic language, but a similar patterning with a worse (or non-existent) written record might

not yield as decisive a conclusion. Accordingly, when we evaluate language change, when disparate superstrates and substrates are overlayed, the appraisal of word order change will be inherently contentious, because it challenges our very definitions and assumptions of when a language is still part of an unbroken genetic line. We will see this issue come up later in Sri Lankan Portuguese.

Subtler forms of language contact are also possible. We saw in Chapter 3 that pragmatic borrowing is possible in language contact scenarios with languages mirroring the construction of another language but with their own tools. In Chapter 3, it was a purely morpholexical pragmatic borrowing, but Mithun (2007) documents pragmatic borrowing in the California language region which treads in the morphosyntactic domain. Mithun shows that within California certain typologically rare features, such as means/manner prefixes and locative/directional prefixes, are surprisingly common across all indigenous language families. These have given rise to many mistaken genetic hypotheses for the languages of the region, but what appears to have been the case was a phenomenon of language contact, where bilingual speakers would recreate the structures of one language with the tools inherent to the other language. Over time, these forms and constructions regularized and grammaticalized, resulting in multiple genetic lineages within California all demonstrating the same grammaticalization pathways for the same typologically rare morphology (ibid). Once the borrowed construction interacts with the syntax, we can see the opportunity for influences on the word order of the language. The principle distinction at our present level of abstraction between this and the full substrate influence described previously is that the contact influence here is limited to specific areas of the affected language. We might also assume that this model of pragmatic borrowing

suggests less social disruption compared to the full substrate contact, but that will be outside the scope of our present concerns.

5.4 Word Order, from the Source to the Result

So far we have looked at Greenbergian correlative universals and the nature of word order change, especially with a focus on the areal and genetic components of the phenomenon. Greenberg's universals, especially with their modern reformulations, fundamentally imply a teleological virtue — that there is an advantage gained within a language by maintaining those universals. And accordingly, every time a (statistical or non-exceptionless) universal would be violated, we might infer a disadvantage for the language which would be felt as a pressure on the language toward change. This advantage and disadvantage duality would suggest either a functional pressure towards an advantaged end form or a functional pressure away from a disadvantaged source form. This push versus pull is an important distinction, and the two are not actually the same. We have seen in Chapter 2, that there is a strong body of linguistic theorists who would argue at most for the push, and some within that group might reject the dichotomy altogether. Source-based theorists argue that change flows from the starting point and that there is no teleological virtue, no inherent good, to be found in a language's subsequent form (Cristofaro 2019). Word order change and its interaction with Greenbergian universals is therefore an important area of consideration. Greenbergian universals represent broad patterns in world languages, and according to the uniformitarian theory, unless some astounding historical research demonstrates otherwise, we can assume that the broad patterns of the languages we can document today are true patterns for languages throughout time. This means that Greenberg's universals must be maintained, and this maintenance of universals is an opportunity to see

different forces at play with perhaps insight into the question of teleological forces in functionally motivated language change. Evaluating the maintenance of universals for teleological pressures is important for our overall understanding and modeling of language change. So far, in both our evaluation of the second-person pronoun forms in Chapter 3 and the affixation asymmetry in Chapter 4, we could explain the phenomena in the simple terms of our first two categories of explanation: social and individual level pressures. In Chapter 3, the development of the T-V distinction in European languages and of its erosion and recreation in Spanish can be clearly linked to social pressures on the language system, while in Chapter 4 the presence and maintenance of the affixation asymmetry was able to be cleanly linked to functional pressures operating at the level of the individual speakers, such as processing efficiency. We have not required a structurally motivated explanation so far, and with that being the case, the burden is on that line of explanation to demonstrate utility so as to avoid being subsumed by the other types of explanation. To this end, I will cover two major and well-documented examples of word order change and the corresponding maintenance of Greenberg's universals, with a particular eye to what forms of explanation merit application to each.

5.4.1 Sri Lankan Portuguese

The first example we will look at is that of Sri Lankan Portuguese. This is typically presented as a dialect of Portuguese spoken in Sri Lanka with substantial influence from local languages, especially Tamil (Bakker 2000, Heine & Kuteva 2005). Sri Lankan Portuguese sits on the cusp of being a historically creolized Portuguese dialect with forms reduced centuries ago upon the arrival of the Portuguese in Sri Lanka (Bakker 2000) and instead being a creole with Portuguese superstrate and very recent Tamil substrate (Heine & Kuteva

2005, p214) but without the reduced grammatical complexity expected of creoles (Bakker 2000). This dialect of Portuguese has seen a word order re-alignment from SVO to SOV to match Tamil, and this realignment likely occurred in the space of less than 60 years (Heine & Kuteva, p241) thus increasing the comparisons to a creole despite the lack of reduced complexity, not to mention the SOV word order which has been relatively unusual for documented creoles (cf. Bakker & Daval-Markussen 2017, McWhorter 2005). Sri Lankan Portuguese, prior to the influence of Tamil, was a classic SVO language, in line with most Romance languages back in Europe. The change to SOV carried a number of consequences.

