

# Hard Constraints Mirror Soft Constraints!

Bias, Stochastic Optimality Theory, and Split-Ergativity

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## Abstract

Person-based split-ergativity is one of a number of linguistic phenomena which reflect the general tendency, first noted in Silverstein 1976, for the hierarchy of grammatical functions to be “aligned” with hierarchies of other linguistic substance, such as person. A diachronic, usage based explanation is offered both for the existence of person-based split-ergative systems and for the absence of systems exhibiting the reversal of this marking pattern. This account may be distinguished by its use of a particular grammatical framework, Stochastic Optimality Theory, to provide precise analyses both of the person-based split-ergative system’s synchronic status and also of the process of reanalysis by which, over a period of time, a statistical regularity in language use may be transformed into an obligatory grammatical rule.

## 0 Introduction

One goal of this paper is to defend both a synchronic analysis of person-sensitive split-ergative systems, such as found in the language Dyrbal, and a diachronic explanation of the absence of languages manifesting the inverse of such systems. Another goal is to defend a certain picture of how diachronic explanations of typological generalizations should be structured. Organizing this paper was therefore a difficult challenge, given that I wanted neither of these points to be sidelined in favor of the other. My answer to this challenge may strike the reader as awkward at first, but I believe I have found an optimal solution.

In the first section, I introduce the work of Silverstein 1976, as well as the basic data concerning the split-ergative system in Dyrbal. Section 2 reviews Aissen 1999’s formalization of Silverstein’s hypothesis within the framework of Optimality Theory and its analysis within that framework of the Dyrbal system. In section 3, decisive objections against the Aissen 1999 system are compiled. Section 4 introduces the notion that a diachronic explanation may be offered for the typological universals governing split-ergativity, but also criticizes the form that diachronic explanation receives in such works as Haspelmath 2001. Section 5 reviews the diachronic and usage-based account of the Dyrbal system offered in Zeevat and Jäger 2001. Although most of their “bias-based” account will be incorporated into my final analysis, the account is still subject to two of the same criticisms as Haspelmath 2001. The final few sections of the paper present my own analysis. This analysis makes use of the framework of Stochastic Optimality Theory, so section 6 explains the features of that system. Section 7 is the analysis in detail. The

paper ends with a conclusion.

Although, from a certain perspective, this paper has been written “backwards,” with proffered analysis given last, the reader will find that elements from every preceding section find their way into the final analysis. Because I am in essence building on and combining two different approaches to the same problem, more preliminary work than is usual has had to go into the telling of my story.

## 1 Split-Ergativity, A Creature of Alignment

Within the functionalist tradition of language typology, various analyses of typologically and genealogically separate languages have been taken to indicate the existence of a number of distinct “scales” reflecting a kind of prominence ordering along different linguistic dimensions<sup>1</sup>. Silverstein 1976 was the first to propose the existence of such scales and to relate them to known implicational universals governing morphological and syntactic systems. A more recent version of two of Silverstein’s scales, taken from Aissen 1999, is given in (1).

- (1)
- Local person > Pronoun 3rd > Proper Noun 3rd > Human 3rd >  
Animate 3rd > Inanimate 3rd
- Agent > Patient

Silverstein 1976 proposes that some of the more exotic rule systems employed in native Australian languages may be understood as arising from a general tendency for these scales to be in alignment with one another. It is, in a sense, “more natural” for items at the higher end of the first scale to be agents and for items at the lower end of the scale to be patients. This is evidenced by the relative difference in markedness of these feature combinations. For example, in many languages if a third person inanimate NP rather than a local person pronoun is employed to refer to the agent of the verb, an increase in the sentence’s morphological or syntactic complexity results.

Some split-ergative systems, such as that found in the Australian language Dyirbal, are striking examples of this tendency.<sup>2</sup> In such systems,

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<sup>1</sup>See Aissen 1999, 2000 for discussion.

<sup>2</sup>Throughout this paper, I concern myself solely with split-ergative systems display-

the morphological marking of a pronoun depends both on the person of the pronoun and its grammatical function within the sentence. A pronoun with local person (first or second) is morphologically marked if it is an object of the verb; it is unmarked if it is the subject. A third person pronoun, however, is marked if it is the subject of the verb and unmarked if it is an object. In all persons, the subject of an intransitive verb is unmarked. Thus, local person pronouns can be analyzed as being marked according to a nominative/accusative paradigm, while third person pronouns appear to be marked according to an ergative/absolutive paradigm<sup>3</sup>.

TABLE I  
Person-based split-ergative case marking system (Aissen 1999)

	Unmarked	Marked
Local Persons	Subject	Object
Third Person	Object	Subject (of transitive)
Case	Nominative/Absolutive	Accusative/Ergative

TABLE II  
Case marking in Dyirbal (Aissen 1999)

	Acc	Nom	Abs	Erg
Local Subject-3rd Object		S	O	
Local Subject-Local Object	O	S		
3rd Subject-3rd Object			O	S
3rd Subject-Local Object	O			S
	marked	unmarked	unmarked	marked

Bearing in mind the scales from (1), however, a different perspective emerges. Following Silverstein’s suggestion that these scales prefer to be in

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ing the sort of sensitivities to person as are found in Dyirbal. There are, indeed, split-ergative systems in which tense, rather than person, is the crucial factor in whether an ergative/absolutive or a nominative/accusative marking strategy is used (Hindi being the common example). Clearly, the analyses of split-ergativity discussed here are not meant to cover these other cases. It should be noted in passing that this in no way indicates that these analyses are limited in their coverage. “Split-ergative” is simply a cladistic label and may have no theoretical importance.

<sup>3</sup>It is unclear from the literature I have reviewed whether languages like Dyirbal truly involve the coexistence of two distinct case systems. In this regard, it would be useful to determine whether the marking appearing on local person objects and third person subjects aren’t underlying the same morpheme.

alignment, the most natural combinations of person features and grammatical functions should be those in which local persons appear as the subject of the sentence (quite typically receiving the agent role of the verb) and third persons appear as the object (typically receiving the patient role). These are exactly the combinations which split-ergative languages like Dyirbal leave morphologically unmarked. Furthermore, the most unnatural combinations should be those in which local persons appear as the object of the sentence and third persons appear as the subject, the combinations which these person-sensitive split-ergatives mark morphologically.

In addition to Dyirbal, Silverstein 1976 demonstrates that a variety of morphological and syntactic systems witnessed across languages, and particularly evident in the Australian language families, can be viewed as reflections of this sort of scale alignment. Such a preference for alignment may furthermore be linked to the fact that no languages are known to exhibit the “inverse” of the Dyirbal marking pattern, marking subjects only when they are local persons and objects only when they are third. Linguistic theory, since at least the work of Jakobson, has hypothesized that the complexity of a linguistic sign lies in direct proportion to its “special-ness” or “unnatural-ness”. Thus, a language showing a disposition to mark objects only in the third person would vitiate the universal “natural” status of these combinations, as predicted by Silverstein’s scale-alignment hypothesis.

A skeptical linguist might, however, wonder why these scales should appear in alignment. Linguistic theory contains a number of “hierarchies”: the sonority hierarchy (vowel > glide > fricative > affricate > stop), the agreement hierarchy (adjective > relative pronoun > anaphoric pronoun), the number scale (singular > plural > dual > trial). Why aren’t *all* of these scales in alignment? What determines which scales align with one another? A common answer to these questions originates in the functionalist literature following Silverstein 1976. This literature traces the cause of scale-alignment to the typical habits of language use. The alignment of the person scale with the scale of grammatical functions, for example, is said to be the result of the habits people have of speaking of themselves or their audience. “Local person” is aligned with “Subject” because the subject of the sentence is quite typically the topic, and the topic of the sentence is quite typically the speaker or his audience. The “alignment” of these scales is thus often understood as being a mere metaphoric expression of the statistical tendency, witnessed in scores of corpus studies, for subjects to be local persons and objects to be third.

The proposal that linguistic markedness corresponds in some cases to clashes of alignment between various prominence scales has enjoyed its rich-

est popularity within the typological studies carried out by linguists of a more “functionalist” bent <sup>4</sup>. Thus, the majority of analyses based on the ideas in Silverstein 1976 bear those features which many generative linguists find most “irksome” in the functionalist literature: a lack of precise synchronic analysis and only the vaguest hints of what the mechanisms and principles supporting the generalization are. Aissen 1999 argues, moreover, that as stated the generalization made in Silverstein 1976 is in some cases too weak and in others incorrect.

Such weaknesses of Silverstein’s original proposal, combined with its overall merit, point to the need for generative linguists to address the data supporting Silverstein’s scale-alignment hypothesis and to build more explicit, formal models of Silverstein’s proposal. The best known work in this area is Aissen 1999, which also provides one of the most popular analyses of the Dyirbal system.

## 2 Aissen 1999’s Analysis of Split-Ergativity

### 2.1 Harmonic Alignment and Prominence in Morphosyntax

The analysis proposed in Aissen 1999 is born from the observation that the mechanism of Harmonic Alignment developed in Optimality Theory (Prince and Smolensky 1993) may be used to state Silverstein’s scale-alignment hypothesis within the formal framework of OT. Harmonic Alignment was introduced into Optimality Theoretic phonology in an attempt to develop a theory of the correspondence between different phonological prominence dimensions. If we are given two prominence scales  $S_1$  and  $S_2$ , Harmonic Alignment creates a set of OT constraints which enforces the association of elements from the upper end of  $S_1$  with those from the upper end of  $S_2$ . More concretely, assume that linguistic items may be categorized according to two features  $A$  and  $B$ , and that  $A$  has two possible values  $A_1$  and  $A_2$  while  $B$  has  $n$  possible values  $B_1 \dots B_n$  for some  $n \geq 2$ . Moreover, suppose that there exists an ordering  $>_A$  and an ordering  $>_B$  such that

$$(2) \quad \begin{array}{l} A_1 >_A A_2 \\ \\ B_1 >_B B_2 >_B \dots >_B B_n \end{array}$$

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<sup>4</sup>Independently of Silverstein 1976, the concept has also made its way into phonological theory, particularly within the study of prosody and syllable structure.

The Harmonic Alignment of  $A$  with  $B$  is the set of features  $A_1/B_j$  and  $A_2/B_j$  (read as “the combination of  $A_1$  ( $A_2$ ) with  $B_j$ ) under the ordering  $\succ$  characterized as follows.

$$(3) \quad \begin{aligned} A_1/B_1 &\succ A_1/B_2 \succ \dots \succ A_1/B_n \\ A_2/B_n &\succ A_2/B_{n-1} \succ \dots \succ A_2/B_1 \end{aligned}$$

The scales in (3) represent the combination of  $A_1$  with a highly ranked  $B$  feature as being more “harmonic” than its combination with a lower ranked  $B$  feature. The same holds for  $A_2$  combined with a lowly ranked  $B$  feature. Such a scale then induces the creation of set of constraints  $*A_j/B_i$  universally ranked in the order below.

$$(4) \quad \begin{aligned} *A_1/B_n &\gg *A_1/B_{n-1} \gg \dots \gg *A_1/B_1 \\ *A_2/B_1 &\gg *A_2/B_2 \gg \dots \gg *A_2/B_n \end{aligned}$$

This “universal ranking” of the constraints requires only that all grammars respect the *relative* ranking given above. Grammars may differ in whether other constraints are interpolated between those in the universal “subhierarchy” and how different Harmonically Aligned subhierarchies are ranked and interpolated relative to one another.

Aissen 1999 shows that Harmonic Alignment is indeed tailor-made for importing Silverstein 1976’s markedness theory into OT. If  $A$  is taken to be the dimension of person, consisting here of the values “Local” and “3<sup>rd</sup>”, and  $B$  is the dimension of grammatical function, then Silverstein’s observation provides us with the following two scales.

$$(5) \quad \begin{aligned} \text{Local} &> 3^{rd} \\ \text{Subject} &> \text{Object} \end{aligned}$$

The operation of Harmonic Alignment thus produces the universal subhierarchies below.

$$(6) \quad \begin{aligned} *Local/Object &\gg *Local/Subject \\ *3^{rd}/Subject &\gg *3^{rd}/Object \end{aligned}$$

The presence of these universal subhierarchies of markedness constraints captures, at least rhetorically, the original insight of Silverstein 1976, that language prefers for local persons to be subjects and for third persons to be objects. The constraint \*Local/Object penalizes candidate structures in which an NP of local person lies in object position. This constraint is more highly ranked than \*Local/Subject, and so languages are predicted to “disprefer” local person objects to local person subjects. This is, of course, simply an inverted way of saying that language prefers local person subjects to local person objects. Similarly, the ranking \*3<sup>rd</sup>/Subject  $\gg$  \*3<sup>rd</sup>/Object reflects the preference of language for 3<sup>rd</sup> person objects over 3<sup>rd</sup> person subjects.

In itself, this is not an impressive result. However, Aissen 1999 demonstrates that these systems of constraints predict precisely the typological universals discussed in Silverstein 1976, and do so in a more explicit and controlled manner than Silverstein’s original hypothesis. The interpolation of the universal subhierarchies with a modest number of other constraints yields the wide variety of linguistic rule systems discovered through the work of Silverstein and others<sup>5</sup>. In demonstrating how Harmonic Alignment predicts such rule systems, Aissen 1999 simultaneously provides explicit synchronic analyses of these languages within the framework of OT, circumventing one of the deepest weaknesses of the functionalist tradition following Silverstein. Furthermore, the inability to rerank constraints within the subhierarchies explains the absence of the unattested rule systems. Thus, the absence of these languages is more definitively shown to be the result of the universal “natural-ness” of certain feature combinations.

## 2.2 The Analysis of Dyrbal

One component of the argument in Aissen 1999 is the use of OT equipped with Harmonic Alignment to provide deep analyses of grammatical systems which appear subject to the alignment of the scales in (1). One such system, we’ve noted, is the split-ergative marking strategy of Dyrbal. Aissen 1999 begins its discussion of Dyrbal with the Jakobsonian observation that languages tend to morphologically mark feature combinations which are “unnatural” or “special.” This tendency is modeled within Aissen’s OT system through the introduction of two constraints, \* $\emptyset$  and \**STRUC*, and the

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<sup>5</sup>Readers familiar with OT may balk at my use of “rule systems” here, given that OT is not a rule-based theory. Nothing interesting is intended here by “rule system”; it may be taken as synonymous with “system.”

mechanism of Local Conjunction.

Local Conjunction, like Harmonic Alignment, is a formal operation on OT constraints, which creates new constraints from ones already present in the grammar. First introduced in Smolensky 1995, Local Conjunction takes as its input two constraints,  $C_1$  and  $C_2$ , and a domain type  $D$ . It returns a constraint  $C_1 \& C_2$  which is violated when there is some domain of type  $D$  in which both  $C_1$  and  $C_2$  are violated. The intuition behind this operation is that in some cases “two constraint violations are worse when they occur in the same location” (Smolensky 1995). That it is always *worse* to violate  $C_1 \& C_2$  than either constituent constraint individually is enforced by the condition that  $C_1 \& C_2$  must universally outrank both  $C_1$  and  $C_2$ . Again, grammars are permitted to interpolate constraints between  $C_1 \& C_2$  and its constituent constraints, but the relative ranking of  $C_1 \& C_2$ ,  $C_1$  and  $C_2$  must always be preserved.

