

# Universals constrain change; change results in typological generalizations

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## 1 The relation between synchrony and diachrony

### 1.1 Structure explains change

If language change is constrained by grammatical structure, then synchronic assumptions have diachronic consequences. Theories of grammar can then in principle contribute to explaining properties of change, or conversely be falsified by historical evidence. This has been the leading idea behind historical linguistics in most of the generative tradition.

Syntactic change has been modeled, depending on the general approach to linguistic description, as parameter-resetting (Lightfoot 1991), as grammar replacement (Kroch 1989), and as constraint reranking (OT, recently especially in its stochastic variety, Jäger 2002, Clark 2004). All make different claims about the causes and mechanisms of change and about how change is related to synchronic variation. Specific theories of syntax make further predictions about co-variation between different aspects of grammar, notably between morphology and syntax.<sup>1</sup>

A widely shared assumption is that change takes place mainly in the transmission of language (through “imperfect learning”). Specifically, it occurs when aspects of grammars based on incomplete data, or outputs of such grammars, are retained from earlier stages of acquisition into the final system. Thus, the theory of acquisition becomes a crucial link between synchronic and diachronic linguistics. This idea has provided the basis for a new approach to analogical change, and, coupled with the theory of Lexical Phonology, provides a solution to the problematic type of phonological change known as lexical diffusion (Kiparsky 1995).

The idea that properties of language change might be explained by the way language is organized in the mind is by no means original to generative grammar. It goes back to structuralist explanations for the empirical generalizations about language change discovered by the neogrammarians. Structuralism, in fact, is really an attempt to ground these findings in a new understanding of the language faculty. The pervasive role of analogy as a regularizing force in change came to be seen as a manifestation of the mechanism that underlies the normal acquisition and creative use of language. The regularity and exceptionlessness of sound change was derived by de Saussure

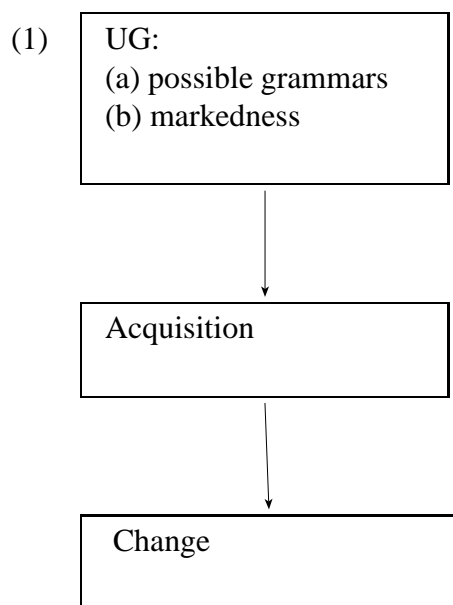
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<sup>1</sup>For example, on some versions of syntax, rich inflectional morphology entails a highly ramified structure of functional categories to which categories move to check their features, predicting that loss of verb agreement entails loss of V-to-I movement (e.g. Vikner 1995). Others treat structural position and inflectional morphology as alternative argument licensers, deriving the Sapir/Jespersen generalization that loss of inflectional morphology entails fixed order of direct nominal arguments (Kiparsky 1997).

from the arbitrariness of the sign. Bloomfield's explanation for it, on the other hand, was based on the notion of separation of levels, and in particular on the premise that the phonological and morphological organization of language are independent:

'Theoretically, we can understand the regular change of phonemes if we suppose that language consists of two layers of habit. One layer is phonemic: the speakers have certain habits of voicing, tongue-movement, and so on. These habits make up the phonetic system of the language. The other layer consists of formal-semantic habits: the speakers habitually utter certain combinations of phonemes in response to certain types of stimuli, and respond appropriately when they hear these same combinations. These habits make up the grammar and lexicon of the language.' (Bloomfield 1933, 364-365)