The first is that according to our discussion in Chapter 4, and the reformulation of Universal 27 to link to word order used in that chapter (generally: OV languages are expected to be strongly suffixing and that prefixing is primarily expected to be seen in VO languages), we would expect the newly Tamil-influence Sri Lankan Portuguese to be strongly suffixing. The rapidity and recentness of the change in word order would suggest that simple drift on this axis would be unable to account for any such realignment. But instead what we see is a rapid redevelopment of the toolset used in Tamil's grammar from Portuguese components, and that development is exclusively suffixing, in line with the universals (as seen in Table 5.4.1.1). The explanation of this development is not a simple case of grammaticalization in situ. Instead there is a two-part process of movement and grammaticalization — the new Sri Lankan Portuguese suffixes are not simply frozen in place where they were before the word

Table 5.4.1.1 Postposed and Suffixed Tense Markers in Sri Lankan Portuguese

	Portuguese Source-word	Sri Lankan Portuguese	Sri Lankan Tamil
NOM	ø	ø	ø
ACC	*pera / para* 'for'	ø /-pə	ø / -a(y)
DAT	*para* 'for'	-pə	-(u)kku
GEN	*sua /seu* 'his/her/its'	-su(wə)	-Ra
LOC	*junto* 'together'	-untu	(i)la(y) / -(i)TTa(y)
ASSOC	*junto* 'together'	ju:ntu	-o:Ta(y)
INSTR	*banda* 'side'	wəndə	-a:la(y)

(Bakker 2000)

order change, the source words moved to the position and then grammaticalized after. This is likely best treated as a radical example of pragmatic borrowing (borrowing a model or framework for meaning formation rather than the lexical items themselves) in the wake of contact-induced word order change, with the word order change creating the pragmatic need and bilingual speakers reaching for similar constructs as what they were accustomed to in Tamil. Indeed, if we refer to examples 1, 2, and 3, we can see this demonstrated clearly. In (1), we can see a standard Portuguese rendition of "I gave him the money", and in (3) we can see the same for a dialect of Tamil which is spoken in Sri Lanka. By looking at (2), we can see that -pa has moved to be after ele and attached as a suffix marking the dative case, just like in Tamil, and consistent with what we would expect for an SOV language. Even with the word order, this case marker, and a new passive particle, the sentence is still recognizably close to Portuguese.

(1) **Portuguese** (Smith 1979 in de Silva Jayasuriya 1999, p258)

eu	dei	ele	o	dinero
I	gave	him	the	money

(2) **Sri Lankan Portuguese** (ibid.)

e:w	eli-pa	diñe:ru	ja:	dá:
I	him-DAT	money	PAST	give

(3) **Sri Lankan Tamil** (ibid.)

na:n	avan-ukku	calli-ya	kúTu-tt-an
I	him-DAT	money-ACC	give-PAST-CNC

Mithun (2007), as previously mentioned, has documented similarly consequential, although less dramatic, examples of pragmatic borrowing before, and in this case we can see that one possible mechanism for the maintenance of universals is the borrowing of the correlated structures at the same time that the language was adapting to the neighboring language's word order.

5.4.2 Ancient Greek

Our next example comes from a groundbreaking study of the history of Greek by Seržant and Rafiyenko (2021). This study specifically set out to test whether word order changes necessarily flowed from the source as claimed by source-based theoreticians. Accordingly, this will be the most direct confrontation possible for testing the source-based theory where the change is drift determined by the starting point versus individual level analysis where pressures at the level of the speaker may be pushing in a particular direction. The final possibility of course is that another model of analysis may better apply here, such as changes motivated by the structure of the language itself.

Seržant and Rayiyenko (ibid) conducted their research on a corpus of Greek spanning from ~750 BC to ~700 AD. This breaks down into the rough timespans shown in Table 5.4.2.1, with New Testament Koiné getting its own category due to being unusually representative of vernacular spoken Greek at the time without as much of the literary conservativism. While this is an extraordinary timespan, the actual bulk of their data falls in the 700 year range between ~450 BC and ~250 AD and the

Table 5.4.2.1: Periods of Ancient Greek

Period	Date Range
Archaic	750-450 BC
Classical	450-315 BC
Hellenistic	340-0 BC
Roman	50-250 AD
New Testament Koiné	100 AD
Early Byzantine	500-700 AD

(Seržant and Rafienko 2021)

specific focus is on the changes experienced between Classical and Postclassical Greek. From Archaic (Pre-Classical) Greek through to Post-Classical Greek, Seržant and Rafiyenko demonstrate a constant movement toward greater syntactic dependencies, and a loss of the allomorphy that was typical of Archaic Greek. Through the course of these changes, we can see that the conventionalization of a structure allows new pressures to be exerted on the system. In this case, the relatively free positioning of clitics and adverbials was lost and they were gradually locked into ever more restrictive placement. If we look at Greek in the Archaic period, we can see that the adverbials do not seem to have any obligatory position within the syntax of the sentence.