The constraint  $*\emptyset$  (read, “star zero”) is violated whenever morphological marking for a particular feature is absent <sup>6</sup>. Aissen 1999 posits that in languages like Dyrbal, the subhierarchies  $*\text{Local/Object} \gg * \text{Local/Subject}$  and  $*3^{rd}/\text{Subject} \gg *3^{rd}/\text{Object}$  are both locally conjoined with  $*\emptyset$ . An operation of “Conjunction with Subhierarchies” is introduced which results in the creation of the following new constraint subhierarchies.

$$(7) \quad \begin{aligned} & * \text{Local/Object} \& * \emptyset \gg * \text{Local/Subject} \& * \emptyset \\ & * 3^{rd}/\text{Subject} \& * \emptyset \gg * 3^{rd}/\text{Object} \& * \emptyset \end{aligned}$$

These subhierarchies capture (again, at least rhetorically) the fact that it is more “natural” for a local person object to be marked morphologically than for a local person subject. Similarly, they represent that languages prefer to mark third person subjects rather than third person objects. More to the point, since the order of the constraints within any locally conjoined subhierarchy is predetermined by the order of the constraints within the subhierarchy from which it is derived, Aissen 1999’s operation of locally conjoining  $*\emptyset$  with Harmonically Aligned subhierarchies reflects the Jakobsonian generalization that unnatural configurations of features are more likely to be morphologically marked than natural ones.

The constraints in (7), however, are not yet enough to explain the pat-

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<sup>6</sup>As such,  $*\emptyset$  may be better thought of as a family of constraints, each indexed by the feature whose marking it enforces, but for present purposes, such considerations are unimportant.

tern of marking observed in Dyirbal. Taken on their own, these constraints will require the marking of *all* combinations of person features and grammatical functions. There must, then, be some interacting constraint which serves to suppress morphological marking. *\*STRUC*, a constraint which penalizes the realization of morphological structure, seems a likely candidate. Depending on where *\*STRUC* is interpolated, the grammar will exhibit different marking strategies. For example, if *\*STRUC* is ranked below both subhierarchies, the resulting grammar will mark all combinations of person and grammatical role; languages like Latin are an example of such a strategy. If *\*STRUC* is ranked above both subhierarchies, the language will not case mark any arguments. The Dyirbal system is created by the ranking shown below.

$$(8) \quad \begin{aligned} &*Local/Object \ \& \ * \emptyset \gg *3^{rd}/Subject \ \& \ * \emptyset \gg *STRUC \gg \\ &*Local/Subject \ \& \ * \emptyset \gg *3^{rd}/Object \ \& \ * \emptyset \end{aligned}$$

In this system, a candidate in which a local person object or a third person subject goes unmarked will lose to any competing candidates in which those constituents appear marked. However, any candidate in which a local person subject or a third person object appears marked will be eliminated by the constraint *\*STRUC*, which dominates all constraints preferring the markedness of such combinations. This is illustrated in tableaux (1) and (2).

TABLEAU 1  
Dyirbal ( $3^{rd}$  person subject/Local person object)

V(Agt/3, Pat/1)	*Local/O & * $\emptyset$	* $3^{rd}$ /S & * $\emptyset$	* <i>STRUC</i>	*Local/S & * $\emptyset$	* $3^{rd}$ /O & * $\emptyset$
Subj/3-ERG, Obj/1- $\emptyset$	*!		*		
Subj/3- $\emptyset$ , Obj/1-ACC		*!	*		
Subj/3- $\emptyset$ , Obj/1- $\emptyset$	*!	*			
$\Rightarrow$ Subj/3-ERG, Obj/1-ACC			**		

TABLEAU 2  
Dyirbal (Local person subject/ $3^{rd}$  person object)

V(Agt/1, Pat/3)	*Local/O & * $\emptyset$	* $3^{rd}$ /S & * $\emptyset$	* <i>STRUC</i>	*Local/S & * $\emptyset$	* $3^{rd}$ /O & * $\emptyset$
Subj/1-ERG, Obj/3- $\emptyset$			*!		*
Subj/1- $\emptyset$ , Obj/3-ACC			*!	*	
$\Rightarrow$ Subj/1- $\emptyset$ , Obj/3- $\emptyset$				*	*
Subj/1-ERG, Obj/3-ACC			**!		

When the output sentence structure must contain a local person object and a third person subject, the optimal candidate is that in which both the subject and object are morphologically marked. Similarly, if the output contains a third person object and a local person subject, the optimal structure is that in which *neither* are marked. As the reader can quickly check, the system of case marking described in Table II is predicted.

### 3 Criticisms of Aissen 1999 <sup>7</sup>

Aissen 1999 successfully imports into a generative formal framework the insights first expressed in Silverstein 1976. It also stands as a landmark use of Optimality Theory within the study of syntax, and has demonstrated the potential utility of OT outside of phonology. Nevertheless, what gains the theory makes are offset by a number of non-trivial difficulties, all of which spring from the mechanisms of Harmonic Alignment and Local Conjunction.

#### 3.1 No Principled Account of Scalar Direction

In Aissen 1999, the scales Local > 3<sup>rd</sup> and Subject > Object seem to come down from on high. No principled explanation is offered of why the prominence order is in the one direction rather than, for instance, 3<sup>rd</sup> > Local or Object > Subject. One possibility is, of course, that these scales are universal and fixed, but until more is said about their ontological and psychological status, this would remain merely a stipulation. Thus, nothing yet seems to prevent the existence of “inverse Dyrbal”, a language in which local persons are aligned with the object position and third person with the subject.

Notice that, as stated, this is a criticism of the original work in Silverstein 1976 as well. Although, as was mentioned, there exist in the functionalist literature attempts to explain the alignment of these scales in terms of habits of use, nothing has been offered to explain the order of the scales as given. In the OT system of Aissen 1999, where the grammar is formally encapsulated from the influence of pragmatic tendencies <sup>8</sup>, this matter is simply all the more pressing. One may like to explore the possibility of understanding these prominence scales, as well as their alignment, as simply metaphoric expressions of statistical tendencies rooted in habits of speech. However, it is clear that the structure of the Aissen 1999 system prevents one from

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<sup>7</sup>The criticisms presented here are not original, and are taken from various sources: Bresnan et al. 2001, Haspelmath 2001 and Zeevat & Jäger 2001.

<sup>8</sup>Or, at least pragmatic tendencies are not an explicit component of the theory.

doing so. The scales are reified: Harmonic Alignment isn't defined over metaphors.

### 3.2 No Principled Account of Alignment

A related criticism is that by characterizing the operation of alignment between scales as a formal operation within the grammar, Aissen 1999 is subject to the criticism levied in section 1: there seems to be no principled account of why certain scales align with one another and not others. Why doesn't Harmonic Alignment operate on all linguistic scales? Why are the person and grammatical function scales Harmonically Aligned but not the person and number scales? The notion, popular with functionalists, that scale alignment results from pragmatic tendencies is tempting, but as yet it is impossible to incorporate into the system.

### 3.3 Strength of the Machinery

Although both Harmonic Alignment and Local Conjunction were independently introduced into Optimality Theory, one may still be wary of their regular appearance. There does not yet exist a comprehensive theory of these operations, nor has it been shown that they somehow follow from any independent principles of OT. In itself, this may cause linguists of more "Minimalistic" tendencies some discomfort. Behind these seemingly reactionary fears of added machinery, however, there lie real worries about the expressive power these operations add to OT. On this matter, nothing is widely known.

Moreover, there is a real sense in which these operations, though "technically compatible with the overall OT architecture . . . (do) not fit in very naturally" (Zeevat and Jäger 2001). Both operations involve the creation of "subhierarchies", sets of constraints whose relative ranking is invariant across languages. The existence of such subhierarchies weakens the claim that OT provides a purely "factorial" typological theory, in which the space of possible grammars is defined as the set of *all* re-rankings of the universal constraint set. Moreover, it's clear from Aissen 1999 that the operations of Harmonic Alignment and Local Conjunction cannot apply identically across languages (c.f. 5.2 and 5.3). The theorist, then, can no longer categorically assert that the set of OT constraints is universal. This introduces a new complexity into OT learning theory which, again, does not seem to have yet been studied.

Besides committing these "crimes against elegance," the very idea of

local conjunction appears antithetical to OT. The one characteristic of OT which most sets it apart from other theories of constraint interaction is its reliance upon a relation of “strict domination” between the constraints. If a constraint  $C_1$  strictly dominates the constraint  $C_2$ , then a candidate that is dispreferred by  $C_1$  cannot possibly be optimal, no matter how strongly  $C_2$  prefers that candidate over others. Local conjunction, by its very nature, militates against this. Consider tableau (3).

TABLEAU 3

	$C_1 \& C_2$	$C_3$	$C_1$	$C_2$
$\alpha$	*!		*	*
$\beta$	*!		*	*
$\Rightarrow \gamma$		*(!)		

Limiting attention to the subtableau beginning with constraint  $C_3$ , one finds that candidates  $\alpha$  and  $\beta$  outperform candidate  $\gamma$  on  $C_3$ . Since  $C_3$  dominates  $C_1$  and  $C_2$ , the fact that  $C_1$  and  $C_2$  each prefer  $\gamma$  should be of no consequence, and both  $\alpha$  and  $\beta$  ought to be derived as the optimal candidates. However, that’s not what occurs in tableau (3). The joint violations of  $C_1$  and  $C_2$  by  $\alpha$  and  $\beta$  combine together and result in their violating  $C_1 \& C_2$ , which is ranked above  $C_3$ . In effect, the system becomes one in which violations of the dominated constraints  $C_1$  and  $C_2$  may “multiply” together to form constraint violations more severe than violations of constraints strictly dominating  $C_1$  and  $C_2$ . Although, on paper, the process of calculating optimal candidates remains unchanged, it is certainly questionable whether such systems as these truly involve relations of “strict domination.”

### 3.4 Harmonic Alignment Unnaturally Limited

As is clear from its definition, Harmonic Alignment can only apply to a pair of scales if one of the two is binary. The effect of this restriction is obvious throughout Aissen 1999 and 2000. For example, in Aissen 2000’s study of Differential Object Marking, the observation that grammatical function aligns with animacy in some languages is captured by aligning a binary scale of grammatical function with a ternary scale of animacy.

- (9)  
subject > object  
human > animate > inanimate

Although this binary scale of grammatical functions is adequate for the data Aissen 1999 and Aissen 2000 consider, the full scale proposed by language typologists is much richer, and contains at least the following elements.

- (10) subject > object > indirect object

One wonders where, in the scale in (9), the other grammatical functions of the hierarchy have gone to. Dissatisfied with using such an unnaturally truncated scale, one may wish to align the ternary hierarchy in (10) with the ternary animacy hierarchy in (9). Doing so may indeed create new systems of marking whose existence is at least as plausible as the person-based split-ergative systems. Unfortunately, we cannot extend the operation of Harmonic Alignment to these scales in their entirety.

### 3.5 Presence of “Unfriendly” and “Inactive” Constraints

Harmonic Alignment operating on the scales in (5) creates markedness constraints like \*Local/Subject which are violated by sentences in which the *least marked* combinations of person features and grammatical function appear. As Zeevat & Jäger 2001 points out, “Technically this is harmless because they are always dominated by constraints that are effectively their negation. Nevertheless, one rather does without constraints that exclude the least marked configurations one can imagine.” Moreover, many of these “unfriendly” markedness constraints are inert and play no role in the actual syntactic analyses proposed in Aissen 1999. It would be preferable to have some means of removing these constraints from the system, but as presently defined the operation of Harmonic Alignment does not permit this.

### 3.6 Unconstrained Nature of Local Conjunction

The existence of person-based split-ergative systems and the absence of languages exhibiting the inverse of these systems is clearly a result of the tendency language has to correlate the “natural-ness” of a feature combination with the morphological simplicity of its linguistic realization. Aissen 1999,

then, reasonably attempts to include this tendency as part of its analysis of these systems, and it does so by locally conjoining the subhierarchies  $*Local/Object \gg *Local/Subject$  and  $*3^{rd}/Subject \gg *3^{rd}/Object$  with  $*\emptyset$ . However, nothing within the theory of Local Conjunction prevents one from conjoining these subhierarchies with  $*STRUC$  instead. Indeed, if  $*\emptyset$  and  $*STRUC$  are exchanged for one another in the ranking for Dyrirbal given in (8), the following OT system results.

$$(11) \quad \begin{array}{l} *Local/Object \ \& \ *STRUC \ \gg \ *3^{rd}/Subject \ \& \ *STRUC \ \gg \ *\emptyset \ \gg \\ *Local/Subject \ \& \ *STRUC \ \gg \ *3^{rd}/Object \ \& \ *STRUC \end{array}$$

As the reader is invited to check, this ranking produces the unattested inverse of the person-based split-ergative system. Although the system of constraints in Aissen 1999 rules out such grammars, some account is needed of why the system of constraints above is impossible.

### 3.7 Predicts Unattested and Implausible Languages

In Aissen 1999, the operation of Harmonic Alignment at one point creates the system of constraints given in (6), repeated below.

$$(6) \quad \begin{array}{l} *Local/Object \ \gg \ *Local/Subject \\ *3^{rd}/Subject \ \gg \ *3^{rd}/Object \end{array}$$

Bresnan et al. 2001 observes that the constraints  $*Local/Object$  and  $*3^{rd}/Object$  taken together penalize transitivity. Thus, if ranked high enough, these constraints produce a language in which passivization is obligatory in all sentences. That such a language has never been documented and is typologically quite counterintuitive poses a problem whose solution is not clear.

### 3.8 False Predictions for Dyrirbal System

Aissen 1999 notes in passing that the constraint system proposed for Dyrirbal in (8) wrongly predicts that morphological marking should appear on  $3^{rd}$  person subjects of *intransitive* verbs as well. The cause of this error is clear: the constraints proposed are sensitive only to the combinations of person feature and grammatical role and are insensitive to the valency of the

matrix verb. Aissen 1999's suggested remedy is the device of ever more complex constraints, locally conjoining the hierarchy in (7) with the constraint \*Person/Object, which assigns a violation to any sentence containing an object. Although this certainly does the trick, it is quite clearly ad hoc, and the constraint \*Person/Object is of dubious origin and existence.

In its discussion of this matter, however, Aissen 1999 contains some rather prescient comments, ones which hint at the general perspective that will be pursued throughout the rest of this paper.

What is at issue, though, seems to be the functional motivation for case marking, which is generally seen as discriminating subject from object. But the need to distinguish subject from object arises only in transitive clauses. The fact that the constraints [in (7)] are only relevant then in transitive clauses seems reasonable, but the present analysis does not restrict them to transitive clauses. What appears to be needed here is a more systematic view of case marking, one which can formally express the functional motivation for case marking. (Aissen 1999, p. 703)

### **3.9 Conclusion**

Although Aissen 1999 achieves precise synchronic analyses of the various languages subject to Silverstein 1976's generalization, and captures that generalization within a relatively well-understood formal framework, the methods employed to do so are questionable. On the one hand, one should follow Aissen 1999 in seeking a deeper theory of prominence alignment in syntax, which can both provide precise typological predictions and real synchronic analyses. That such a theory should employ the OT operation of Harmonic Alignment, on the other hand, is unquestionably incorrect.

## **4 Typological Universals, Diachronic Explanation, Universal Grammar**

### **4.1 The Horns of the Dilemma**

Two attempts at incorporating Silverstein 1976's scalar-alignment hypothesis into a theory of language have so far been encountered. The uses made of Silverstein's hypothesis in the functionalist literature are too often inexplicit about the synchronic features of the languages under consideration and provide only informal accounts of why language follows the pattern observed by

Silverstein. The attempt made by Aissen 1999 to bring the scale-alignment hypothesis within the purview of generative grammar succeeds at providing a number of clear, synchronic analyses, and in a way yields a richer picture of how Silverstein’s generalization works to limit the space of possible languages. However, this particular approach has some fundamental drawbacks: characterizing the alignment of scales as a purely “grammar-internal” operation renders unanswerable the question of why some scales are aligned rather than others, and simply introducing “alignment” into the grammar appears to require the use of some rather heavy machinery whose behavior we do not at this time know how to control. We find ourselves between the horns of a dilemma. A functionalist account of Silverstein’s generalization provides the gift of relative theoretical parsimony and a partial understanding of scale alignment, but it comes at the cost of losing synchronic analyses and more explicit models of typological restrictions. However, Aissen 1999’s gift of synchronic analyses and more explicit models of typological restriction comes at the cost of a rather heavy theory and an ignorance of how scales come to be aligned. Can’t we have the best of both worlds? That we can is what I shall spend the remainder of this paper defending.