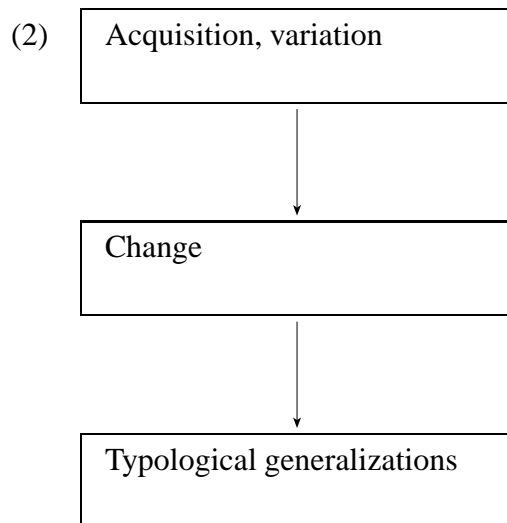
So, the structuralist/generative program for historical linguistics during most of the last century looked something like this:



This program is by no means incompatible with the idea that functional factors, as well as UG itself, might constrain change and acquisition. For example, evolutionary pressures might have caused UG to favor grammars of a type that optimizes perception, production, and/or stable transmission, and a language designed in modular fashion, with different levels of representation subject to their own constraints, may well be the most efficient for this purpose.

## 1.2 Change explains structure

Seemingly at odds with the paradigm in (1) is an older, pre-structuralist idea which is recently regaining popularity, that the direction of explanation goes the other way: cross-linguistically recurrent structural patterns in grammar are due to recurrent patterns of language change (Bybee 1988, Aristar 1991, Garrett 1990, Blevins 2003).



One variant of this type of historical explanation presents itself as a kind of supplement or corrective to the formal theory of grammar. For example, it is sometimes claimed that UG should be seen as a theory of “core grammar”, and that vicissitudes of language change under some circumstances can produce “marked” or anomalous structures which fall outside the remit of UG. Such is, for example, a common view of split ergative case systems, which have no straightforward analysis in GB and its successor theories:

‘The split nature of many ergative-absolutive case systems looks like another Rube Goldberg feature of grammars, but we can understand how they might have arisen historically.’ (Lightfoot 1999:141)

Conversely, it has been claimed that the formal theory of language should *overgenerate* by allowing for possible types which are unattested simply because they cannot arise — or at least cannot easily arise — through normal processes of change (Harris 2003).

The most radical form of this program does not seek to complement the theory of UG but to replace it. It tries to explain away putative universals as by-products of recurrent patterns of language change. As Aristar puts it, “... disparate diachronic processes can conspire to give the effect of synchronic universals” (1991). As a case study he offers an account of the Greenberg word-order universal in (3):

- (3) Genitives, relatives, and adjectives usually precede their heads in SOV languages and follow them in VSO languages.

The basis of (3) (according to Aristar’s historical reinterpretation) is simply a *historical* relationship between the three categories:

‘One reason why genitives, relatives, and adjectivals pattern so similarly in languages across the world is that genitives and relatives can potentially arise from the same anaphoric construction, and genitives and relatives are a potential diachronic source for adjectival forms.’ (Aristar 1991)

The competing *structural* explanation for (3) is of course that genitives, relatives, adjectives, and subjects are specifiers, and that, by cross-categorical harmony (Hawkins 1983) all specifiers will normally either precede or follow their heads.

In this particular case, the structural explanation based on cross-categorial harmony seems to have the edge over the historical explanation. It correctly extends to adjectives from other sources than genitives and relatives, for which the word order correlation stated in (3) is just as valid. Moreover, the fact that genitives and adjectives tend to go hand in hand in word order *change* (as they have in the history of English) indicates that the syntactic relation between them is intrinsic. Furthermore, the parallelism is more fine-grained than common origin could explain, and there are systematic disparities which are subject to additional generalizations (Dryer 1988, 1992). Giorgi & Longobardi (1991, Ch. 3) show that at least some of these additional generalizations can be explained within the theory of grammar by independently motivated assumptions about the phrase structure of nominals.