(4) **Archaic Greek** (Hom. *Il*. 7.509, Smyth, 1920: 366, as cited in ibid)

amphì	*dè*	*khaîtai*	*ṓmois*	*āïssontai*
around	PRT	hair.NOM.PL	shoulder.DAT.PL	float.PRS.3PL

'and his mane floats about his shoulders'

(5) **Archaic Greek** (Hom. *Od*. 24.20, Hewson and Bubenik, 2006: 6, as cited in ibid)

elyth'	*épi*	*psykʰḗ*	*Agamémnonos*
go.AOR.3SG	near	soul.NOM.SG	Agamemnon.GEN

'the soul of Agamemnon approached'

If we look at *amphì* in (4), we can see that it defies any implied logical association by its placement. It is unbound to the verb or to what we might identify as the expected position adjacent to a noun phrase if it were an adposition. Meaning is frustratingly opaque if we force interpretation through a lens of analytic syntax where word order defines the construction of meaning. This is reinforced by (5) wherein *épi*, an adverbial of similar qualities at this time,

occupies an entirely different placement in the sentence, this time adjacent to the verb. Seržant and Rafienko contextualize this freedom with the caveat that while there is no syntactic dependency or constituency in the form of prepositional phrases for these adverbials, there is in fact a semantic dependency. This relatively free word order was to calcify over time, and in a different balance from the predecessor state — Archaic Greek showed a verb-final tendency within a relatively free word order as seen in (4) while Classical and Roman Era Greek showed a strong bias towards a verb-medial order (SVO). Table 5.4.2.2 shows this trend clearly.

Table 5.4.2.2: Word Order Tendencies in Clauses with Full NPs

	Archaic (Homer)	**Classical (Heroditus)**	**New Testament (Luke)**
Verb Final (OV)	44%	27%	8%
Verb Medial (OV/VO)	44%	57%	62%
Verb Initial (VO)	12%	17%	31%

(Taylor 1994, as presented in Seržant and Rafienko 2021, Table 12)

The relatively free word order also calcified, and one side effect of this was a shift towards these adverbials being adjacent to their modified items, and in the process forming constituents. In (6) we can see the adjacency principle in full with a much more developed syntactic structure compared to that of the Archaic period.

(6) **Hellenistic Greek** (as cited in ibid)

tà	*perì*	*tò*	*Lilýbaion*	*stratópeda*
DEF.NOM.PL	PERI	DEF.ACC.SG	Lilybaeum	army.NOM.PL

'the army at Lilybaeum'

Perì is unglossed in this example because *perì* was in an ongoing state of semantic flux with a complex bevy of meanings, and the utility of this example lies not just in the development of the syntactic structure but also in the continued change of the semantics of *perì* itself. We can start with the original meaning of *perì* in Archaic Greek ('around'/'about') and work forward with that as a fuzzy assignment of meaning which works well enough for this example and will allow us to avoid getting too deep into the specifics of the many other connotations that it can still carry in this era. Rather, our interest lies in two aspects of the example: first, the syntactic structure; and second, the case assignment of the lexical item associated with *perì*. The linear ordering of the information is now significantly more conventionalized compared to the Archaic Greek, and we can see that each word is now likely to be adjacent to its phrasal dependents (again, comparing back to (4)). *Perì* is positioned next to the location, and in this instance is behaving very much like we would expect it to in a prepositional phrase as all the necessary components are grouped in the unbroken sequence of *perì tò Lilýbaion*. We are not yet at the point that constituents as contiguous entities can be assumed (see for example *tà* and *stratópeda* at the furthest positions away from each other), but the language is in the process of losing that freedom of ordering. The second point of interest is the accusative case associated with *perì*, as seen with the accusative assignment of *Lilýbaion* shown on *tò*. This accusative assignment was actually in an emerging characteristic of *perì* at this stage of Greek's development, with the previously dominant genitive case assignment in decline. Genitive was the overwhelmingly most common case assignment with *perì* in the Classic Greek texts (71%), trending down to 45% by the Roman period, with the intervening Hellenistic period at 59%. The accusative assignment seen in (6) only accounted for 41% of the uses of *perì* in the Hellenistic era, but would become the majority by the Roman era. The decline in the genitive case with *perì* is indicative of two

simultaneous changes: a deprecation of the compositionality of the meaning and a trend towards conventionalized case assignment according to the specific meaning of *perì* rather than the old pattern where case assignment was correlated with the intended directionality. The end point of this change would be the eventual total reduction to a grammatical item, where *perì* and other adverbials undergoing this change would become prepositions with trailing NP dependents, very much like what we see forming in (6).