## 4.2 Diachronic Paths as Source of Explanation

“Grammars do best what speakers do most” goes the Boisian aphorism (Du Bois 1985). One of the guiding principles of functionalist typology since the 1970’s, it is based on the observation that structures which are categorically banned in many languages are similarly “dispreferred” in languages where they are permissible. It is often the case that when a language  $L$  allows a form  $\phi$  which another language  $L'$  does not permit, that  $\phi$  has a very low probability of occurring in spoken language corpora for  $L$ .

It has already been noted that many functionalists take Silverstein 1976’s generalization and the grammars which support it as instances of this general tendency. An example which illustrates both this tendency and how it relates to the ideas in Silverstein 1976 is the language Lummi, spoken in British Columbia. As discussed in both Aissen 1999 and Bresnan et al. 2001, Lummi, like Dyirbal, appears to enforce alignment between the person and grammatical role hierarchies. In transitive sentences, the person of the subject cannot be “higher” than the person of the object. If the agent of the verb is a third person while the theme is local, passivization of the verb is obligatory. Similarly, if the agent of the verb is a local person while the theme is third, passivization is forbidden. Thus, all sentences in Lummi exhibit those configurations of grammatical role with person predicted by

Silverstein 1976 to be most “natural.” Moreover, there is no known language which shows the inverse of the Lummi system, requiring passivization when the subject is of higher person than the object, and banning it when the object is of higher person.

Besides being in accord with Silverstein 1976’s markedness theory, Lummi also offers an example of language doing best what speakers do most <sup>9</sup>. As Bresnan et al. 2001 demonstrate, even in languages permitting the active and passive sentences that are the pariahs of Lummi, one can find statistical reflections of the Silversteinian markedness theory. English, for example, permits both active and passive sentences containing all combinations of person feature and grammatical role. In the SWITCHBOARD corpus of spoken English telephone conversations, however, one finds that the great majority of passives are those in which the subject patient is a local person and the oblique agent is a third person. Thus, if the passive is employed by English speakers, it is much more likely to be in a structure for which the passive would be obligatory in Lummi. <sup>10</sup>

All of this, of course, prompts a dumbfounded “why?” From a certain perspective, these facts are rather disturbing. What exactly are these Silversteinian scales, where are they, and by what dark devices are they exerting their influence over speakers of English? From a different perspective, however, these facts become far more mundane. All the worry arises from taking the concept of the Silversteinian scale too seriously <sup>11</sup>. The tendencies for use of the passive discovered in the SWITCHBOARD corpus are not the *result* of hierarchical alignment, but in a real sense are its source. As has been adumbrated in earlier sections, the idea is that Silverstein 1976’s “scales” and their “alignment” is simply a visual metaphor for the fact that there exist statistical tendencies of the kind discovered in the SWITCHBOARD corpus, statistical tendencies which find their roots in more fundamental habits of language users. For example, it is already known that, for various reasons to do with informational structure, the subject of a sentence is quite often the sentence topic. Moreover, it is simply a basic psychological fact about human beings—obvious from a cursory glance at any language corpus—that the topic of our sentences and conversations is overwhelmingly ourselves or

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<sup>9</sup>Or rather, language doing worst what speakers do least, which, I will argue, is perhaps a truer analogon of the aphorism.

<sup>10</sup>Although, as this sentence itself indicates, the strongest factors influencing the appearance of passive appear to be related to the maintenance of topic.

<sup>11</sup>As, for example, it is in Aissen 1999 and Bresnan et al. 2001. Both of these accounts require the reification of the prominence scales and their inclusion within the grammatical system. Section 3.1 already indicates reasons for not pursuing such a course.

our audience: the local persons. When added with the fact that the passive construction in English is often deployed for reasons of maintaining topic, we can quite easily understand the English “alignment” of local persons with subject position, witnessed in our usage of the passive, as emergent from these more basic habits.

This would explain why scalar alignment is reflected in the statistical preferences of languages such as English. But scalar alignment was introduced in Silverstein 1976 as an explanation of cross-linguistic markedness. How could a “visual metaphor” for statistical tendencies come to reflect itself in constraints on rule systems? The answer popular in the functionalist literature is that paths of diachronic development can bear a large part of the burden in explaining typological universals. Research into language diachronics, it is claimed, has shown that what begin as speaker strategies or tendencies eventually become “fossilized” or “entrenched” as grammatical rules. Concretely, fundamental features of human psychology entail the overwhelming tendency across all languages for subjects to be local persons. In particular languages, this can result in a variety of marking strategies used to distinguish “unlikely” combinations of person features and grammatical roles. These marking strategies are, at first, simply optional “stylistic” conventions used to discourage speakers from interpreting unlikely configurations as instances of more frequent ones. After a time, these strategies become “entrenched”, or (to use the more popular name) “grammaticalized” as obligatory rules in the grammar. Thus, the absence of rule systems going against the Silversteinian scale alignment is the result of the absence of pragmatic strategies which would have to be their diachronic source; the absence of these strategies is due to the absence of the statistical frequencies which would have to be their *raison d'être*; the absence of such frequencies is, to complete the circle, due to basic facts of human psychology.

The preceding is, of course, merely the sketch of an explanation; it offers only the form which a potential analysis of the grammatical systems covered by the Silverstein 1976 generalization could take. Analyses adopting this form typically identify themselves as “usage-based” explanations. There are many “usage-based” analyses within the functionalist literature, and we will later in the paper defend an analysis of the person-based split-ergative system which proceeds along these lines. However, in order for a usage-based analysis to truly constitute an *explanation*, and not merely another sketch of an explanation, there are a number of questions which it must clearly and explicitly address.

**What are the Statistics, and How Robust are They?** Needless to say, if a proposed usage-based account does not even establish that the statistical tendency it is basing itself on exists, then it can hardly be considered complete. Similarly, the crucial statistical generalization should hold over a variety of corpora studies. Of course, in the interests of devising practicable research projects and not boring a theoretical audience with statistics, this latter condition can be, and is often, greatly relaxed. The condition that an actual statistical study be mentioned or cited in the analysis is, thankfully, never transgressed.

**How Do the Statistical Patterns Emerge from More Basic Ones?**

The claim that “since subjects are most likely topics, and topics are most likely local persons, then subjects should be most likely local persons” sounds plausible, but it must be confirmed by an actual statistical model. The problem is that this inference is, taken on its own, not valid. From the facts that the majority of celebrity activists are millionaires, and that the majority of millionaires are criticized by celebrity activists, it does not follow that the majority of celebrity activists are criticized by celebrity activists. There are finer relationships between the different variables which must also be stated in order for the inference to go through. Unfortunately, one finds far too often in the functionalist literature appeals to inferences of this form without the additional work of providing the “implicit premises” which truly justify the reasoning. The following, taken from Haspelmath 2001, is characteristic.

I have proposed that the DPRC should be explained with reference to frequency in language use and grammaticalization: because the person and role scales correlate strongly with topicality and animacy, “high persons” tend to occur in “high roles” and “low persons” tend to occur in “low roles”, i.e., harmonic person-role associations are more frequent than disharmonic person-role associations. (Haspelmath 2001, p. 23)

It isn't immediately clear what precisely the author's reasoning here is, but it seems to be an inference of the form “person is highly correlated with topicality; topicality is highly correlated with role; thus, person is highly correlated with role.” Charitably interpreting the author as not engaging in a fallacy, he must inform the reader more explicitly of the subtler relationships between person, role and topic which justify his “emergentist” account of scalar alignment. Otherwise, there is just no reason to believe the emergentist account, and our ignorance of how person becomes “aligned” with role is not relieved.

**How Do the Pragmatic Strategies Arise?** A necessary component of any usage-based account is the claim that at some point in a language’s development, speakers begin to adopt various marking strategies which help to distinguish “unlikely” combinations of person and grammatical function from “likely” ones<sup>12</sup>. These strategies become entrenched over time as grammatical rules, and thus arises the cross-linguistic tendency for more “unlikely” configurations to be more morphologically or syntactically marked. Implicit in this story, however, is the assumption that when such “pragmatic strategies” are introduced, there exists some pressure for speakers to mark unlikely combinations rather than the likely ones. That is, if all that motivates the new pragmatic strategy is a need to disambiguate person and role configurations, a strategy of using a special marking for likely combinations should prove just as effective as correlating the marking with unlikelihood. One may easily imagine a community of speakers which adopts a strategy of placing a special morphological marking on the most common configurations of person and role; the absence of this marking would then communicate the use of the unlikely combination.

If the proposed “usage-based” explanation is to be believed, why then did the unlikely combination become specially flagged? Notice that, although it appears to be closely related, an appeal to a principle such as “Zipf’s Law” would be a *non sequitur*, since diachronic processes of phonological reduction cannot affect what strategies speakers at a particular time invent. Furthermore, any appeal to the cross-linguistic tendency for unlikely combinations of grammatical function and person to be morphologically marked would be circular.

The need for a usage-based account to address this question is real; this is not a matter of peripheral concern, nor is it simply a trivial case of “any explanans is itself an explanandum”. If it were somehow discovered that there often arise pragmatic marking strategies which specially flag likely rather than unlikely combinations, then the usage-based explanation would suddenly predict the possibility of languages which obligatorily mark only the most harmonic combinations of person and role. Given how little seems to be known about these putative “pragmatic marking strategies”<sup>13</sup>, the proponent of a usage-based account should at least provide some reason

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<sup>12</sup>A point of clarification: certainly the language at this time will have some syntactic or morphological marking system already in place for making these distinctions. The strategies are introduced at first only to *support* these systems, which for various reasons are entering into a state of erosion.

<sup>13</sup>Strangely, they seem only to be discussed by functionalists employing their existence in usage-based explanations, and even there no general studies of them appear cited.

for doubting the existence of strategies which, according to the analysis's own logic, would eventually lead to the creation of languages the analysis is meant to exclude. Put simply, the usage-based account does not truly predict the absence of the unattested languages until it is made more clear why the pragmatic strategies it posits are the only ones possible. It is greatly worrying that this issue is not addressed in the "grammaticalization" literature, and it is not evident that the functionalist typological literature even recognizes it as a problem.

**What are the Possible Ways These Grammatical Systems Can Develop?** The proponent of a usage-based explanation commits himself to a particularly strong diachronic claim. The logic of a usage-based account is that "since grammatical system *A* develops from pragmatic tendency *B*, and since pragmatic tendency *B* cannot have property *C*, then grammatical system *A* cannot have property *C*. The previous question addresses the regular assumption that "pragmatic tendency *B* cannot have property *C*," but just as questionable an assumption is that "grammatical system *A* *must* develop from pragmatic tendency *B*." If the generalization to be explained is that *A* always lacks property *C*, then the existence of other possible diachronic sources for *A* defeats the usage-based explanation for the generalization. Conserving the usage-based account would require a disjunctive explanation, and disjunctive explanations tend to have a very short shelf life.

Unfortunately, although the champions of usage-based accounts often recognize their commitment to this strong historical claim, it is not typically defended in their works and is sometimes incorrect. Haspelmath 2001, for example, proposes a usage-based account of an implicational universal concerning possible pronominal clitic combinations. The account quite clearly requires that pronominal clitics only develop from full, independent pronouns, and the author equally clearly recognizes this commitment.

The synchronic distribution of grammatical systems is constrained in the observed way because of a restriction on the way in which languages change. The unattested language type [languages in which the implicational universal is falsified] does not exist because there is *no way in which it could arise*, not because of some synchronic reason (e.g., because it is unlearnable due to the structure of Universal Grammar). (Haspelmath 2001, p. 20; emphasis added)

That the language could not arise, however, is nowhere defended within the paper. All that is defended is the claim that if a clitic system develops from free independent pronouns then that system cannot be one which falsifies the implicational universal. But it is not at all obvious that pronominal clitics must originate from free pronouns. For example, it is known that in some cases clitics have as their ancestors verbal inflectional affixes (Newmeyer 1998a, p. 265). Since many languages have verbal affixes indicating the person, gender and number of the verb's arguments, it is certainly possible to imagine a pronominal clitic system arising from an ancestral affixal system<sup>14</sup>, and there is no reason yet why such a clitic system should obey the targeted generalization.

Haspelmath 2001, however, should not be singled out for this oversight. For reason relating, again, to devising practicable research projects, writing accessible research papers and not boring theoretical audiences, the requirement that there appear a full review or citation of literature within historical linguistics supporting the strong historical claims entailed by a usage-based account can be, and is often, greatly relaxed.

The preceding are questions which, if gone unanswered in a proposed usage-based account, severely weaken its credibility and status as a fully developed explanation. In addition to these “essential questions,” there are two further issues which, though it would be wrong to fault a usage-based account for not addressing them, nevertheless should be everpresent in the theorist's mind, whose regular discussion would greatly benefit the “usage-based” functionalist literature and whose usual absence from that literature should dismay many.

**How Does Reanalysis Occur?** All usage-based accounts proceed from the observation, supported by numerous studies within historical linguistics, that what a speaker perceives often in their linguistic environment—whether a “stylistic” convention, a pragmatic strategy or a propensity for one property to be associated with another—can be reinterpreted over time as the result of an obligatory rule of the language. Thus, frequency and the concept that frequent associations can become obligatory associations play a most noticeable role in usage-based accounts. One is, in fact, loathe to dispute the importance of the influence which frequency of use has on the development of linguistic systems. This importance inspires the aphorism beginning this section as well as the following quote, taken from Newmeyer 1998a.

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<sup>14</sup>Whether there in fact are any is another matter worth discussion.

... [L]anguage changes at every level of grammar appear to be related in some way to the relative frequency of use of a particular word or grammatical element. (Newmeyer 1998a, p. 123)

This quote is particularly apt, because it emphasizes an ambiguity and an important distinction which is often obscured by scholars when discussing the importance of frequency in understanding language change. Taken in context, the quote is intended to express that there are, at every level of grammar, some forms of linguistic change which appear related to frequency of use, not that *all* changes may be understood in terms of frequency, nor that all the most robust statistical correlations have a related linguistic change or grammatical reflection. This subtlety is crucial, since the latter two claims are quite clearly false. One of the most common causes of language change is language contact and borrowing, which are processes only tangentially connected with frequency and functionality<sup>15</sup>. The falsity of the last claim requires a richer discussion.

The Boisian aphorism is that “grammars do best what speakers do most.” Under its most natural interpretation, however, this suggests that whatever speakers “do most” has the potential to reflect itself in a grammatical rule. It can be seen under further reflection, however, that this is terribly implausible, and the reasons why it is implausible actually raise very deep but approachable questions about our language faculty. The problems arise from the freedom with which a creative skeptic may identify statistical regularities in language use. Presented with a corpus, we may, with our little red pens, mark down occurrences of whatever ill-begotten *ad hoc* property we like, and we should certainly be able to find, after no diligent effort, a number of frequently occurring properties and combinations which are nonetheless either already known from typological research not to be grammatically relevant or else of such monstrous shape that their grammatical relevance would be shocking. The author has himself not carried out such an exercise, but he would like to suggest the following as plausible conjectures which could easily be followed up with corpus study and careful review of the typological literature.