Moreover, historical explanations, once spelled out, often turn out to appeal implicitly to tendencies that are themselves in need of explanation. For example, without a theory of categories and phrase structure, the direction of reanalysis which Aristar takes as a given is just as puzzling as the typological universal it is supposed to ground. For a true explanation we need a theory of phrase structure and grammatical categories. Genitives and relatives are indeed among the diachronic sources for adjectives, but the possibility of the reanalysis is rooted precisely in their common status as nominal specifiers.

### 1.3 The program

The task then, is to determine the proper direction and locus of explanation. When is change the explanans, when is it the explanandum? Answering that question will give a basis for distinguishing true universals from typological generalizations. The issue goes well beyond the simple question how cross-linguistic generalizations should be explained. It is about the nature of those generalizations themselves. Whatever arises through language change can be lost through language change (unless it gets somehow incorporated into the genome). Any generalization that is caused by change is inherently unstable. It can be washed out by other changes, or replaced with the opposite generalization. Such TYPOLOGICAL GENERALIZATIONS are not true universals. By the same token, if a generalization is itself a determinant of historical change, it must be a true intrinsic UNIVERSAL, which is properly the subject matter of UG. Such a generalization should never be violated except when another, dominant universal constraint compels it, in the sense of OT.

Summarizing the above discussion, and anticipating what follows, let us posit the tentative criteria in (4).

(4)	<i>Universals</i>	<i>Typological generalizations</i>
	Irreversible	Reversible
	Convergence	Single source
	TETU effects	No TETU effects
	Manifested spontaneously in child language	Not manifested in child language
	Pathways of change	Inert
	Structurally encoded in the grammar	Not necessarily structurally encoded

I will now apply these criteria to a number of proposed typological generalizations and candidate universals. The material is drawn from both phonology and syntax; many of the cases involve a scale or hierarchy which defines the parameter of a rule or constraint. The results show, I think,

that the criteria in (4) converge fairly neatly to sort out the true universals, in the above sense, from the typological generalizations.

## 2 Morphology and binding properties of reflexives

A case where I think diachrony convincingly explains a set of typological generalizations has to do with the relation between the morphological properties of anaphors and their binding behavior. Here I will be drawing on the typology of anaphora proposed in Kiparsky 2002.

Reflexive pronouns are of two main morphological types, SIMPLE and COMPLEX. Simple reflexives are typically monomorphemic elements, such as French *se*, German *sich*, Russian *sebjä*. Complex reflexives are of two types: (1) the *head*-type, which consists of a possessive pronoun combined with an inalienably possessed noun, typically “head” or “body”, and (2) the *self*-type, which consists of a reflexive or pronominal combined with an adverb that means “self” (German *selbst*, Swedish *själv*, French *même*, Italian *stesso*, Russian *sam*).<sup>2</sup>

A typological generalization (“Pica’s generalization”) discovered by Faltz 1977 and theoretically explored by Pica 1987 states that complex reflexives typically differ from simple reflexives as follows:

- (5) a. They allow object antecedents.
- b. They must be bound locally within the same clause.
- c. They typically lack possessive forms.

The synchronic explanation for (5) that has been proposed in the literature is that complex reflexives are maximal projections, whereas simple reflexives are heads, and cliticize to Infl at S-structure or at LF, where they are C-commanded by subjects only (Pica 1991, cf. Katada 1991, Hestvik 1992). Long-distance binding by successive cyclic movement. Problems with this account include that this LF movement would violate both the Coordinate Structure Constraint and the ECP; it is also not clear how to get long-distance binding of reflexives inside maximal projections.

The alternative historical explanation is that complex reflexives arise as anti-obviation strategies. A universal principle of *Coargument Disjoint Reference* (CDR) requires that coarguments (arguments of the same predicate) cannot overlap in reference, unless they are specified as non-obviative (Kiparsky 2002).