These sorts of changes had repercussions for the adpositions of Ancient Greek. Archaic Greek had been flexibly pre- and post-positional, demonstrating the lingering flexibility of recently grammaticalized adverbials. By the end of the inspected time window, however, Greek would be prepositional and, more importantly, harmonically aligned across three structures (VO word order, prepositional, and noun-genitive order). Notably, these harmonic outcomes co-evolved with the change in word order but appear unmotivated by contact with languages such as Latin and uninfluenced by felicitous source structures which would be inherently biased to naturally grammaticalize along this pathway. Latin, for example remained mixed typology (VO/OV) with free word order and a tendency towards SOV until the development of the daughter Romance languages (cf. Coleman 2009, p159; Green 2009). Thus, despite Roman rule in Greece during part of these changes, the Latinate parallel structures do not chronologically align with when these changes were taking place in Greek since the Romance movement towards SVO occurred much later. The starting circumstances in Archaic Greek would appear to not have a predisposition at all towards the development of prepositional particles and morphemes, as opposed to the postpositional ones characteristic of head-finality and the SOV structures of Archaic Greek and other ancient Indo-European languages such as Sanskrit, Hittite and Tocharian. The source specifically appears at odds with this development. What we see,

however, is that the development of constituency and adjacency is paralleled by an ever-increasing closeness of the adverbials to their dependents. Over time, they transitioned from stand-alone adverbials to clitics, but importantly: clitics which were still undefined as certainly enclitic or proclitic. These clitics migrated to fit the typology of the language as it changed, rather than getting locked in early, but once in place were indistinguishable from grammaticalized morphemes which had taken more conventional pathways. As these clitics moved, most narrowed to fit only a single semantic role, but this semantic limitation was not universal — the example of *perì* in (6) is just one of its semantic readings and *perì* in particular was notable for maintaining a number of different semantic readings.

The motivated changes therefore within Ancient Greek morphosyntax extend both to grammaticalization where we see the adverbials transition to clitics and prepositions, and to item position itself. Crucially, they were not predicated on, or predicted by, the starting configuration of the language — the language's word order typology can best be thought of as a moving target, and the state of the word-order-correlated features (such as prepositional vs postpositional) keeps pace with the word order typology. Seržant and Rafienko suggest that the maintenance of this correlation may be through the continued pressures of Hawkinsian processing efficiency (Hawkins 1994, 2004, 2014), with the syntactic structure of the language responding actively to those pressures at every stage, incentivizing change and adjustment even when an an already existing option was extant and available. Thus, despite being mixed typology in the sense that Archaic Greek was capable of free word order, there was a dominant OV order. The language did not carry through with the "easiest" drift pattern where OV was maintained as word order calcified. Instead Greek developed toward a head-initial configuration and all the universal correlations developed in parallel. The correlated features (such as prepositions) were not

common in the starting configuration but were motivated by the structural changes of the language and became systematically consistent.

5.5 Discussion

Greenbergian universals can be maintained through a number of different mechanisms, and not all of these mechanisms fall under a single category of research approach. This was to be expected from the outset. Such a discovery would have challenged the core premise of this work: namely that no single theoretical position has already solved and explained everything. What is interesting however was that in this one area of study, all of our categories of explanation proved essential.

With Sri Lankan Portuguese, we saw that the social factors of a bilingual population play out as contact-induced change, but this was not problematic in analyzing this kind of syntactic change. The universals were maintained as part of the natural follow-through of contact and pragmatic borrowing. This gives us our first answer for the maintenance of universals: that when a language changes due to an areal pressure, the language cannot be treated as changing in a vacuum. The same areal pressure which motivated the change in word order is likely to supply harmonic solutions for the new syntactic structure. Also worth noting here is that our analysis was unhindered by the questionable genetic status of the language. Even with an ambiguous creole status, it was possible to demonstrate the mechanism of change.

Ancient Greek's development of prepositions demonstrated multiple structural pressures which we had posited in the discussion of such explanations in Chapter 2. We could see that when a form became typologically disadvantageous due to shifts in the dominant word order (Ancient Greek's calcification of a VO word order after an OV-dominant mixed typology

predecessor state), it did not survive against newly advantageous competitors (prepositions) in the new word order despite the advantage of being already extant before the development of the prepositions. This is a clear illustration of functionally motivated change — where the advantages proposed by typological theoreticians (Greenberg 1963; Hawkins 1979, 1994, 2004, 2014; Dryer 1992, 2019) for harmonic word orders were shown to control which forms will survive ongoing language evolution. At the same time, the presence of potentially harmonic forms (prepositions) in competition with the non-harmonic alternatives allowed for the predictable exertion of that pressure on the language with the victor being the harmonious form.