- **Conjecture 1.** Parallel function relative clauses, in which the grammatical function of the gapped constituent is the same as that of the head which the clause modifies, appear far more frequently in corpora than non-parallel function relative clauses. This conjecture is inspired by the psycholinguistic evidence that speakers have great difficulty in

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<sup>15</sup>For a strong support of this point, see Newmeyer [?], section 3.4.

processing non-parallel function relative clauses (Kirby 1998). However, it is known that there are no languages allowing only parallel function relative clauses, nor are there any implicational universals governing parallel and non-parallel relatives (Kirby 1998).

- **Conjecture 2.** Proper names of locations appear much more frequently in oblique cases than in either nominative or accusative case. However, no language will be found in which the proper name of a location cannot appear as a subject or an object of the sentence.
- **Conjecture 3.** Verbs of consumption, such as “eat,” appear predominantly with inanimate objects denoting animal flesh, vegetable matter, digestible liquids and the like. However, no language will be found in which these verbs are permitted only to take inanimate objects.
- **Conjecture 4.** Raising verbs have local person subjects far more frequently than third person subjects. However, no language will be found in which *only* raising verbs are disallowed third person subjects.
- **Conjecture 5.** The main verbs of subordinate clauses are more likely to appear in either past or irrealis tense than present tense. However, no language will be found in which such verbs are prevented from having present tense.
- **Conjecture 6.** Despite the many statistical regularities it violates, every language will be found to have a grammatical equivalent of “Colorless green ideas sleep furiously.”
- **Conjecture 7.** The low central vowel /a/ will be overwhelmingly the most frequent vowel for any language. However, no language will be found in which /a/ is the *only* available vowel.
- **Conjecture 8.** In spontaneous conversation, the utterance of the affirmative anaphor (“yes,” in English) will be nearly always accompanied by a shake or a nod of the speaker’s head, even when visual contact with his audience is impossible. However, no community will be found in which the utterance of the affirmative anaphor is “infelicitous” without the accompanying action.

The final two conjectures are intended to provoke a dismissive response from the reader, especially if he is inclined towards functionalist analyses. “Of course,” this reader will reflexively respond, “we won’t find languages

like *that*. As anyone can see, the first language will be ruled out for reasons of expressivity, and the second one will be ruled out simply because languages never adopt as grammatical rules imperatives of that kind of cross-modal type.” *Pace* our fictional reader, it is not obvious that the language of conjecture 7 can be ruled out for reasons of expressivity. Why should such a language not compensate for its lack of vocal distinctions by use of a tone system or a richer repertoire of consonants? More to the point, however, both responses reveal a set of assumptions which the reader implicitly makes regarding whatever machination transforms statistical regularities into linguistic structure. This process, for which I propose the name “reanalysis,” is assumed, for example, to be sensitive to the expressive power of its products; it will not apply to a correlation which, if made into a grammatical rule, would severely hinder the expressivity of the language. Furthermore, reanalysis is assumed to be constrained by whatever conditions exist on linguistically possible rules. Notice that in order for this latter assumption to make sense, there must be restrictions on linguistic systems besides those arising purely from diachronic development and interests of communicative efficacy.

The upshot of the discussion thus far is that, contrary to the aphorism, there are *many* things which language users do rather often, but which grammar seems not to do at all. Reanalysis cannot be a funnel but a sieve. It is not an indiscriminate attacker, threatening all correlations equally. Moreover, the theorist cannot avoid speculation into the nature of this mechanism, and she will always tacitly assume in her analyses some restrictions on its operation. The obvious conclusion is that we should stop continuing in this habit of implicit assumption, and instead we should begin an explicit exploration of hypotheses. The conjectures above, if confirmed, must be explained, and their explanation will require a detailed, nuanced theory of what reanalysis is, how it occurs, and what constrains its operation.

In fact, conjectures of the kind above may prove to be an indispensable tool in our understanding of language change. Since the 1950’s it has been understood that what isn’t seen in language is just as important, perhaps more important, than what is seen. Knowing what sorts of reanalyses are impossible, what kinds of “soft” correlations *never* become “hard” obligations, provides us with information of inestimable worth concerning the nature of reanalysis, a pivotal character it seems in the biography of any language. Further, such absences may provide details into the functioning of the language acquisition device, in as much as they demonstrate that certain frequencies cannot be analyzed as evidence of a grammatical rule, and, consequently, they may raise and settle questions regarding UG. UG

has a particularly clear relevance to discussions of reanalysis, since one condition on reanalysis is that it produce rules consistent with UG. Indeed, the strongest hypothesis one could make is that reanalysis is constrained *only* by the principles of UG. This hypothesis is immediately falsified if there are any features of the language acquisition device which further winnow away the space of possible reanalyses. It is, however, a plausible conjecture—and tantalizingly unexplored.

To highlight a point briefly mentioned before, these considerations demonstrate that the functionalist community, including all those who propose usage-based accounts, cannot rest content with the simple statement that “correlations of high frequency are susceptible to reanalysis as a grammatical rule.” Given its universal reading, such a statement is obviously false. It must be recognized that nearly all usage-based accounts glance over an exceedingly rich open question, one which no one but functionalists have the greatest onus to investigate. Moreover, conjectures of the kind above are an additional argument that there exist properties of language which cannot receive a purely functional or statistical explanation. A functionalist account which assumes the non-existence of UG—assumes that there are no preëxisting constraints on linguistic rule systems as part of the genetic heritage of a language learner—and seeks to explain all language universals by way of statistical relationships resulting from non “domain-specific” properties of human speech, will be unable to incorporate the sorts of restrictions on reanalysis conjectured above. There must be some kind of “filter” on the “reanalysis engine,” rendering some statistical relationships accessible to it but not others. That this filter could be anything but specific to language is unlikely, given the character of the filtering which it effects. How could a filter which allows through the high correlation of person with grammatical function but not the high correlation of verbal tense and clausal subordination be part of some general purpose attentional mechanisms? In short, statistical relationships may help us to understand why we see some of the things we do see, but it cannot explain all that we do not see. For this, we would be wise to turn to Universal Grammar and special features of the language acquisition device.

The question of what constraints there are on reanalysis appears not to be widely discussed in the functionalist literature, and where it does appear there seems a strange reluctance to address it fully. Haspelmath 2001, for example, does not propose any model or cite any known theory of reanalysis. He does, however, make the following statement.

One possible objection to the frequency-based explanation... is that the frequencies of grammatical patterns may vary along countless dimensions: subordinate vs. main clause, past tense vs. present tense, singular vs. plural, specific lexical item, speakers' age and sex, spoken vs. written language, and so on. Why do we get a grammaticalization effect only with person and role categories, and not dependencies between various other factors?... My answer to this potential objection is twofold: On the one hand, I readily admit that frequency is not the only important factor determining grammatical structures, although I would insist that its importance is generally underestimated. On the other hand, I do believe that whenever there are strong frequency skewings in discourse, the possibility of conventionalizing these tendencies exists, and if we look hard enough, we may very well find everything that the usage-based explanation predicts. (Haspelmath 2001 p. 25).

The author, disappointingly, mentions only one other "important factor determining grammatical structures": analogy. Oddly, in his discussion of analogy, it is never explained how this device can account for the *absence* of certain reanalyses. In an apparent *non sequitur*, the author discusses only how some linguistic changes can be understood as the result of analogy. The uses which the author makes of analogy here are themselves rather dubious, the putative mechanism being something so poorly understood and so thinly characterized that it very nearly approaches being empty.

Moreover, the author's few examples of other "conventionalized frequency skewings" do not wholly evaporate the skepticism which he recognizes in the quote above. The author appears to underestimate the degree of freedom which one has in identifying the statistical tendencies of language users. Given the wide variety of dimensions for taxonomizing an utterance, the possibilities for identifying robust correlations seems limitless. The skeptical inquirer will doubt that the language learner is sensitive to *all* of these regularities, and he will want to know what separates the cows from the bulls.

In summary, the nature of reanalysis and the constraints placed on its operation ought to occupy a visible location in the functionalist literature, and there should be a richer discussion in that literature of its basic properties. In particular, usage-based accounts, which necessarily appeal to a mechanism of reanalysis, should either propose explicit hypotheses concerning it, or cite such hypotheses published elsewhere. As should be clear from the

tone of the preceding discussion, this has not yet been observed to happen in any of the published literature.

**What is the Exact Nature of the Construction at Present?** One of the more noticeable characteristics of the functionalist literature is its dearth of precise synchronic analyses. Few characteristics are more upsetting to the generative linguist, and few complaints are more mysterious to the functionalist than the generativist's insistence that synchronic analysis appear more explicitly in his work <sup>16</sup>.

Haspelmath 2001 is a vivid example of this trend in the functionalist literature. The paper takes as its object of study a puzzling restriction which often appears on the possible combinations of clitics. The author reviews a number of proposals in the generative literature which attempt purely structural, synchronic analyses of the restriction. The author justly discredits these proposals, and develops instead a usage-based diachronic account. The final analysis, however, contains no synchronic description of the languages subject to the restriction. The author's proposal would explain how this restriction could find its way into a language, as well as why other imaginable restrictions could not appear, but it leaves entirely unanswered the question of what the nature of the restriction in these languages actually is, how it arises from some rule or principle of the grammar. The generativist will complain that the deeper mystery, how the clitic system is arranged such that the ungrammatical combinations are impossible, has not at all been addressed by the functionalist account. *Something* about these grammars prevents the possibility of a certain group of clitic combinations. The functionalist account may tell us how that "something" can enter into a language through the process of reanalysis, but it does not yet tell us what that something is.

The functionalist's reaction to the generativist's complaint is often analogous to the reaction of the rebellious teenager to his father's admonitions that he work harder in school. "I'm not like you, dad!" shrieks the youngster, and the functionalist cries in concord "You generativists just don't get it. We've *rejected* your theoretical presuppositions!" Thus, one finds in

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<sup>16</sup>By a "synchronic analysis," I have in mind a precise specification of the grammatical structure of interest, one which is clear enough to make falsifiable predictions regarding at least what other structures are permissible within the language. Thus, the structure should be characterized as part of a system of symbolic relationships, though the full system may be left greatly underspecified and could incorporate principles grounded in language function. Optimally, such an analysis would appeal to other scholarly studies of the structure in question.

polemical works such as Dryer 1999 the following sorts of responses.

The view of theories as characterizing the innate human linguistic endowment is rejected by functionalists because it claims (or appears to claim) that general cognitive factors and communicative function play no role in explaining why languages are the way they are. And since under the normal functionalist view, there is no reason to posit innate human endowment that is specifically linguistic [sic], the goal of constructing a theory or metalanguage that characterizes this innate endowment makes no sense. (Dryer 1999, p. 4)

Taken in context, Dryer's claim is that generative linguists place such a heavy importance on synchronic analysis because from their perspective, the synchronic status of the linguistic item is the *only* factor determining its typological properties. For the functionalist, however, the typological properties of an item are a result of factors such as communicative efficiency, diachronic development and typical habits of use. Therefore, the construction of precise synchronic analyses are "irrelevant" to the kind of explanations which the functionalist seeks to develop. The generativist who disparages functionalist analyses on the grounds of being too vague about the synchronic status of their items of investigation is, at best, confusing the functionalist's line of research with his own, and at worst, simply harassing and arrogantly dismissing dissenting perspectives.

If Dryer's claim is being offered as a defense of the functionalist's habit of not including precise synchronic analyses, then a few criticisms in response are warranted. They appear ordered in increasing degree of tendentiousness.

First, there really doesn't appear any reason why an explicit synchronic model should be inconsistent with the stated theoretical assumptions of the functionalist. Sometimes it is believed that any synchronic analysis must presuppose some strong version of the "autonomy of syntax" hypothesis. This, of course, need not be the case; the symbolic system described by synchronic analysis may well consist of principles which can only be stated using properties and concepts endemic to "external" systems governing language use. In fact, synchronic analysis may reveal that the principles of the symbolic system can *only* be stated using these concepts, which is quite in line with the functionalist's general theoretical perspective. Moreover, I assume that everyone, functionalists and generativists alike, are in agreement that language is a "cognitive phenomenon," that it is an object of knowledge for the speaker. Similarly, I assume that everyone agrees that the task of the

linguist is not only to explain “why language looks the way it does,” but also to characterize the knowledge which a speaker has of their language, the mental state both necessary and sufficient for a human to speak the language. The idea that the latter task is an important one is not inconsistent with the notion that the former is best approached through considerations of language use and function, rather than putative innate endowments. One can believe whatever they like concerning why grammars are constrained in the ways observed without adopting the perverse perspective that hypotheses about the synchronic nature of a structure are “irrelevant” to the discipline of linguistics.

This is all to say that functionalists also recognize the importance of accurate synchronic analysis to the discipline of linguistics<sup>17</sup>. Now, given the functionalist’s recognition of the importance of synchronic analysis, he should ask himself “who is best equipped to provide this analysis?” Who, given the functionalist’s own assumptions, will be the source of accurate grammars for the world’s languages, accurate theories of cliticization, accurate analyses of tense projection in subordinate clauses? Although the generativists currently have a monopoly on this work, one of their defining assumptions is that typological generalizations have, by and large, grammar-internal origins. Thus, typological arguments have saturated their synchronic analyses in ways which, according to the functionalist, are entirely unjustified. The synchronic analyses proposed by generativists must, then, be fully incorrect, otherwise a large number of typological generalizations would be the result of innate constraints which, for the functionalist, simply do not exist. Therefore, the onus is on the functionalist now to provide his own synchronic analyses, to argue for these analyses on the basis of his own assumptions, and to incorporate them into his functionalist, usage-based explanations of typological universals. It is equally the duty of the functionalist to discover laws of argument linking, weak-crossover effects and binding as it is his duty to explain these laws as resulting from features of historical change and language use. Thus, the puzzlement one feels from the absence of synchronic analysis in the functionalist literature.

Secondly, contrary to the intended lesson of Dryer 1999’s illustrative examples of functionalist analyses, it is not at all obvious that synchronic

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<sup>17</sup>There are functionalists who claim to “reject the distinction between diachronic and synchronic linguistics.” All they seem to mean by this, however, is that language structure at any particular time is greatly influenced by language history, and so the two together should never be far from the theorist’s mind. Well and good; such parties still recognize the importance of accurate analysis of the speakers’ cognitive state taken as an individualistic property, which is all is intended here by the term “synchronic analysis.”

analyses are always irrelevant to the functionalist's explanations of typological generalizations. Indeed, Dryer at one point concedes that his own 1992 analysis of the Greenbergian word order correlations, which argues that the correlations are due to the interests of effective parsing, assumes a particular syntactic analysis of the English genitive construction. The few lines explaining why this doesn't contradict the position that synchronic analysis is irrelevant to the functionalist's claims are not clear (Dryer 1999, p. 7), but they seem to suggest that the author finds the assumed analysis to be so obvious and basic that explicitly mentioning it or arguing for it based on other synchronic properties of the language would be pointless. However, persons having a familiarity with the perversities of modern syntax might not find the kind of left-branching construction the author proposes to be so obvious, nor is one confident that it would be accepted by all syntacticians, nor is one even sure that the very notion of phrase structure is apodictic to all syntactic research. Needless to say, synchronic evidence which suggested a structure different from that assumed by the author would pose great problems for his purported typological explanation.

A similar case in point is highlighted in Johnson 2002. Many functionalist accounts adopt the Jespersenian claim that the infinitival element "to" in modern English is "a mere empty" marker, and this claim often forms an essential component of the analysis. However, the simple comment that "to" is an empty marker is far too vague to be considered a synchronic treatment of the infinitive in English. Furthermore, however one understands the property of being an "empty marker," recent synchronic work suggests that the infinitival marker may be a verb, and so a member of a rich lexical class. Thus, functionalist analyses based on the assumption that "to" has always been of a more "functional" or "grammatical" nature should address this research and establish that it is of no consequence to their analysis.