- (6) CDR: An obviate pronoun cannot overlap in reference with a coargument.
  - a. John hates him. (there must be two people involved)
  - b. Each of the men hate him. (“he” isn’t one of “the men”)

CDR applies not only to R-expressions (nominal and pronominal elements) but also to anaphors, unless they are specially marked as exempt from CDR.

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<sup>2</sup>Complex reflexives of the *head*-type: “(POSS)+head”: Hausa *da kansa*, Dogon *ku wo*, Fula *hoore mum*, Abkhaz *ax*, Ewe *e dokui*, Basque *bere burua*, Mordvin *þřan+ts*; “(POSS)+body”: Fox *ni:yawi*, Krongo *ðonó*, Jukun *-dì a `*, Bari *mUgUn*, Yağ Dii *f’o’o wòð*, Mundani *enyit*; “(POSS)+heart”: Nubian *ai(l)*, Turkic *ö:z*; “(POSS)+side”: Godié *ɔ yəku*; “(POSS)+self” and others: Tagalog *sarili niya*, Lyélé *n cinə*, Finnish *itse+nsä*; Modern Hebrew *acmo*; Trique *ma<sup>32</sup>ã<sup>13</sup> zo<sup>23</sup>*. Complex reflexives of *self*-type: *himself*, Spanish *si mismo*, Russian *samogo sebja*, Classical Greek *he+auton*, Amele *uga dodoc*, Tzotzil *s-ba*, Dakota *ic’i*. Also reduplication of the personal pronoun, as in Ostyak (Khanty).

The two types of complex reflexives in represent precisely the two ways in which a pronoun (whether pronominal or anaphor) subject to CDR can be marked so as to escape it. “Head”-type complex reflexives defeat this constraint by putting the pronoun into a non-coargument position. *Self*-type complex reflexives defeat it by marking the pronoun as non-obviative (by adding an element which asserts identity between the pronoun and a contextually determined element). The distribution of complex reflexives is restricted to environments where CDR needs to be defeated. The properties of complex reflexives in (5) then follow straightforwardly.

- (7)
- a. Complex reflexives allow object antecedents because they are not subject to CDR.
  - b. They are bound within the same clause because long-distance antecedents are not coarguments of them.
  - c. They typically lack possessive forms because a possessor is not a coargument of its possessum’s coarguments.

If this explanation is correct, then (5) is not a linguistic universal and should NOT be expressed in the synchronic theory of grammar. This might be a welcome conclusion for several reasons.

- (8)
- a. The generalization is not inviolable (see the exceptions documented in Huang 2000:96).
  - b. All complex reflexives seem to arise in the same way, by the route described above.
  - c. There no acquisition evidence which would show that learners access it.
  - d. The generalization is probably not structurally encodable anyway. Typically the distribution of morphologically complex reflexives like *himself* is the same as that of the simple pronouns they contain (such as *him*), so they are probably not maximal projections (Toivonen 2001). And (at least on lexicalist assumptions) the syntax has no access to the morphological composition of words.

Another typological generalization is (9):

- (9) There are no nominative anaphors.

This is trivially true in languages in which anaphors must be locally bound to a nominative subject. But it is not obvious why it would hold even in languages which allow long-distance binding, or in which nominative case may be assigned to objects (such as Icelandic).

- (10) Icelandic (Maling L&P 1984)

- a. \*Honum finnst (sjálfur) sig (vera) veikur  
Him-Dat finds self Refl-Nom (be) sick-Nom  
‘He considers himself (to be) sick’ (no reflexive nominative object!)
- b. Hann sagði að sig vantaði hæfileika  
He said that self-Dat lacked ability-Nom  
‘He considers himself (to be) sick’ (reflexive dative subject!)

As we’ll see, the generalization (9) is actually not true, but it is still a pretty robust tendency, so some explanation is called for. Synchronic explanations that have been proposed for (9) include LF movement subject to the ECP, and several agreement-based hypotheses.