I think it necessary here to focus on one final aspect of word order change. In my initial planning of the research for this chapter, I assumed that this area of language change might be uniquely inclined towards categorical change — situations where incremental change would prove impossible. If a universal suggests that an item in the grammar should be post-posed and it is currently pre-posed, there is no gradual way for it to move to the other side. It is either in front or behind: there are no other options of gentle gradation. If this hypothesis had been born out, then word order change would have been a singular affront to variationist modeling of change and would have posed a large problem to the unification of the field. I do not mean to say that all word order change would have followed this kind of categorical movement, but rather that the tendency would be there to support such movement. Instead, the vast majority of examples of word order change I surveyed were explainable through other means rather than a sudden jump in the syntax that had to occur without precedent. Even the Sri Lankan Portuguese example is not much of a counter-example to this, because it sidesteps those language-internal difficulties and uses another language as template in bilingualism-induced change. The work on Ancient Greek by Seržant and Rafienko (2021) is revolutionary precisely because it is a clear

and unambiguous case study which directly, successfully, and clearly tested and affirmed a non-contact explanation of word order change which also would fit poorly within a strict variationist model. Such examples are rare, and I suspect that they are rare because they are the path of greatest resistance. If a language is "inconsistent", as Harris would say, then it will be likely to undergo changes to alleviate that inconsistency (Harris 1976), but those changes are agnostic to the designs we might place on the language. This would be a statistical preference — that disprefered or inconsistent features *can* develop, but that they will be rare and language states with such features especially likely to be transient or unstable (this also serves as a maintenance mechanism for the Uniformitarian Principle in that rare features are rare regardless of the era). Speculatively, we might expect that the pressures on typologically inconsistent languages, in this case, those that do not maintain universal correlates, might be especially likely to experience changes of any kind which would alleviate the structural pressures created by those inconsistencies. Thus, if a neighboring language has a solution, that solution might be more likely to be stolen. If the language itself is already close to consistency, then the simple path of grammaticalization may be the gradual solution to a weak but insistent functional pressure. And only if those "easier" pathways are insufficient will the teleological advantages to the system be witnessable as direct pressures, as seems to be the case with Ancient Greek.

CHAPTER 6

A UNIFIED APPROACH TO LANGUAGE CHANGE

My goal upon setting out on this work was to establish a unified theoretical approach to language change. The key innovation here is to fully integrate linguistic typology and synchronic studies into the study and analysis of language change at the level of theory and research philosophy. The Uniformitarianism Principle is a broadly accepted pillar of linguistics, but it has not been leveraged to the full extent of its implied power. It is not enough to say that languages have changed in the same way over the course of human history. It is not enough to say that languages today are not fundamentally different from languages of the past. Instead, these twin claims of the Uniformitarianism principle need to be utilized in tandem. The two assertions can and should be used together and at the same time. In doing so, synchronic evidence and theories become equivalent and interchangeable with diachronic evidence and theories for the purpose of the typological analysis of language change.

6.1 Three Types of Answers: The Case Studies in Context

In Chapter 2, we laid out a tripartite typology of research as it applies to language change. Each of these categories was agnostic to the theoretical background of the work conducted — a phonologist and a syntactician could each do research which could be categorized under any of the categories. The same can be said for a generativist or a functionalist, to use the deep split in the field of the last half century. These traditional splits in the field of linguistics do not matter. Similarly, researchers who find themselves arguing vociferously with each other over core theoretical claims (perhaps even the generativists and functionalists previously mentioned) could

discover that their work lives together under a single category. Our typology of language change research, rather than following these presumed splits, instead asked what type of answer the research yielded or pursued. And broadly speaking, all research in the field could be successfully categorized into the three categories outlined in Chapter 2. I labeled these three categories of research according to the level on which the pressures (whether implied or proposed) acted: the individual, society, or the structure of language itself. One of our principle research questions was whether all three of these types of analysis were important for the comprehensive analysis of language change. An implied subordinated question was whether and how these different forms of explanation could be integrated. Answering these two questions would also directly address the claims of the growing body of research advocating source-based theory and lineage-specific generalizations. With this in mind, we should turn to the three case studies covered in this work and consider how they apply to these questions and the overarching goal of this research.

The three case studies were carefully chosen. Each case study applied at a different level of analysis and across the three there was the intent that all forms of analysis would be addressed and have at least one opportunity to demonstrate value and utility. This was a deliberate thumb on the scales. It is easy to claim that a certain type of analysis is ill-suited to a specific issue. But our question is whether each category of analysis has a contribution to make at all, again within the multi-disciplinary harmonizing approach of this work. So being important or essential just once is enough to justify that line of inquiry as a valid inclusion in our modeling of language change.

We started with the case study on the Spanish *usted* in the context of European honorifics. Here we saw a clear pressure of societal pragmatics pushing forward a change in

Spanish. This case study is important in a number of different ways. First, this is a demonstration of an "external" pressure — a causal force on language which is not part of the narrow band of simple language usage. One does not *require* a formal mode of address to merely communicate — this is a need imposed by the social realities of the community. This is our first exploration of non-linguistic input into the nature of language change and is a direct affirmation of the utility and value of the category of societal explanations in language change. Second, in the course of the T-V distinction in European languages, and especially in Spanish, we can see a similar linguistic change in response to that pressure at multiple points in history. This allows us to demonstrate a causal relationship between pressure and response. In doing so this represents the ability of language change to be predictable. This causal relationship is quite informative in other ways as well, because in addition to presenting a narrative for how this broad cross-linguistic effect is seen within the European cultural sphere, it also provides an explanation for the exceptions. We can see how when the societal context serving as a pressure on formality shifts, so too does the effect on language, and this provides the solution for why this cycle no longer appears to be rolling on in European languages. English for example has been noted for being unusual among Germanic languages for its missing second person plural pronoun (cf. McWhorter 2002) but we can easily see that *you* (2PL) replaced *thou* (2SG) just as this occurred in Spanish with *vos* drifting ever more familiar. The lack of development of a new formal second person address could be for a number of factors but shifting societal norms and demands would be in the conversation. This raises an interesting part of the extrapolations made possible by this understanding of language pressures and change: when we can so clearly demonstrate a pressure and resulting response in language, as we did in Spanish, when another language community tracks closely and then diverges, it might sometimes be possible to make