In short, the author would like to parrot Johnson 2002's plea that "before claims are made about diachronic developments, let's pay some attention to what the best synchronic analysis is at the various stages being examined." He would also like to offer the following analogy which, although not perfect, is nonetheless provocative: If we are seeking to understand how wings develop across animal species in order to explain constraints on their form<sup>18</sup>, then knowing in each case the present physiological structure of the wing may inform our account, and it would at least provide it with details which enrich the story considerably. Furthermore, there may be parts of the pro-

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<sup>18</sup>For example, the fact that no animal has a species of locomotion similar to that of the airplane.

posed developmental theory which suddenly become implausible once more biological and anatomical information is collected.

The final point will be convincing only to those who hold the strongest of “internalist” perspectives on linguistic science. Arguably, the purpose of doing typological work is to gain an understanding of the limits on human grammars. The typologist, whether he adopts functionalist or generativist assumptions, therefore ought to behave as a student of grammars. The language data, the utterances permissible within a language, are entities of only the most superficial concern to the typologist. Grammars are his primary objects of study, and his task is to investigate and explain the boundaries on their diversity. Thus, even the functionalist typologist is in the business of explaining why a particular rule or a particular structure evidences itself across languages. A typologist who does not provide a detailed description of that rule or structure, but only a story of how a rule or structure with a certain effect on the permissible utterances in the language is likely to arise, has not performed his stated intellectual duty. He has, to use the cliché, told the story of Hamlet while leaving out the prince.

It has not been argued that the proponent of a usage-based account has an onus to provide a synchronic analysis with his proffered account, only that functionalists should engage in more synchronic study. It should be clear that the claim made in Dryer 1999 that synchronic analyses are inconsistent with or irrelevant to functionalist research is not credible. Even if the functionalist’s theory of typological universals is correct, there remains the question of how the linguistic system is represented in the mind of its speakers, and research into this question must be unified with the functionalist’s typological discoveries. Since Dryer 1999 is correct in its claim that the generative linguist will, by and large, only seek synchronic explanations of typological data, the proponent of the usage-based account is in a better position to put forward a synchronic analysis that is, in his view, accurate and consistent with his work. Those attempting *purely* synchronic accounts of typological universals are no more going to seek a unification of the functionalist’s diachronic discoveries with their synchronic analyses than christian scholars are going to seek a reading of the Bible consistent with the Koran. Thus, it would be optimal if the question heading this section were explicitly addressed in any usage based account.

### 4.3 Conclusion

In this section, the basic logical structure of a usage-based account was introduced, as well as a number of questions which any such account must

address in order to be accorded the status of a full explanation. It should be noted that the deck has now been stacked so that any usage-based explanation fitting the criteria above will automatically fill the need which we were left with at the end of the last section, the need for an account of the person-based split-ergative system which could provide both a principled explanation of alignment, a precise synchronic analysis of the morphological system and clear typological predictions. Thus, the remainder of this paper will be committed to the search for such a usage-based account. The next section begins the search with an account which so nearly satisfies our demands, that this paper can be understood as offering merely an elaboration rather than a competitor of it.

## 5 Bias and Split-Ergativity in Dyirbal

Zeevat and Jäger 2001 offers an especially complete usage-based account of argument marking systems and person-based split-ergative systems in particular. This section presents the basics of their analysis as well as a few criticisms which motivate the original work that follows. The account can be divided, roughly, into three components, each of which corresponds to a characteristic feature of any usage-based account: (i) corpora analysis and statistical argumentation in support of “alignment” being due to basic habits of language use; (ii) a theory of how marking strategies arise as a response to this “alignment”; (iii) a theory of how, over time, these pragmatic strategies become entrenched as principles of the grammar.

### 5.1 The Character of Alignment

Usage-based accounts of Silversteinian alignment are primarily answers to the question “what is alignment and how does it come to affect syntax?” The aim of all such accounts is to explain the alignment of two categories “functionally,” to identify it as merely a statistical regularity for the two categories to appear together, emerging from more primitive habits of use. Zeevat and Jäger 2001 convincingly demonstrates that the alignment of grammatical role with person can be viewed in this way.

Briefly, the authors first describe the results of two corpora studies, one of English newspaper text and the other of a Swedish corpus consisting of spontaneous conversations. A number of interesting statistical relationships are found, including the high likelihood for local person pronouns to be subjects and third person pronouns to be objects. That this correlation

holds across two distantly related languages and two very different styles of discourse suggests that it is rather robust.

Furthermore, Zeevat and Jäger 2001 offers a precise statistical model which would explain this pattern as arising from more basic correlations and causal relationships. In contrast to studies such as Haspelmath 2001, the emergentist account of scalar alignment in Zeevat and Jäger 2001 is fleshed out concretely through the use of a Bayesian network. Such a network pictures statistical dependencies between features and causal assumptions regarding how the presence of some features influences that of others. The authors design a network that incorporates basic correlations drawn from their own corpus study. These correlations, discovered in the study, are believed to have a more transparent explanation in terms of the habits of speakers. Such a “basic correlation,” for example, would be the relationship between an NP being a topic and its being a subject. With the basic correlations one adds a set of “causal assumptions” governing how the various parameters influence one another <sup>19</sup>. The resulting model, the authors report, “gives the values . . . measured in the corpus within a couple of percentage points” (Zeevat and Jäger 2001, p. 9).

Zeevat and Jäger 2001 successfully argues that there exists a cross-linguistic statistical relationship between person and grammatical function, and that this relationship emerges from more basic patterns. Thus, their account answers the first two “necessary questions” raised in section 4.2. In particular, their answer to the question “how do the statistical patterns emerge from more basic ones,” should certainly be recognized as an advance in our understanding of alignment, even if the authors caution that they only “show that a causal model is possible . . . not that [they] have found the true explanation” (Zeevat and Jäger 2001, p. 10). The use of a Bayesian network to provide a precise model of “emergence” in this context may well constitute the first truly principled explanation of the directionality and selection of alignment.

## 5.2 Bias and Man’s Discovery of Marking

The cross-linguistic statistical relationships discovered by the authors present a problem for speakers of a language in which the distinction between subject and object is not clearly signaled. Zeevat and Jäger 2001 hypothesizes that speakers are sensitive to these statistical regularities and interpret ut-

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<sup>19</sup>Unfortunately, the authors are not explicit about what the “causal assumptions” in their model are, only noting that they are “not unreasonable, but remain to some extent arbitrary” (Zeevat and Jäger 2001, p. 9).

terances in a manner that is most consistent with them<sup>20</sup>. Therefore, unless some clear morphological or syntactic distinction is in place, any unlikely combination of person and grammatical role will be systematically misinterpreted. In the terminology of Zeevat and Jäger 2001, there exists a “bias” for local person pronouns to be interpreted as subjects and for third person pronouns to be interpreted as objects. It is this bias which inspires the development of pragmatic marking strategies and explains the form they take.

The authors formalize this general idea within the framework of Bidirectional Optimality Theory (Blutner 2001). Three “production” constraints are posited, representing the competing influences of interpretational bias, morphological faithfulness and economy of expression. The constraint BIAS prefers the normal reading of a sentence, where “normal” is intended as “most likely.” This constraint is violated by any form-meaning pair  $p$  in which the meaning coördinate is not the most often occurring interpretation of the form coördinate, that is, in which the grammatical role of a pronoun in the form coördinate of  $p$  is not that which its person feature would predict. For example, the pair ⟨“Kissed she I”, **kiss(she,I)**⟩ would violate BIAS because the form is interpreted as having a third person subject and a local person object, while the pair ⟨“Kissed she I”, **kiss(I,she)**⟩ would not violate the constraint.

If it were to rest undominated, BIAS would require all local person pronouns to be interpreted as subjects and third person pronouns to be interpreted as objects, resulting in an intolerable ineffability for the unlikely combinations. The effects of BIAS are therefore limited by the dominating constraint GENERATION. This constraint requires all meanings to faithfully reflect the morphology of their forms<sup>21</sup>. More concretely, GENERATION is violated by any form-meaning pair in which the meaning coördinate is not the result of a full semantic parse of the form coördinate. For example, the pair ⟨“Kissed she me”, **kiss(I,she)**⟩ would violate GENERATION. The meaning coördinate of this pair includes a local person subject, which is inconsistent with the presence of accusative morphology on the local person pronoun. However, the pair ⟨“Kissed she I”, **kiss(she,I)**⟩ would not violate GENERATION.

The third constraint, ECONOMY, enforces a minimum use of morphological structure. As will be shown shortly, it also serves to break the parity

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<sup>20</sup>This idea has been further elaborated in recent work by the first author, such as Zeevat 2002.

<sup>21</sup>A more apt name for this constraint, then, would possibly be FAITHFUL.

between a strategy of morphologically marking the most likely combinations of features and one of marking the least likely combinations. ECONOMY is an inherently bidirectional constraint. A form-meaning pair  $p$  incurs a violation of ECONOMY if there exists another pair  $p'$  with the same meaning coördinate as  $p$ , which performs as well on BIAS and GENERATION as  $p$ , but which has less morphological structure in its form coördinate. The effects of ECONOMY in conjunction with the other constraints will be made more clear in a moment.

Zeevat and Jäger 2001 proposes the following universal ranking for the three constraints above.

$$(12) \quad \text{GENERATION} \gg \text{BIAS} \gg \text{ECONOMY}$$

This ranking is combined with a unique variation of bidirectional optimality.

$$(13) \quad \begin{array}{l} \text{A form-meaning pair is optimal iff} \\ \text{(i) The meaning is optimal with regard to the form for the} \\ \text{constraint GENERATION} \\ \text{(ii) The form is optimal with regard to the meaning for the} \\ \text{constraint BIAS} \\ \text{(iii) The form is optimal with regard to the meaning for the} \\ \text{constraint ECONOMY} \end{array}$$

Finally, it is supposed that the language in question has available a pair of morphemes which communicate subjecthood and objecthood. These will be represented as ERG and ACC, respectively. The effect of the ranking above within the production system can now be observed in Tableau 4.

TABLEAU 4  
Bidirectional Tableau for Bias Driven Production System

	GENERATION	BIAS	ECONOMY
⇒ She I kiss: ⇒ kiss(I, she)			
She I kiss: kiss(she, I)		*	
She-ERG I kiss: kiss(I, she)	*		*
⇒ She-ERG I kiss: ⇒ kiss(she, I)		*	
She I-ACC kiss: kiss(I, she)	*		*
⇒ She I-ACC kiss: ⇒ kiss(she, I)		*	
She-ERG I-ACC kiss: kiss(I, she)	*		*
She-ERG I-ACC kiss: kiss(she, I)		*	*
She-ACC I kiss: kiss(I, she)			*
She I-ERG kiss: kiss(I, she)			*
⇒ She run ⇒ run(she)			
She-ERG run run(she)			*

The optimal form-meaning pairs are those marked with the ⇒. Because the reader may be unfamiliar with Bidirectional OT, a few words clarifying the tableau above are warranted. Suppose that the speaker intends to communicate “kiss(I,she)”, the likely combination of person features and grammatical role. He can only communicate this proposition with the form “She I kiss”. All other form-meaning pairs containing this proposition either violate GENERATION, because their form includes a morphological marking which is not interpreted in the paired meaning, or they violate ECONOMY, because their form employs more morphological marking than “She I kiss” but the pair performs equally well on GENERATION and BIAS. Similarly, suppose the speaker intends to communicate “kiss(she,I)”, the unlikely combination of person features and grammatical roles. This can only be communicated by the forms “She-ERG I kiss” or “She I-ACC kiss”. The form “She-ERG I-

ACC kiss” is saddled with gratuitous case marking. The pair containing it performs as well on BIAS and ECONOMY as the pairs containing the optimal forms, and so it violates ECONOMY because of the additional case marking of its form. The form “She I kiss” is ruled out by Bidirectional OT’s unique manner of considering the perspectives of both the hearer and the speaker. In brief, the pair ⟨“She I kiss”, **kiss(she,I)**⟩, is eliminated because the meaning **kiss(I,she)** is the more optimal interpretation of “She I kiss”, as an inspection of the first two rows of Tableau 4 reveals. Finally, suppose that the speaker wishes to communicate “run(he)”. The only form at his disposal is “She run”. The case-marked competitor fails on ECONOMY, since he performs as well on BIAS and GENERATION as the non-casemarked competitor. This follows from the natural assumption that with intransitive verbs the statistics governing combinations of grammatical role and person simply aren’t relevant for interpretation.

The formalization above provides precise answers to a number of questions regarding “pragmatic marking strategies.” The arrival of these strategies within a language is explained as a result of the influence of statistical bias on interpretation. Observe that if a language did not have a morphological marking system of the kind imagined, the only form available as an expression of the meaning “kiss(he, I)” would be “She I kiss.” However, the effects of bias combined with the principles of Bidirectional OT eliminate that form-meaning pair from the set of candidates. Thus, in a language in which the signaling of grammatical role has become impoverished, the unlikely combinations of grammatical role and person would simply be inexpressible. Moreover, this formalization offers a rather principled explanation of the question, raised in section 4.2, of why there don’t exist pragmatic marking strategies in which the *most likely* combinations of person and grammatical role are marked. As can be seen in Tableau 4, the candidate pairs ⟨“She-ACC I kiss”, **kiss(I,she)**⟩ and ⟨“She I-ERG kiss”, **kiss(I,she)**⟩ lose to the candidate ⟨“She I kiss”, **kiss(I,she)**⟩, since the latter performs just as well as the others on GENERATION and BIAS but employs less morphological structure. In short, strategies involving a marking of the most probable forms are ruled out because the effects of statistical bias on interpretation allow one to avoid marking these forms and yet be understood. Since speakers always opt for the path of least resistance, these strategies are never considered. Thus, when the above theory of pragmatic marking strategies is added with the assumption that all person-based split-ergative systems descend from such strategies, one predicts that the inverse of the system observed in Dyirbal should not be attested.

The reader may have already noticed one puzzling feature of the sys-

tem described above. The forms “She-ACC I kiss,” “She I-ERG kiss” and “She-ERG I-ACC kiss” are predicted to be impossible. However, all of these forms are attested in languages in which the pragmatic marking strategy has become entrenched into the grammatical system. A language like Japanese not only permits the form “She-ACC I kiss”, but requires it to convey the meaning “kiss(I,she)”. Similarly, Inuktitut requires “She I-ERG kiss” to convey that meaning, and Dyrirbal requires “She-ERG I-ACC kiss” to communicate “kiss(she,I).” How can these forms go from illicit to obligatory? This question is addressed by the final component of Zeevat and Jäger 2001, which attempts to show that the process of “reanalysis” can be understood partly through the effects of bias.

### 5.3 Grammaticalization and Bias

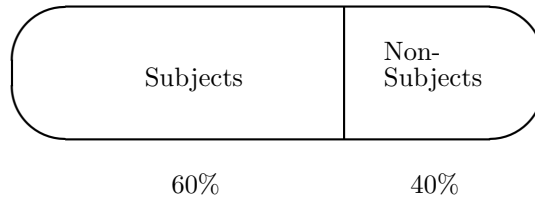
In a usage-based account of the person-based split-ergative languages, their case-marking systems are hypothesized to be descendents of the pragmatic marking strategy described above. In such languages, that marking strategy has become “grammaticalized,” converted into an obligatory rule system of the grammar. This would so far explain why the “inverses” of these case-marking systems are not attested, why no language marks only local person subjects and third person objects. The usage-based account, however, cannot stop here. It cannot simply rest content with some perfunctory hand-waving towards a “grammaticalization” process. The problem is that we have no understanding *how* these split-ergative systems arise from this pragmatic strategy. It cannot be a simple process of the marking strategy becoming “obligatory”, since, as has already been shown, that strategy actually *differs* from the split-ergative system in the markings which it permits. Clearly, as the pragmatic strategy becomes a grammatical system, changes take place. In order for the usage-based account to be complete, it must explain how those changes take place and why they *must* take place.