The ECP explanation, due to Chomsky 1986, posits that anaphors move at LF to INFL, leaving a trace; in subject position the trace would not be properly governed. This does not really account for nominative objects, or for pronominal chains.

Rizzi 1990 and Woolford 1999 proposed instead that nominatives agree with AGR, which is pronominal. If the nominative were an anaphor, the result would be a chain which would have to be both locally bound and locally free. However, this still won't work for nominative objects.

Everaert 2001 developed a minimalist version of the agreement story, according to which V's uninterpretable  $\phi$ -features must be checked against an agreeing N's interpretable features. Nominative anaphors are excluded if they are not fully specified for some  $\phi$ -feature that must be licensed on the V. E.g. Icelandic *sig* is unspecified for number, so it can't check the V's number feature. On the other hand, Georgian and Marathi nominative reflexives are OK because they agree.

(11) Georgian (Harris 1981, 1984, Everaert 2001)

- a. Vano-m daurçmuna tavis-i tav-i  
 Vano-Erg he-convince-him-Aor self's self-Nom  
 'Vano convinced himself' (reflexive nominative agreeing object)
- b. Gela-s turme daurçmunebia tavis-i tav-i  
 Gela-Dat apparently he-convince-him-Ev self's self-Nom  
 'Gela has apparently convinced himself.'
- c. tavis-ma tav-ma  $\emptyset$ -xsn-a president-i  
 self's-Erg self-Erg he-saved-him president-Nom  
 'It was the president who saved himself (no-one else did it)' (reflexive ergative agreeing subject)

(12) Marathi

- Jane-ne<sub>i</sub> John-laa<sub>j</sub> kaļavle ki aapan<sub>i,\*j</sub> turangaat aahot  
 Jane-Erg John-Acc informed-3Sg that self-Nom prison-Loc was-3Sg  
 'Jane<sub>i</sub> informed John<sub>j</sub> that self<sub>i,\*j</sub> was in prison.' (reflexive nominative agreeing subject)

Everaert's solution comes a lot closer, but it still runs into empirical problems. The correlation it predicts breaks down in both directions. Swedish has an unspecified reflexive (*sig*) and no V agreement, yet still lacks nominative anaphors. Choctaw has an unspecified reflexive and rich V agreement, yet does have nominative anaphors (Broadwell *NELS* 18.)

The historical explanation, admittedly rather unexciting by comparison, starts from the observation that when nominative objects are prohibited and subjects can't be bound outside a finite clause, nominative anaphors are simply impossible. Germanic and Romance were originally such languages. The morphological gap persisted even after nominative objects and/or long-distance binding arose in some of them, as in Icelandic. Marathi and Georgian, on the other hand, never inherited such a constraint. Marathi has had long-distance binding, including of nominatives, as long as it has had the reflexive *aapan* (from Sanskrit *ātman* "soul, self").

- (13) khadgena śakyate yuddhe sādhv ātmā parirakṣitum  
 sword-Instr can-3Sg combat-Loc well self-Nom protect  
 'one can protect oneself well with a sword in combat' ("one's self can be protected well")  
 (*Mbh* 12.160.3)

That is really all that needs to be said. There is simply no synchronic principle at work. The historical explanation covers the data perfectly.

### 3 Coda neutralization

As a case where the evidence points in the opposite direction, let's consider the robust phonological generalization that marked feature values tend to be suppressed in certain prosodic positions. Specifically, place and manner features are frequently neutralized in favor of their unmarked values in syllable codas. The best-known example of this process is probably final devoicing of obstruents, as in German, most Slavic languages, Catalan, Turkish, Korean, and in many dialects of English.<sup>3</sup> It seems to be irreversible, in the sense that no language has a converse process of final voicing in its phonology. Why should that be?