extralinguistic inferences with consequences beyond linguistics. Using English as an example again, we can see the same broadening of the formal due to overuse and a consequent dilution of its semantic payload of formality — we no longer see *you* as formal in any regard. But when nothing rises to replace *you* as a formal pronoun, we have a question which can be asked: "why?" It suggests that there are other linguistic factors at play which we might understand, or it can provide grounds for historical investigation into the nature of English speaking society within the relevant time period (approximately the 17th century). At our present level of sophistication, we can in some circumstances associate a specific cause with a specific consequence. Once we can do that for just one cause, it suggests that we can repeat the achievement with other causes. Accordingly, the *usted* study of the European T-V distinction's continued evolution develops and demonstrates the very premise that language change can be predictable, can incorporate causal relationships, and that those causal relationships may extend beyond the narrow bounds of just one language. This premise is essential for the value of proposing a multi-factor model for language change. Implicit to the claim that there might be multiple pressures acting within a system is that a sufficient understanding of those pressures may allow one to better describe the movement within that system. One of my goals in this work has been to develop the theory that language change is best modeled as a multi-factor system. That we could isolate a factor as explanatory and predictive of a change already gives credence to the proposition that more complex situations might eventually be understandable with similar clarity (should the plethora of factors be sufficiently well explored and described).

In the study of the affixation asymmetry in chapter 4, we changed the level of analysis. The study of *usted* was a low-level investigation focused primarily on an exemplar of one language, but was representative of a broad class of changes found across European languages

(cf. Helmbrecht 2015). The affixation asymmetry however takes the analysis higher. By evaluating a phenomenon at this level of abstraction and aggregation, we can draw new conclusions. The affixation asymmetry is one of the oldest observations which still generates debate within linguistics. For over a century, it has been a known observed feature of world languages (cf. Sapir 1921) and still has not generated a strong united consensus on the reason for its existence and persistence. The exploration of the affixation asymmetry problem herein served a number of purposes. First, we were able to harness psychological analyses alongside morphosyntactic analyses and phonological analyses. None were discarded out of hand as without relevance or merit. Second, we were able to see the direct application of individual-level explanations, but importantly here, many of these explanations were fully-formed analyses each directly addressing the same phenomenon. In this case we were able to integrate conflicting theories into a single analysis of the complex system. As with the demonstration of the application of causal forces, this represents a major feature of this work: our evaluation of the affixation asymmetry shows that conflicting theoretical positions can all have relevance and that it is not impossible to reconcile them into a single narrative. Indeed, the opposing positions each contributed to a meaningful understanding of the whole, and in concert allowed us to see a complex system of interoperative pressures yielding a surface phenomenon. Thus, in addition to demonstrating the utility of individual-level explanations, this study of the affixation asymmetry also provided affirmation of the theoretical harmonization approach for reevaluating what we actually know and can demonstrate about the nature of language change and typological universals.

Our final study in Chapter 5 took yet another and different route. Instead of looking at a single phenomenon, we looked at the aggregation of typological universals, taking the level of

abstraction again one level higher. Here we established that there is an entire body of correlated typological universals within word order typology and that word order typology is not a static feature within languages or language families. With that in mind, it means that universals which are correlated in and with word order typology must have a mechanism of maintenance. Naturally, these universals appear to have a structural motivation — they are upheld in languages across the world. Our goal therefore was to test the utility of different categories of diachronic explanation against the maintenance of this class of universals, again with the deliberate thumb on the scales to ensure that if a type of explanation could be valid, there would be an opportunity to actually witness it. In doing so, we also demonstrated that correlations with categorical (non-gradient) features (such as syntactic ordering) can be analyzed and explained on a par with more gradient phenomena. There is no need for a different approach or theory when the phenomenon studied is not exactly comparable to previous applications of a theory or research findings. This was not one of our initial research questions, but it is an important finding for the extension of a unifying theory nonetheless because it contributes to the cross-compatibility of different research approaches. Lastly, we were able to directly test structurally motivated analyses against source-based and drift-based explanations. We found that the simple flow or drift from the source would be unable to satisfactorily explain change such as that witnessed in Ancient Greek and that structural pressures were supported in the changes that we analyzed, showing that the Greenbergian Universals are motivated and upheld by the virtues of the structures at play, not by simple drift.