The analysis offered in Zeevat and Jäger 2001 addresses these questions by first considering why grammaticalization should ever occur in a language employing the pragmatic strategy. Why doesn’t the language simply stabilize here, and the pragmatic strategy remain a pragmatic strategy? Why would a learner ever interpret the language as including an obligatory case-marking rule? The authors suggest that interpretational bias may bear some of the responsibility. To understand how these obligatory systems could develop, it is best to consider first the changing effects of bias on the interpretation of local person pronominals.

Suppose that language  $L$  has reached a stage of development just prior

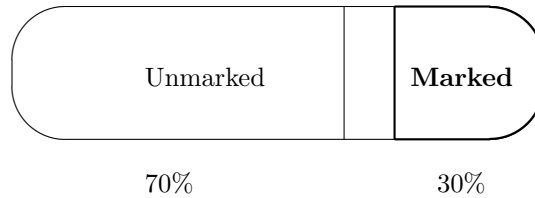
to the introduction of the pragmatic marking strategy. At this stage, the unmarked local person pronominals are subjects with a probability of roughly 60%.

FIGURE 1  
Unmarked Local Person Pronominals



In order to counteract the interpretational effects of this bias, the pragmatic marking strategy is introduced into *L* with the result that 30% of its local person pronominals become marked.

FIGURE 2  
Local Person Pronominals



The other 70% may remain unmarked for a number of reasons: they may be the subjects of their respective sentences, they may be accompanied by third person subjects, which induces the optionality noted above, or there may be counteracting statistical biases which prefer in these situations the least likely combination of person and role. Whatever is the case, this marking strategy has an effect on the interpretational bias. Since local person subjects are never marked in this strategy, all 30% of the marked pronouns are non-subjects. Thus, at this stage of *L*'s development, the unmarked local person pronominals are subjects with a probability of roughly 85%.

FIGURE 3  
Unmarked Local Person Pronominals



This is a drastic increase in the degree of bias favoring the interpretation of unmarked local pronouns as subjects. The marking of local person objects becomes more necessary as a result, increasing the percentage of marked local person objects and further decreasing the percentage of unmarked local person objects. Interpretational bias increasingly builds upon itself in this way, until the marking of local person objects becomes so widespread that it may be interpreted by the learner as an obligatory rule of the grammar. At this stage, *L* obligatorily marks all local person objects.

The story just told for local person pronominals works equally well for third person pronominals. Although bias may at first only require 30% of these pronouns to be appear with ergative marking, this marking in turn affects the interpretational bias, which then produces an increase in marking. The final result is a system in which all third person subjects must appear marked. Since *L* at this stage requires both third person subjects and local person objects to appear marked, one can understand why such forms as “She-ERG I-ACC kiss” are not only permitted but required in the grammaticalized systems. Therefore, by refusing to wave its hands at an unspecified process of “grammaticalization,” Zeevat and Jäger 2001 avoids a potential problem for its usage based account, and also provides a clearer picture of how such unusual marking systems as that found in Dyirbal can be claimed to arise from pragmatic strategies grounded in universal habits of language use.

#### 5.4 Criticisms of the Bias-Based Usage-Based Account

Zeevat and Jäger 2001 goes much further than most usage-based accounts in presenting a clear, precise analysis of Silversteinian scale alignment and an explanation of the typological data supporting it. In contrast to Aissen 1999, its theory of alignment provides a principled account of why certain scales align together but not others, and why alignment takes the direction

that it does. In contrast to many other functionalist analyses, it addresses a significant number of the questions put before potential usage-based accounts in section 4.2. To the question of “What are the statistics, and how robust are they,” the account provides two original corpus studies which confirm both that person is a good predictor of grammatical function and that this relationship is rather robust. The question “How do the statistical patterns emerge from more basic ones” is answered by the features of the Bayesian network the authors propose. The Bidirectional OT production system provides strong insight into the matter of “How do the pragmatic strategies arise,” and the bias-based theory of grammaticalization addresses the question “How Does Reanalysis Occur?”

In spite of these advantages, there are a few points on which this proposal may be criticized. The first concerns the question raised in 4.2 of “What are the possible ways these grammatical systems can develop?” In order for the usage-based account in Zeevat and Jäger 2001 to be correct, it must be that the only ancestral source for person-based split-ergative systems is the kind of pragmatic marking strategy envisioned by the authors. However, that this is the case is not argued in the work, and there in fact exist other hypotheses as to the origins of these systems (Garrett 1990). Furthermore, Zeevat and Jäger 2001 clearly intends this usage-based account to extend to full nominative-accusative and ergative-absolutive systems (Zeevat and Jäger 2001, p. 13). As a general theory of argument marking, however, the account only becomes more tenuous, since it greatly strengthens its diachronic commitments to include that the full accusative and ergative systems also claim as their unique diachronic source this pragmatic marking strategy. Moreover, although it is clear how this strategy becomes grammaticalized into the person-based split-ergative system, it is unclear what must occur for it to give birth to these fuller systems.

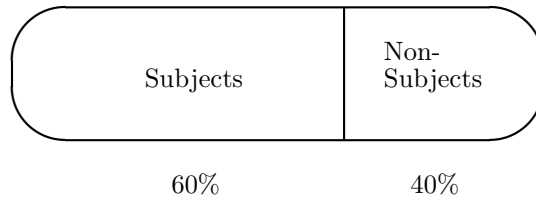
The second point of criticism is simply that Zeevat and Jäger 2001 lacks a synchronic analysis of the split-ergative system. This is not a very strong point, however, since it has already been conceded that including a synchronic analysis is not essential to the success of a usage-based account. Furthermore, Zeevat and Jäger 2001 is clearly a shorter work devoted primarily to exploring the ways in which interpretational bias can play an explanatory role in usage-based accounts; a synchronic analysis would well be outside the scope of the paper. Nonetheless, one would prefer an account in which a synchronic analysis is incorporated in a direct and natural way.

The last point is the most serious and concerns the bias-based theory of grammaticalization. On the one hand, the account is not as explicit as one might like, since the most mysterious and important stage of the process, the

stage at which the statistical relationship becomes reinterpreted as a rule, is left uncharacterized. By leaving the mechanism of reanalysis unstated, the account invites a number of questions. For example, the rule that the learner hypothesizes is responsible for the widespread subject-marking in  $L$  cannot be one such as “all third person subjects have marking.” This would incorrectly predict that the mature  $L$  has subject marking even with intransitives. Why, then, does the learner of  $L$  not consider this rule? Statistics alone? *No* language specially case-marks the subjects of transitive sentences. Thus, it seems that a rule of this kind is *never* considered by the learner, which prompts the question “why?”

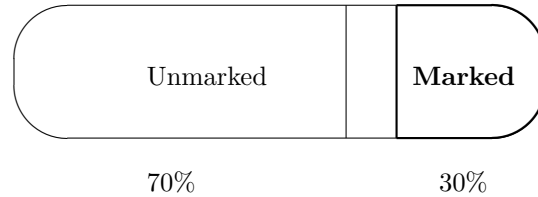
On the other hand, however, the proposed explanation of how the case marking becomes widespread enough to be interpreted as the result of an obligatory rule does not seem to go through. Let us return to the developmental stage of our language  $L$  just prior to the introduction of the pragmatic marking strategy. At this stage, unmarked local person pronominals have a 60% chance of being subjects.

FIGURE 1  
Unmarked Local Person Pronominals



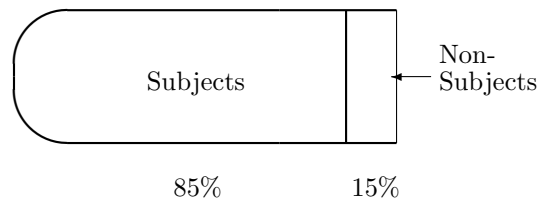
Interpretational bias renders most propositions involving local person objects inexpressible, and thus the pragmatic marking strategy is introduced, with the result that 30% of the local person pronominals become marked.

FIGURE 2  
Local Person Pronominals



Now, recall that the remaining unmarked 70% of these pronouns do not form a homogeneous class. 85% of the remainder are the subjects of their respective sentences. But what of the other 15%?

FIGURE 3  
Unmarked Local Person Pronominals



These are objects that interpretational bias does not demand be marked. Despite their generally rare combination of person and grammatical role, bias in these cases favors the local person pronouns being interpreted as objects. Cases such as this are mentioned several times in Zeevat and Jäger 2001.

The constraint BIAS is not just taking in regularities about the kinds of NP's that are subject and object, but also other kinds of preferences. E.g. an inanimate and a human NP as arguments of the verb "to please" will almost certainly have the inanimate NP as the subject and the human NP as the object and would not have to be marked since the two effects of BIAS obliterate each other. (Zeevat and Jäger 2001, p. 11)

...[U]nmarked NP's become more probable subjects the more often objects are marked, *unless there are opposite biases or fac-*

*tors that guarantee the proper readings are reached all the same.*  
(Zeevat and Jäger 2001, p. 14; emphasis added).

The latter quote is particularly apt and reveals a lacuna in the argument put forth in the last section. The next step in that argument was that the increased probability of unmarked local person pronouns being subjects results at this stage in an increased *need* for argument marking. However, recall that the unmarked local person objects at this stage are, like those appearing with the verb “please,” ones for which BIAS favored their intended interpretation; otherwise, BIAS would have required that these pronouns appear with object-marking. Presumably, the preference of BIAS for these unmarked local pronouns to be interpreted as objects is due to the semantic or pragmatic features of the sentences in which they appear. Therefore, the mere introduction of object-marking into *L* should not affect the preferences of BIAS in these cases; the simple enrichment of the language’s morphology should not affect the probability with which local person pronouns are the objects of “please.” Thus, at this stage as well, BIAS prefers for these local person pronouns to be interpreted as objects. One concludes that, contrary to the argument of the last section, there is no increased need for object marking at this stage. The minority of unmarked local person objects exists because BIAS favors their interpretation as objects; thus, marking is not needed for them to be interpreted as objects. In fact, in each of these cases, ECONOMY *prevents* the appearance of object marking on the pronouns; thus, there is no subsequent increase in the appearance of object marking in *L*. There is no cascade towards widespread marking. There is no descent into grammaticalization. The language stabilizes here, and the pragmatic marking strategy, it seems, remains just a strategy.

## 5.5 Conclusion

The criticisms offered above should, of course, not distract one from the real strengths of Zeevat and Jäger 2001. Its probabilistic analysis of alignment and bias-based theory of pragmatic marking strategies will be assumed, without alteration, throughout the rest of this paper. What is needed, however, to make the story complete is an argument that person-based split-ergative systems arise only from these bias-based marking strategies, and an explanation of how the pragmatic marking strategies come to be grammaticalized in the form in which they do.

As regards the diachronic argument, the author shall not attempt in this paper to support the hypothesis that person-based split-ergative systems

descend solely from pragmatic marking strategies. This matter is beyond his present expertise and would in the hands of a qualified scholar likely require a paper of its own. However, the scope of the bias-based account shall be limited purely to the person-based split-ergative system. No additional hypotheses shall be made regarding the origins of the full accusative and ergative systems, and so the overall diachronic commitments of the account will be lessened. By the standards proposed in section 4.2, this paper should therefore be considered incomplete. In that regard, it is no worse than all other usage-based accounts, but, unfortunately, also no better.

The remainder of this paper will therefore be devoted to the problem of how the pragmatic marking strategy described in Tableau 4 can become the split-ergative system described in Table II. In light of the above criticisms of Zeevat and Jäger 2001, the first question to address is “how does argument marking come to be so widespread that it is interpreted as the result of an obligatory rule?” The answer to be proposed employs the framework of Stochastic Optimality Theory (Boersma 1998, 2000, Boersma and Hayes 2001, Boersma and Levelt 1999, Bresnan et al. 2001); therefore, the next section will explain the basic principles of that system. The completed analysis will not only answer the question of how pragmatic argument marking becomes “grammaticalized,” but will also provide a synchronic analysis of the person-based split-ergative system and a theory of reanalysis which ties its constraints to the factorial typology of OT.

## 6 Stochastic Optimality Theory

Theorists have seldom been troubled by the existence of “probabilistic” or “gradient” data in linguistic studies. Forms which are acceptable but highly marginal are a familiar feature of linguistic research, as are forms which, although categorically rejected by informants, can appear in spoken language corpora, albeit very seldomly. These data don’t trouble the theorist because they are typically not on his research agenda. These data are certainly more “production-y” in appearance, and when they aren’t outright labeled production phenomena, there is only some vague handwaving towards a metric of ungrammaticality based on the total structural principles violated. In some cases, however, it is argued that these grades of judgement and probabilities of use are properly viewed as data revealing features of language competence rather than performance. On such views, probabilistic data should be explained by generative competence models. Most mainstream theories of competence, however, describe it as having a dis-

crete, purely combinatorial character, one which prevents the incorporation of “fuzzy,” probabilistic data. Stochastic Optimality Theory (Boersma 1998, 2000, Boersma and Hayes 2001, Boersma and Levelt 1999, Bresnan et al. 2001) is one of a number of frameworks designed to satisfy this doctrinal void, to provide a model of competence in which probabilistic data have a natural status.

Stochastic OT differs from standard Optimality Theory in two characteristic ways.

- **Absolute, Not Relative Ranking** In standard OT, constraints are simply arranged in a primitive partial order  $\gg$ . In Stochastic OT, constraints are assigned absolute ranking values on the continuous scale of real numbers. Constraints with higher ranking values are taken to outrank constraints with lower values. This induces the partial order  $\gg$ , but also provides a unique metric of “ranking distance” between the constraints. Constraints, even when immediately adjacent in the ordering, are specified distances apart, and the size of these distances is essential for the evaluation of outputs.
- **Stochastic, Not Noiseless Evaluation** In standard OT, the ranking of the constraints never alters; on every occasion that the output for a given input  $\alpha$  is computed, the system uses the same constraint ranking. In Stochastic OT, whenever the system is employed to compute an output for  $\alpha$ <sup>22</sup>, the absolute ranking of each constraint is slightly perturbed away from its assigned ranking. This is done by adding to the constraint’s assigned ranking value some real number drawn randomly from a normal distribution. For example, a constraint assigned the rank of 90 may at a particular evaluation be assigned the rank 89.3 or 92.1. This ephemeral ranking is referred to as the “effective rank” of the constraint, and the assigned absolute ranking can be viewed as the mean of the constraint’s effective ranks<sup>23</sup>.

Although the introduction of “absolute rankings” into the system may raise the skeptic’s eyebrow, in Stochastic OT these rankings are simply a means for implementing the concept of “stochastic evaluation,” the real showpiece

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<sup>22</sup>In standard descriptions of the theory, these “moments of evaluation” are understood as analogous to the points at which the grammatical system is accessed by production mechanisms during speech.

<sup>23</sup>Boersma and Hayes 2001 uses the term “selection point” for the effective rank of the constraint and “ranking value” for what is here loosely called the “absolute ranking” or “ranking value.”

of the Stochastic OT system. Stochastic evaluation allows the system to produce outputs in a variable, probabilistic manner, thus providing the theorist with a means to model probabilistic and gradient linguistic data. Suppose there exist two conflicting constraints  $C_1$  and  $C_2$ .  $C_1$  is given a ranking of 92 while  $C_2$  is ranked at 90. When the constraint system is employed to compute an output form,  $C_1$  will typically outrank  $C_2$ . However, there will also be output evaluations in which the effective ranking of  $C_2$  is perturbed sufficiently upward and that of  $C_1$  is perturbed sufficiently downward that  $C_2$  outranks  $C_1$ . Because  $C_1$  and  $C_2$  conflict, these latter evaluations will result in a different output form than is typically observed in the language.