Again there are a priori two possible answers to this question. One locates the neutralization constraint in the design of language. It says that syllable codas universally favor unmarked feature values (such as voicelessness, in the case at hand). This does not mean that coda neutralization applies in all languages; it just means that, whenever it does apply, it always imposes the unmarked feature value. Nor does it mean that the asymmetry is arbitrary or unmotivated. A plausible reason for coda neutralization might be the low perceptual saliency of the relevant featural distinctions in the syllable coda (Steriade 2004), and a plausible reason why neutralization favors unmarked feature values might be the greater economy of the relevant articulatory gestures.

The second possible answer locates the explanation in the diachronic plane. Blevins (MS) suggests that there are documented types of sound change that devoice final voiced obstruents, but there are no documented types of sound change that would voice them. The idea is that the generalization follows trivially from this unidirectionality of change. Of course, the putative unidirectionality would in turn have to be explained; perhaps on the basis of the perceptual and articulatory asymmetries just mentioned, but now operating on the diachronic plane as constraints on the relevant sound change processes.

The two versions of the story are in many ways similar, especially in how they ground the asymmetry. The crucial difference between them is where the explanation applies. The historical account locates it solely in sound change. On this view, once a neutralization process has been “deposited” in a language by sound change, it just sits there as a brute fact. It has no synchronic rationale, any more than, say, the geological stratification of the earth's crust does. The synchronic account, on the other hand, makes it part of the design of language. It says that a language which violates the universal (e.g. through a final voicing process) would be not just historically unattainable, but hard to learn and maybe hard to use, or both. Versions of OT phonology that posit that all constraints are universal and do not allow contradictory constraints even claim that such a system would be impossible to learn.

Several considerations suggest that the historical account is not on the right track in this case. It is easy to construct scenarios that, unchecked, would produce the exact opposite process, of coda neutralization in favor of the marked feature value. For example, final degemination (itself also an instance of coda neutralization) and the transposition of a geminate/singleton opposition into a voiceless/voiced opposition (the Romance languages offer many examples of such lenition) are both common types of sound change. When both sound changes take place in sequence, the result would be final voicing:

(14) A hypothetical path to final voicing: markedness reversal

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<sup>3</sup>Australia, South Africa (Wissing and Zonneveld 1996), plus individual idiolects, including the current president of the U.S.A., who devoices his final fricatives.

Stage 1: tatta tata tat \*tatt (degemination, no distinctive voicing)

Stage 2: tata tada tad \*tat (lenition)

- The resulting grammar has morphophonological (neutralizing) final voicing.

Or consider a language in which intervocalic obstruents are redundantly voiced, and all words end in vowels. If final vowels are then lost after single consonants, the synchronic result is final voicing.

(15) Another hypothetical path to final voicing: lenition plus apocope

Stage 1: takta tada \*tata (intervocalic voicing, no geminates)

Stage 2: takta tad \*tat (apocope)

- The resulting grammar has allophonic final voicing.

These fictitious but plausible scenarios show that final voicing *could* originate by everyday sound changes. If it in fact never arises, *some constraint on the design of language must prevent it*.

There is independent support for the conclusion that the markedness asymmetry seen in coda neutralization processes is not simply a by-product of sound change but reflects an intrinsic linguistic constraint.

First, the asymmetry arises in other ways than by sound change. In a language like German, obstruents which become final when word-final *-e* is deleted are automatically devoiced (just like underlying final obstruents). There is no intermediate stage of voicing which is then devoiced. Moreover, variable deletion of final *-e* is relatively favored after voiceless stops and disfavored after voiced stops.

Additional evidence comes from TETU (“The Emergence of the Unmarked”) effects, manifestations of latent markedness constraints where higher-ranking constraints that override them are not in play (Prince and Smolensky 1993). For example, final devoicing reportedly applies spontaneously in languages with strict CV syllable structure when their speakers attempt to pronounce loanwords ending in -CVC. In Konni (northern Ghana), regressive voicing assimilation (cf. (16a)) is blocked just when it would give rise to a voiced coda, in which case an epenthetic vowel is inserted instead (as in (16b)) (Cahill 1999).