6.2: Source-based Theory

The core goal of this work has been the establishment of a theoretical framework for understanding the changes undergone by languages diachronically. But before we even began, there was already a major theoretical framework which has been gaining traction within the field of diachronic studies. As part of proposing a new approach, a necessary component must be to ask how this other framework applies. Source-based theory is not a theory without some merit. As we discussed in Chapter 2, the framework alleges that the present flows from the past and that new forms today are the consequence of the linguistic forms of yesterday (cf. Aristar 1991; Cristofaro & Ramat 2017; Cristofaro 2017, 2019; Collins 2019). But these claims are realized as a single-factor model.

To use the works by Cristofaro referenced above as an example, diachronic analysis is about the source and then grammaticalization simply happens. The source is the beginning and end of the analysis. In this way, source-based explanations tend to suffer in the same way that any single-factor explanation does: they overweight the significance of a single factor and in doing so blind us to the processes of more complex systems. When we looked at the Spanish pronoun *usted* in Chapter 3, we saw a good example of a phenomenon where a source-based explanation has utility. The transition of *vuestra merced* to more reduced forms was exactly in line with source-based theory: you have a historical starting point, then a well-documented process occurs, and out of the other end comes the descendent form which is dependent on the input. All of this so far is in line with source-based theory and is exactly why I am not so quick to say that source-based explanations have no place in an integrative analysis. Similarly, when looking at the analysis of the affixation asymmetry, a large component of the forms we see in a given language are the consequence of the forms which were available within the language when

the changes were undergone. But this is as far as I can go with the strict source-based approach. I have no ability to apply a source-based analysis in any predictive way that tells us why this particular grammaticalization happened where and when it did. And when we look at both the situation with *usted* in Chapter 3 and the affixation asymmetry in Chapter 4, both have more data to apply than the just the application of a single linguistic process. *Usted* is uninteresting from a source-based analysis except as a simple and classic example of an often-observed phenomenon: grammaticalization. It does not allow for any greater understanding or knowledge, because it is exactly like every other instance of grammaticalization. But upon incorporating more factors and data surrounding its development, especially external and societal factors, *usted* becomes an example of a predictable change. Enough factors were controlled for that we could reasonably associate one change with one cause. Just as importantly, that one cause was something which could be recognized in societies past and present and that could be applied to the analysis of other languages. By limiting ourselves to so spartan an analysis as would be in keeping with a strict application of source-based theory, we would be failing to consider other phenomena of direct relevance for its description and explanation. Similarly, our analysis of the affixation asymmetry quickly developed into a complicated web of interacting forces. The source within the language was only one component and it alone could not account for the differentiated correlations witnessed in the data. We were able to break down the inputs into the development and maintenance of affixes to include linear processing efficiency, perceptual advantage, typological norms of head-ordering, etc and show how these inputs do not always push together in a single direction, and this is reinforced by modern work such as that of Thomas Berg (2020) where ordering biases for syntactic vs morphological elements were actually in opposition to each other (thus allowing us to infer conflicting pressures to be accounted for in analyses on

grammaticalization). Interestingly too, we were able to identify when those pressures might cause well-described processes to run in reverse, such as the relexification of affixes in English, which runs against the core supposition of source-based drift where grammaticalization is a one-way process by definition. Lastly, in Chapter 5, we incorporated the study and analysis by Seržant and Rafiyenko (2021) of the history of word order in Greek. That study was written as a polemic to counter source-based theory. But while it succeeded in demonstrating that more was happening than a simple outflowing from the source, I would not say that it truly broke source-based analysis. Instead it demonstrated what I have proposed here: that there must be more. More than the simple assumption that the source is all you need to be able to explain all the changes. My criticism of source-based analysis is that it is presently implemented strictly with specific and explicit prohibitions against the integration of other causes and factors. Willfully excluding part of the picture necessarily obscures the understanding of the whole. I believe that any researcher in the diachronic space can comfortably agree that for every language there are demonstrable features in the present which are tied to the features and configuration of the past. This is the very premise of historical reconstruction — that something about the present language state has direct linkage to the past language state. If this were not the case, we would not be able to expect a language to last for generations in a recognizable form. Shakespeare's English today is different from what is currently spoken. But it is recognizable. The present did flow with considerable input from the past. Drawing a comparison to the natural world, I can say that the sun will rise in the East roughly 24 hours after the previous sunrise. There is a meaningful observation there and it has value in the construction of our understanding of the world around us. But to frame the sunrise solely as a consequence of the passage of time will not allow us to deconstruct and understand the system. I believe the greatest contribution of source-based

analysis will be in its integration within a multi-factor analytical framework where the specialization in source-influences can be married with the other modes of explanation for a more sophisticated understanding.