The degree of output variation in the language is determined by the ranking distance between  $C_1$  and  $C_2$  and the degree to which their assigned rankings are perturbed during evaluation. By varying these parameters, one can produce languages with differing degrees of variation. As the difference between the assigned rankings of  $C_1$  and  $C_2$  increases, the number of ranking reversals occurring during evaluation quickly becomes rare. A total absence of output variation can typically be achieved by assigning the conflicting constraints absolute rankings of more than 10 units difference<sup>24</sup>. Stochastic OT can then be justifiably described as a generalization of standard OT. A standard OT system arises as the special case in which either all constraints are assigned rankings at least 10 units apart from one another or the amount of perturbation during evaluation is 0.

As a brief illustration of these ideas, suppose there exists a language in which syllable final consonants are, for the same input, sometimes deleted and sometimes preserved. The phonological and phonetic environments of the input may not be good predictors of the deletion, and there may be no other reasonable choice but to suppose that it is a purely free variation in the language. The data reveal that the underlying form “pat” surfaces as /pat/ with a probability of about 92% and as /pa/ with a probability of 8%<sup>25</sup>. Such a language can be modeled by a Stochastic OT grammar in which the constraint PARSE is assigned a ranking of 92 and NOCODA a ranking of 90.

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<sup>24</sup>A word of qualification is in order here. Because the normal distribution of effective ranks for a constraint only approaches a probability of zero, there is still a possibility of ranking reversal between two constraints with absolute rankings 10 units apart. However, at this distance the probability of such a reversal is one in 10 billion (Boersma and Hayes 2001, 2.3), rendering variation, for all intents and purposes, nonexistent.

<sup>25</sup>Boersma and Hayes 2001, 4.4 discusses a few real cases of such free variation in which one variant is much more common than the others.

FIGURE 4  
Stochastic OT Syllable Grammar

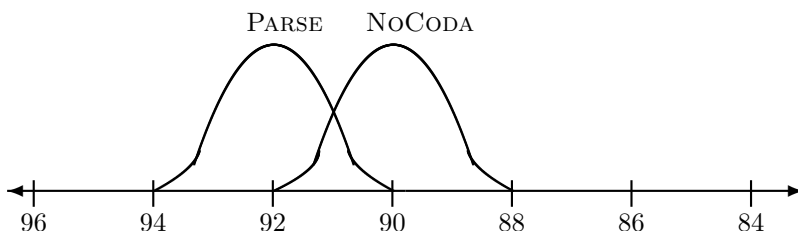
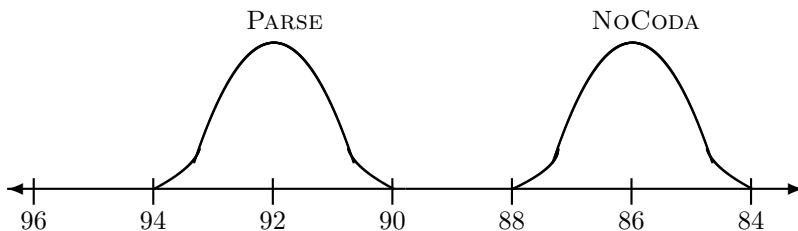


Figure 4 represents the distribution of rankings for these constraints. In the majority of evaluations, PARSE will dominate NOCODA. Thus, in the modeled language, the typical output for “pat” will be /pat/. However, there also exist evaluations in which NOCODA comes to dominate PARSE. In these evaluations, the output for “pat” will be /pa/. If the standard deviation for the distribution of effective ranks is 1 unit<sup>26</sup>, then the resulting distribution of outputs for “pat” is about 92% /pat/ and 8% /pa/<sup>27</sup>.

The modelling of a phonology such as that of English, in which there is no random variation between coda deletion and coda faithfulness, is straightforward.

FIGURE 5  
Stochastic OT Syllable Grammar



If PARSE is given a ranking of 92, NOCODA a ranking of 86 and the stan-

<sup>26</sup>The standard deviation for the distribution of effective ranks is the same for all constraints in the system. It is often referred to as the “evaluation noise,” a term intended to suggest that the value is a property of the evaluation process, rather than the individual constraints (Boersma and Hayes 2001).

<sup>27</sup>This result was confirmed through use of the Praat computer program (Boersma and Weenick 2000).

dard deviation is kept at 1 unit, then the outputs for this language will be exclusively coda-preserving <sup>28</sup>.

Besides providing a model of competence in which output forms may vary freely, Stochastic OT comes equipped with a theory of how learners can acquire knowledge of these free variations. The Gradual Learning Algorithm (h.f. “GLA”) of Boersma 1998, 2000, Boersma and Hayes 2001, Boersma and Levelt 1999 is a mechanical method for generating a Stochastic OT grammar which produces a targeted output distribution. Beginning from an initial state in which all constraints have the same absolute ranking <sup>29</sup>, the GLA is fed linguistic data consisting of input-output pairs. After approximately 10,000 such pairs, the algorithm converges onto a constraint ranking which produces exactly the probabilistic distribution of outputs as found in the data. If fed data from a language in which coda deletion occurs at a rate of 98%, the GLA produces a grammar which, though it may differ from the target grammar in the values of its absolute rankings, also produces coda deletion in 98% of the cases.

The preceding is all that must be known about Stochastic OT in order for the analysis which follows to be understood. As will be seen, the GLA is an indispensable catalyst in this analysis, triggering the grammaticalization of certain habits of speech. It is through the mediation of this algorithm that argument marking becomes widespread enough to be “reanalyzed” as a rule of the grammar, and the moment of reanalysis itself is, because of the character of the GLA, no different in kind from the individual steps leading to more widespread use of marking.

## 7 From Soft Constraint to Hard Constraint: The Analysis of Dyrirbal

### 7.1 Randomness and the Rule

There is something fishy about Stochastic OT and its GLA. One might at first be suspicious of the idea that there exists purely “free” variation in language. The use of Stochastic OT to model a distribution certainly would resemble a confession that one simply doesn’t understand the phenomenon. Let us accept, however, that some variations are not cognitively rule-governed <sup>30</sup>. The GLA has the surprising property that rule-governed

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<sup>28</sup>This result was also confirmed using Praat.

<sup>29</sup>Alternatively, the initial state could be one in which the set of markedness constraints dominates the set of faithfulness constraints.

<sup>30</sup>Boersma and Hayes 2001, 3.6 can help to alleviate one’s skepticism on this point.

variations may be misinterpreted as purely random, free variations. We should assume that the information to which the GLA has access consists solely of the set of universal constraints and the input-output data distributions. The GLA is thus “blind” to relationships between output forms and non-linguistic properties not employed in its constraint set. Not noticing these relationships, the GLA will yield a grammar which produces purely random output distributions that happen to match the probabilities fed into it, though those probabilities were the result of a systematic rule.

To make this point more concrete, suppose that there exists a community in which, for some reason, there is a convention that people only delete codas before quarter of the hour. The variation between outputs that are coda-preserving and coda-deleting is not purely random, but is systematically related to the conventional measure of time. Therefore, a language learner employing the GLA with a constraint set consisting only of NOCODA and PARSE will find themselves in a difficult situation. Their GLA is simply fed input-output pairs reflecting the 75% probability of the language preserving codas. Given this data, the GLA will mechanically search for a ranking of NOCODA and PARSE in conformity with the output probabilities. The ranking it converges onto produces, in conflict with the linguistic conventions of the learner’s community, a *random* distribution of coda-deletions. 25% of the learner’s codas are deleted, but the resulting Stochastic OT grammar can cause them to be deleted at 6:30 PM.

This may or may not strike one as a serious concern for the GLA. There are various equally “hand-wavy” solutions one could propose here: perhaps the algorithm has access to constraints like NOCODABEFOREFIFTEEN, perhaps the operation of the GLA can be halted when more “general purpose” learning algorithms detect the presence of extra-linguistic conventions, perhaps the scenario is too fanciful to be of real interest. Whatever one believes the consequences of this thought experiment are to the viability of Stochastic OT and the GLA, it can actually be used to the advantage of a usage-based account of argument marking and reveals one possible route from pragmatic marking strategy to widespread grammatical rule.

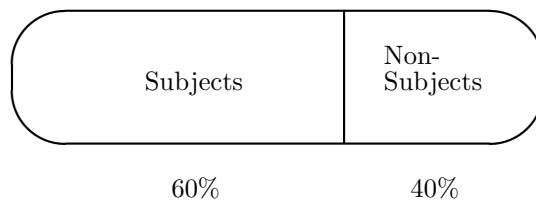
## 7.2 A Strategy Becomes A System

Let us suppose that Universal Grammar contains the following OT constraints governing the expression of morphological structure. \*STRUC, as before, penalizes the morphological expression of any case or person features. REAL-ERG/LOCAL is a constraint requiring the morphological realization of the ergativity and local person features, where “ergativity” is

a property holding of an NP whenever it is the agent of a transitive verb. This constraint is violated by any candidate in which a local person subject of a transitive verb appears either without morphology indicating person or without morphology indicating ergativity<sup>31</sup>. Similarly, REAL-ACC/LOCAL is a constraint requiring the morphological realization of the accusativity and local person features, where “accusativity” is a property holding of an NP whenever it is the object of a transitive verb. This constraint is violated by a candidate in which a local person object of a transitive verb appears either without morphology indicating person or without morphology indicating accusativity. The constraints REAL-ERG/3RD and REAL-ACC/3RD are the same as those above, except that they enforce an expression of the third person feature rather than the local person.

Constraints such as REAL-ERG/3RD are clearly impotent in languages lacking a system of case-marking. However, if a community of speakers adopts the pragmatic marking strategy of Tableaux 4, the GLA can force these constraints to play a more active role in the grammar. Suppose that language *L* is at the stage just before the adoption of the pragmatic marking strategy. 60% of local person pronouns are subjects and 60% of third person pronouns are objects.

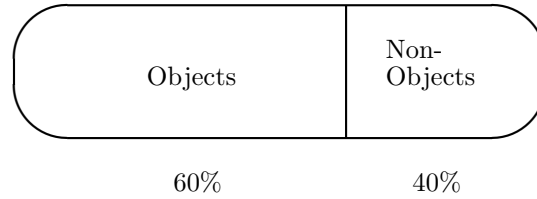
FIGURE 1  
Unmarked Local Person Pronominals




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<sup>31</sup>One could, perhaps, consider REAL-ERG/LOCAL as the result of Local Conjunction operating on two constraints REAL-ERG and REAL-LOCAL. However, since the analysis to be offered does not require the existence of these latter two constraints, it would be preferable to reduce the technical machinery used and introduce REAL-ERG/LOCAL as a primitive constraint.

FIGURE 6  
Unmarked Third Person Pronominals



By assumption, *L* at this stage lacks a system of case-marking. Thus, the constraint \*STRUC must strictly dominate all of REAL-ERG/LOCAL, REAL-ERG/3RD, REAL-ACC/LOCAL and REAL-ACC/3RD. Such a constraint ranking can be found in a Stochastic OT grammar which assigns \*STRUC the ranking of 100 and the other constraints rankings of 80.

*L* now enters a stage in which the pragmatic marking strategy is used to communicate the unlikely combinations of person and grammatical role. Subsequently, 30% of local person pronouns and 30% of third person pronouns in *L* become marked.

FIGURE 2  
Local Person Pronominals

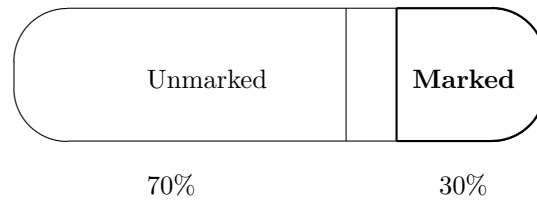
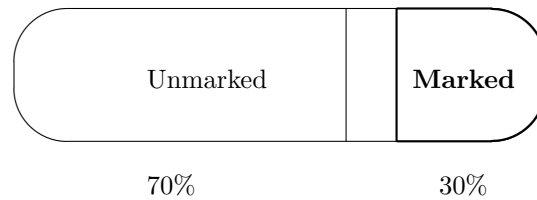


FIGURE 7  
Third Person Pronominals



Recall, however, that all the newly marked local person pronouns are objects<sup>32</sup>. Therefore, one calculates that at this stage, of the local person pronouns which are objects, 75% are marked. Similarly, 75% of third person pronoun subjects are marked.

FIGURE 8  
Local Person Pronoun Objects

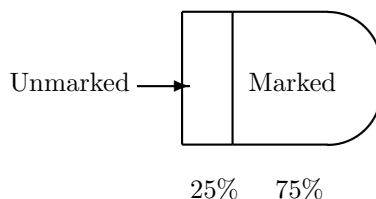
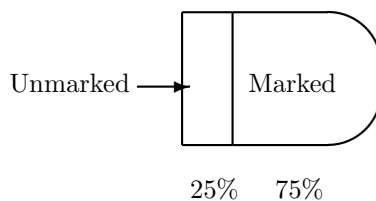


FIGURE 9  
Third Person Pronoun Subjects



Consider a language learner equipped with the GLA who is growing up in the linguistic environment described by Figures 8 and 9. Consider the perspective of his nascent parser. Because the presence of the subject marker ERG coincides regularly with the pronoun carrying both ergative and third person features, the parser analyzes this marker as an agglutinative morpheme expressing both ergativity and third personhood<sup>33</sup>. Similarly, the parser interprets the object marker ACC as an agglutinative morpheme

<sup>32</sup>For ease of calculation, it is assumed that all non-subjects are objects and that all non-objects are subjects.

<sup>33</sup>This behavior of the parser would need to be justified by a separate theory of parsing in language acquisition. However, it would follow naturally from the automaticity of the parser and a default assumption that all affixes it encounters are part of the morphosyntactic system.

expressing accusativity and local personhood. Now, the input data provided to the learner by the parser reveals that for inputs containing third person subjects of transitives, the morphological realization of person and ergativity occurs with a frequency of 75%, and that for inputs containing local person objects, the morphological realization of person and accusativity occurs with the same frequency.

One may suppose that the operation of the GLA is automatic, immediately triggered by learning data emanating from the parser. Given these output distributions, then, the learner’s GLA goes to work, converging onto a Stochastic OT grammar which produces this same distribution of marking. Because the ergativity and third person features are sometimes morphologically realized, the learner’s grammar can no longer rank \*STRUC so that it strictly dominates REAL-ERG/3RD. Similarly, \*STRUC cannot strictly dominate REAL-ACC/LOCAL in the learner’s grammar. However, since marking never appears on local person subjects or third person objects, the constraints REAL-ERG/LOCAL and REAL-ACC/3RD must still be strictly dominated by \*STRUC. Some experimentation reveals that a Stochastic OT grammar in which REAL-ERG/3RD and REAL-ACC/LOCAL are assigned a ranking of 99, \*STRUC a ranking of 97.3, and REAL-ERG/LOCAL REAL-ACC/3RD rankings of 80 produces the desired distribution <sup>34</sup>.

Although the grammar onto which the GLA converges does mark 75% of its local person objects with ACC and 75% of its third person subjects with ERG, the language it generates is distinctly different from the language of the learner’s community. The distribution of argument marking produced by the learner’s Stochastic OT grammar is just that—stochastic. The learner produces a 75% distribution of ergative marking across *all* third person pronoun subjects. One may presume, however, that the more “general purpose” learning mechanisms responsible for the acquisition of such things as style and register still acquire the pragmatic marking strategy of Tableau 4 as method for increasing the expressivity of the language. The result is that the learner uses the ERG marking in all cases in which the pragmatic marking strategy would require its use, but also in 75% of the cases in which the bias-based strategy does not require the use of marking. The learner thus marks an additional 18.75% of third person subjects with the ERG marking <sup>35</sup>.

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<sup>34</sup>This result was confirmed using the Praat program.