6.3: Future Applications

The thrust of this model has been that there is an opportunity for the total integration and representation of different fields of linguistic research to the problem of diachronic analysis. By integrating gains in our synchronic understanding of language and language typology, we gain in our ability to explain diachronic phenomena. The most impactful area of this effect is the merger of historical linguistics and diachronic studies with synchronic linguistic typology, with typology in turn encompassing both psycholinguistic and sociolinguistic considerations in addition to purely grammatical ones in order to explain cross-linguistic patterns synchronically. As discussed earlier in this chapter, we were able to demonstrate the utility of each type of linguistic explanation in the understanding of language change and more importantly, how different well-substantiated explanations can be leveraged together to create a unified narrative of change. This approach of a unified narrative is where the greatest pay-off lies for this research. When we discussed the affixation asymmetry, we were able to demonstrate conflicting forces in the development and decay of linguistic structures, operating together to create the universal patterns observed by Sapir (1921) and Greenberg (1963), among others. This theoretical model has an obvious next step. In the abstract, my work here might be called "pure" research — research done for the purpose of knowledge and understanding but without any immediate applications to the practical world. After pure research comes the process of slowly integrating and applying the understanding gained to the real world. In this case, the future applications fork.

First is the application of this model to future diachronic research, where pressures, whether in conflict or alignment, are accounted for and put in balance. I would expect this to yield continued dividends in the pure research sphere. Language, and the corresponding human experience, is a tremendously complex phenomenon and the greater allowance for complex multifactor models and explanations is essential to continued breakthroughs. Single-factor models simply are not appropriate anymore, given the research which has already been done and is waiting to be capitalized on. The second future application is where we start to entertain practical utility. If a qualitative multi-factor model is already possible now, I expect that a quantitative multifactor model is going to be possible in the future. Should this develop as I expect, we are then looking at the real ability to make predictive statements about language change. In practical applications, this may open the door to predictive future-proofing of human-facing computer systems. One can predict possible changes the language may experience, and pre-seed those possibilities into the model. Even just pre-seeding a learning algorithm with these possibilities could yield better and more responsive systems taking direct user language input where undocumented or unstudied dialectal variation still may be accounted for despite a lack of deliberate support, and language drift in the short term can be already accounted for in the end product. We have already seen that computer systems and interfaces last longer than people initially plan for; a classic example is how only accounting for the then-present requirements for dates in early computers resulted in the need for major emergency corrections ahead of the year 2000 in the famous "Y2K" crisis (cf. Aspinwall et al 2005). We can only expect that designers and engineers of computer systems will occasionally miss obvious future problems again as computers take on an ever-larger role in society, and that proactively building in the ability to

handle language variation and change, even to just a fifty-year time depth, can potentially mean one fewer unforeseen reckoning lurking in the future.

Moving back to the pure research zone, should a quantitative model be made truly viable, we might be able to better interpret clues in historical data. The nature of academia is a constant series of revisions and corrections with the outcome of ever better understanding. Proto-Indo-European was the major breakthrough and triumph of 19th century linguistics (cf. Campbell 2013), but it was not left alone since then. Indeed, 200 years after the initial breakthroughs with Proto-Indo-European (cf. Rask 1818, Grimm 1822; Bopp 1816, 1842); there are still new revisions and improvements on the analysis and postulated features (cf. Hopper 1973, Pereltsvaig & Lewis 2015) as well as attempts to extend the same breakthrough to ever-deeper time depths with reformulations to proposed macro-families dating back tens of thousands of years (cf. Ruhlen 1994, Bomhard & Kerns 2011). Every improvement that is made on the overall understanding of language change writ large presents a new opportunity to improve our maximum time depth of reconstruction and the reliability of our genetic and typological claims about languages and language families.

6.4 Summation

A theory of everything of relevance to change is a potentially wide and rambling set of claims, and it can be easy to get bogged down in the minutiae. In the sections above, we covered those minutiae, but it is worth also outlining our findings in the greatest brevity. We have taken the core axiom of the Uniformitarian Principle and implemented it in a stronger form than has been done before, and by applying it in this more extensive and more thorough manner, we were able to demonstrate the utility of synchronic analyses for diachronic explanation. We can see

from the case studies presented herein that functional pressures appear to act at the level of the individual, society, and the mechanisms of language structure itself. None of the three is without merit for consideration when evaluating a new problem regarding language change. Connected to this finding, we can see demonstrated functional pressures on language — language is a communication tool to be used in human society and it responds to the burdens of that role. Similarly, language is a phenomenon of the human mind and it responds to the physical burdens and costs of the brain. These functional pressures on language are many, and it is specifically through their interaction that we can make claims about the nature of language change — whether predictive or descriptive. We have seen that differential correlates could be explained by functional pressures being either in alignment or in opposition (such as with the affixation asymmetry's overall bias towards suffixation but some word order typologies had sufficient pressures to have both affixation types or even a preference for prefixation). Lastly, by demonstrating the exertion of functional pressures in these different ways, we explicitly reject the premise that language change can be fully explained by simple drift or by any single factor model or by generalizations unique to particular lineages. The only way forward for more robust explanations of language change is the interdisciplinary multi-factor model of competing pressures, and that model needs to be open to the integration of lessons and findings from a broad and disparate array of fields.

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