<sup>35</sup>That is, one considers the 25% of third person subjects not marked by the pragmatic strategy as a subset of all third person subjects. The free distribution of a 75% use of marking across *all* third person subjects entails that 75% of this 25%-subset has marking. 75% of 25% is 18.75%.

FIGURE 10  
Third Person Pronoun Subject Marking Enforced  
by Pragmatic Marking Strategy

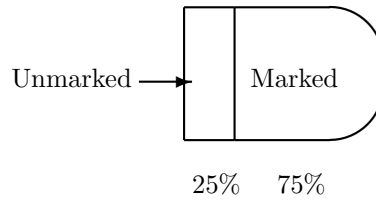


FIGURE 11  
Third Person Pronoun Subject Marking Enforced  
by Stochastic Grammar

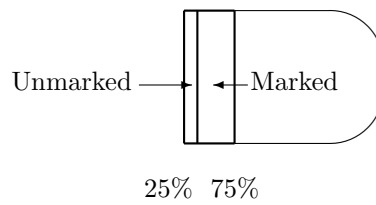
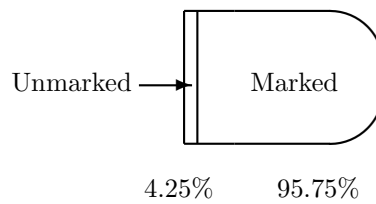
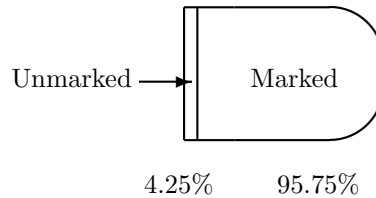


FIGURE 12  
Third Person Pronoun Subject Marking  
of the Resulting Language



The same is, of course, true for marked local person objects: in the learner's language, they appear with a frequency of 95.75%.

FIGURE 13  
Local Person Pronoun Object Marking  
of the Resulting Language



One can imagine that an entire generation of learners develops in this manner, changing the appearance of  $L$  so that it enters a stage in which 95.75% of its third person subjects and local person objects are marked. The process now iterates, as the next generation of  $L$ -learners are presented data in which subject marking occurs at a rate of nearly 96%. This generation may also be aware of the existence and necessity of the pragmatic marking strategy, with the result that, again, all 75% of third person subjects which the marking strategy requires be marked are marked by the learners. The learners' acquired Stochastic OT grammars, moreover, mark 96% of the remaining 25% of third person subjects, yielding a language in which a full 99% of third person subjects are marked with ERG. Similarly, the marking of local person objects with ACC occurs at a rate of 99% in the new generation's language. As this generation of  $L$ -learners matures, the appearance of  $L$  changes significantly; argument marking seeps further into  $L$ , always its residue an indelible stain. This tumult towards obligatory marking does not abate in the succeeding generations, and  $L$  soon has entered into a state in which all 100% of its local person objects and third person subjects are marked <sup>36</sup>. The Stochastic OT grammar representing  $L$  now features a strict, "discrete" ranking of constraints, which may be represented in the classical manner as follows:

$$(14) \quad \{\text{REAL-ERG/3RD, REAL-ACC/LOCAL}\} \gg *STRUC \gg \{\text{REAL-ERG/LOCAL, REAL-ACC/3RD}\}$$

The ranking in 14 describes a language interestingly similar to one utiliz-

<sup>36</sup>Simulations on the Praat program reveal that four generations is always sufficient for the marking strategy to be fully grammaticalized.

ing a person-based split-ergative system. As the following tableau illustrates, if the subject of a transitive verb is third person, it must appear with an ergative/local person marking, and if the object of a transitive verb is a local person, it must appear with an accusative/third person marking.

TABLEAU 5  
Third Person Subject/Local Person Object

V(Agt/3, Pat/1)	REAL-ERG/3RD	REAL-ACC/LOCAL	*STRUC	REAL-ERG/LOCAL	REAL-ACC/3RD
Subj/3-ERG, Obj/1-∅		*!	*		
Subj/3-∅, Obj/1-ACC	*!		*		
Subj/3-∅, Obj/1-∅	*	*!			
⇒ Subj/3-ERG, Obj/1-ACC			**		

Furthermore, if the subject of a transitive verb is a local person or the object a third person, marking of the argument cannot appear.

TABLEAU 6  
Local Person Object/Third Person Subject

V(Agt/1, Pat/3)	REAL-ERG/3RD	REAL-ACC/LOCAL	*STRUC	REAL-ERG/LOCAL	REAL-ACC/3RD
Subj/1-ERG, Obj/3-∅			*!		*
Subj/1-∅, Obj/3-ACC			*!	*	
⇒ Subj/1-∅, Obj/3-∅				*	*
Subj/1-ERG, Obj/3-ACC			**!		

In Tableau 6, the constraints REAL-ERG/3RD and REAL-ACC/LOCAL are not violated by any candidate because in no candidate are the features “ergative” and “third” co-occurring. Thus, the competition between candidates is decided entirely by the constraint \*STRUC. Moreover, by a careful definition of the constraints, this OT system avoids one of the problematic consequences of the approach in Aissen 1999. Third person subjects of intransitive verbs are correctly predicted not to bear ERG marking.

TABLEAU 7  
Third Person Subject of Intransitive Verb

V(Agt/3)	REAL-ERG/3RD	REAL-ACC/LOCAL	*STRUC	REAL-ERG/LOCAL	REAL-ACC/3RD
Subj/3-ERG			*!		
⇒ Subj/3-∅					

The candidate in which the subject is unmarked does not violate the constraint REAL-ERG/3RD since the features “ergative” and “third” do not co-occur in it. Ergativity is a property holding only of the subjects of *transitive* verbs, and so it is absent in the subject pronoun of this structure. One can suppose that in the absence of ergativity, the third person feature is expressed by null marking or some other marking on the root pronoun. The reader is invited to check that the remainder of the person-based split-ergative system described in Table II is indeed produced by the constraint system in 14.

The preceding considerations lead one to the following simple synchronic analysis of Dyrbal and other person-based split-ergative languages. In these languages, the morphological markings which linguists have traditionally glossed as ergative and accusative case are, in fact, agglutinative markings communicating both the case and the person of the pronoun. Furthermore, the marking system of these languages is characterized by the OT constraint system listed in 14. Tableau 5 - 7 demonstrate how these suppositions would predict the nature of the split-ergative system as described in Table II.

Of course, if the paper began with this synchronic analysis of split-ergativity, the objection which would immediately spring from the reader’s lips would be that it predicts the existence of “anti-split-ergative” languages, ones in which only local person subjects and third person objects are marked. The reader is invited to confirm that the UG-permitted ranking in 15 produces precisely such a language.

$$(15) \quad \begin{array}{l} \{\text{REAL-ERG/LOCAL, REAL-ACC/3RD}\} \gg * \text{STRUC} \gg \\ \{\text{REAL-ERG/3RD, REAL-ACC/LOCAL}\} \end{array}$$

In light of sections 4 and 5, however, the obvious response to this objection is an appeal to the constraints on linguistic form placed by the principles of diachronic development. Although there do exist constraint rankings in which “anti-Dyrbal” is produced, there is no diachronic way for these rankings to be reached. Split-ergative marking systems, we’ve assumed, develop from the kind of pragmatic marking strategy described in Tableau 4. It was argued in section 5 that such a strategy would never allow the marking of only third person objects and local person subjects. One predicts, then, that the process whereby the GLA gradually transforms the pragmatic strategy into a grammatical system would never result in the language described by 15. The putative language is ruled out, not by the synchronic analysis of split-ergativity, but by the diachronic explanation of its origins.

The synchronic analysis does, however, shed light on some interesting typological generalizations governing person-based split-ergativity. It was pointed out in sections 5.2 and 5.4, for example, that the diachronic origin of these systems would alone not explain the fact that the double-marking in structures such as “She-ERG kissed I-ACC” is obligatory in them, nor the fact that they require the subjects of intransitives to be unmarked. One could well imagine reanalyses of the pragmatic marking strategy of Tableau 4 in which these generalizations were broken. Following the present account, however, languages violating these generalizations can be ruled out by the factorial typology of OT. As the reader may quickly check, no reranking of the constraints in 14 will produce either a language in which ergative marking is obligatory on third person subjects with third person objects but optional on third person subjects with local person objects, or a language in which case marking can appear on the subjects of intransitive sentences.

Thus, as was foreshadowed in section 4, limits on the power of reanalysis arise from the limits imposed by UG—in this case the factorial typology of OT—on the possible form of a human language. In brief, the perspective adopted here is that diachronic analysis sometimes offers insight where explanations in terms of UG fall short, but UG is sometimes necessary to understand the limits on diachronic development. Nothing in this is inconsistent or doxastically unstable; it’s exactly what one would expect from the existence both of UG and of laws governing the historical development of language.

This account also makes an interesting claim regarding the “point of reanalysis,” at which the pragmatic marking strategy is made a rule of grammar: it is no different in kind from the previous stages at which the pragmatic marking becomes more widespread. All these stages of development are merely ones in which the rankings of the language’s constraints are adjusted to produce the input-output distributions witnessed by the learners. There is, then, only a vague boundary between the wide-spread use of the pragmatic strategy and its full grammaticalization, a boundary which may have no real theoretical interest. At some point in *L*’s development, the pragmatic marking strategy must fall out of use, and the Stochastic OT grammar alone produces the argument marking in *L*. By this time, however, it is already too late. Argument marking has become so widespread that sociolinguistic pressures against slight variation may nudge the heavy tendency to mark towards the discretely ranked system of 14. After which point, within this sequence of events, one wishes to say that the system has become “grammaticalized” does not appear a question worth pursuing.

### 7.3 Cries From the Peanut Gallery

The account offered above rests on a number of claims which the reader may yet be rather skeptical of. Some points can be offered to counter this skepticism, although it is not presumed that they evaporate it entirely.

One potential worry is that on the proffered account, the grammaticalization of the pragmatic marking strategy occurs too quickly. Within one generation, the proportion of marked third person subjects has skyrocketed from 75% to nearly 96%. Within only four generations the pragmatic marking strategy has become a completely grammaticalized system. Whether this is rate consistent with what has been learned from diachronic study is unknown to the author, but he can appreciate a healthy amount of skepticism on this matter. To weaken some of the skepticism, one may note that the quick descent into grammaticalization is largely an effect of the assumed initial distribution of marking produced by the pragmatic strategy. A small change in this initial distribution has a significant effect on the amount of time needed to achieve full grammaticalization. For example, if the adoption of the pragmatic strategy results in the marking of 70% of third person subjects rather than 75%, then the second generation would mark only 90% of these subjects, and it would require an extra two generations to achieve full grammaticalization. Furthermore, the speakers of later generations may indeed be able to exert some conscious control over the free distribution generated by their stochastic grammars. Speech errors, recall, can be avoided, and so the second-generation learner's awareness of the pragmatic strategy may result in a dampening of the number of new, idiosyncratically marked subjects. This would also work to retard the march towards grammaticalization. Finally, a review of what is known, if anything, about the actual development of marking systems from pragmatic strategies may reveal that such strategies are rather "unstable" and become very quickly grammaticalized.

A second source of skepticism may be the claim that the argument marking of person-based split-ergative languages expresses both case and person features. Unfortunately, little is known by the author about the finer details of the Dyirbal case-marking system which could independently support this idea. A thorough morphological examination of Dyirbal may strengthen or weaken the claim. The resources used in the writing of this paper did not list any actual data from Dyirbal, only schematic descriptions of its case marking system. One direction for further work would be to make such a thorough investigation of various person-based split-ergative languages.

Just as in all OT analyses, however, the most contentious issue lies, per-

haps, with the selection of grammatical constraints. On the one hand, the reader may be doubtful of the “ergativity” feature appealed to in these constraints. On the other hand, he may be doubtful of the way the constraints combine this feature with the person features. The skepticism regarding “ergativity” may be countered somewhat by embedding this analysis in an overarching theory of ergativity as an abstract case. A hypothesis consonant with the present analysis is that “ergative” and “accusative” are abstract cases assigned by transitive verbs in all languages. Language differ only in whether these cases are morphologically expressed. Taking this idea as background, the property of “ergativity” appealed to in the constraint system of 14 can be understood as the abstract case assigned to the pronoun by the verbal head.

Although this might dispel some of the doubt surrounding “ergativity,” there is no easy answer to the second worry. As far as imagining a principled explanation of the association of person and case in the constraints of 14, the author is at a loss. A half-answer would be to locate the origin of these constraints in the Local Conjunction of such constraints as “REAL-ERG” and “REAL-LOCAL”. Although one is loathe to reintroduce Local Conjunction into the theory of Silversteinian alignment, the existence of REAL-ERG and REAL-LOCAL may open the way to a more general theory of argument marking and verbal agreement, one which is, like the present account, based on the grammaticalization of pragmatic marking strategies.

## 8 Conclusion

This paper has proposed a usage-based account of the person-based split-ergative system found in such languages as Dyirbal. Borrowing heavily from the work of Zeevat and Jäger 2001, the proposed account divides the typological generalizations governing these systems into those which are due to their development from pragmatic marking strategies and those due to the structure of UG. Which of the two explanations is employed corresponds nicely to a second property of the generalization. Typological generalizations governing Silversteinian alignment more broadly have a diachronic, usage-based explanation. Typological generalizations which relate to the split-ergative system considered as a grammaticalization of earlier pragmatic strategies receive an explanation based on the limits of Universal Grammar.

Let us now take stock of what has been accomplished with the present usage-based account.

- **Principled Account of Alignment Direction and Selection** The corpus study of Zeevat and Jäger 2001 reveals a predominance for local persons to be subjects and third persons to be objects. This predominance can be understood as emerging from deeper probabilistic associations that are more transparently the result of basic psychological features of human beings.
- **Precise Theory of Pragmatic Marking Strategies** The Bidirectional OT constraint system of Zeevat and Jäger 2001 provides a precise, principled explanation of the arrival of pragmatic marking strategies and the form which those strategies take.
- **Theory of Reanalysis and its Restrictions** The phenomenon of a statistical regularity within a language being interpreted as the result of an obligatory rule may have a rather heterogeneous nature. The proffered account claims that one way in which this arises is due, perversely, to the learning mechanism sometimes interpreting non-random, rule-governed distributions as purely random, free variations in the data. The resulting theory hypothesizes that constraints on reanalysis sometimes stem from the constraints UG places on the form of a human language. This would explain why some reanalyses are possible but not others.
- **Synchronic Analysis of the Dyirbal System** The proposed analysis of the person-based split-ergative system predicts that UG allows for the possibility of the unattested “inverses” of these systems. Such systems, however, can be ruled out on independent, diachronic grounds. Thus, one is reminded that the mere fact that an analysis predicts UG to allow for an unattested language is not definitive proof that the analysis is incorrect. There may be functional or diachronic reasons why the language is unattested, and the benefits of a simpler synchronic analysis should not escape our attention.

One failing of this paper is that it does not provide grounds for believing that the split-ergative system of Dyirbal arose from a pragmatic marking strategy of the kind described in Tableau 4, nor for believing that person-based split-ergative systems in general descend from such strategies. Establishing this claim, however, is best left to others more qualified and who are interested in this approach.

If this paper has accomplished anything, it is hoped that it has supported the idea, first proposed by functionalists and now steadily growing in popularity among generative linguists, that soft statistical tendencies rooted

in the prejudices of human conversation can, through a lengthy chain of diachronic argument, be found to explain the existence of hard typological generalizations. Thus, contrary to the title of Bresnan et al. 2001, hard constraints mirror the softer ones. If true, then this simple idea would greatly challenge the approach to soft-constraint/hard-constraint relation found in the recent work of Bresnan and Aissen, and would discredit the particular use which it makes of Stochastic OT.

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