(16) a. /tig-ka/ → *tikka* ‘the village’

b. /biis-bu/ → *biisibu* ‘the breast’ (\*[biizbu])

In Meccan Arabic (McCarthy 2003), voicing assimilation (cf. (17a)) is blocked precisely when it would give rise to a voiced coda (as in (17b)), even though voiced codas are not in general prohibited in the language (see (17c)):

(17) a. /mazku:r/ → *masku:r* ‘mentioned’

b. /ʔakbar/ → *ʔakbar* (\**agbar*) ‘older’

c. /dabdaba/ → *dabdaba* ‘pitter-pat (footsteps)’

Language acquisition points in the same direction: final devoicing often occurs in the speech of young children (Smith 1973, Ingram 1976, Yavas 1994).

If coda neutralization is part of UG, it should function as a rule/constraint and interact with other rules/constraints and principles within grammatical systems. Indeed, in numerous languages, coda neutralization functions as a productive phonological process, sometimes deeply embedded. For example, in Lakhota, coda neutralization operates in the stem-level phonology, but is masked at the word level by insertion of an epenthetic final vowel, which in turn triggers devoicing of the final obstruent if it is a fricative (see (18b,c)).

- (18) a. /čhap/ *čhápa* ‘beaver’  
 b. /pus/ *púza* ‘to be dry’  
 c. /leš/ *léža* ‘to urinate’

The words in (18) are underlyingly monosyllabic, as shown independently of coda neutralization by their initial stress (Lakhota words are otherwise stressed on their second or only syllable) and by reduplication, compounding, and other morphological processes:

- (19) a. *čhap-khúwa* ‘he is beaver hunting’  
 b. *pus-yá* ‘cause to dry’  
 c. *pus-púza* ‘be very dry’

## 4 Stress/weight solidarity

Now let’s look at the well-known sonority hierarchy, and in particular the relative sonority of vowels, proposed by de Saussure and Jespersen (among many others) as an intrinsic universal, which is grounded articulatorily in the relative aperture of the vocal tract and/or acoustically in loudness and intrinsic duration.

- (20)  $a > e, o > i, u > \text{ə}$ .

The relative sonority defined by (20) is one of a larger complex of sonority scales which involve syllable weight, pitch (de Lacy 2002b) and perhaps others. Several phonological constraints refer to sonority, but for our present purposes the relevant one is the stress/sonority solidarity generalization stated in (21).

- (21) Stress seeks heavy syllables and sonorous vowels, where sonority is defined by the scale (20).

De Lacy (2002a) extensively documents the generalization stated in (22), and argues that it is true of all universal hierarchies, at least in phonology.

- (22) Adjacent points on the scale may be conflated, but not reversed.

Let us assume, for purposes of the following discussion, that de Lacy’s empirical generalization is correct, and apply the criteria in (4) to determine whether the hierarchy (20) is truly a universal, or simply a typological generalization, as defined above.

As a basis for our discussion, let us consider the sonority-based stress system of Gujarati extensively analyzed by de Lacy (2002a), who formulates the empirical generalizations in (23).

- (23) • Words are normally stressed on the penult, but  
 • an antepenult is stressed if it is more prominent than the penult on the (partially conflated) sonority scale  $a > e, o, i, u > \text{ə}$ , and  
 • the final syllable is stressed if it is the only syllable with  $a$ .

The data in (24) show how stress is assigned in accord with (23).

- (24) a. *azádi* ‘freedom’  
b. *ekóter* ‘71’  
c. *pəddhóti* ‘guide’  
d. *tájetər* ‘recently’ ( $a > e$ , attracts stress to the antepenult)  
e. *kójəldi* ‘little cuckoo’ ( $o > ə$ , attracts stress to the antepenult)  
f. *ʃəpərá* ‘girls’ ( $a >$  other vowels, attracts stress to the final syllable)

De Lacy proposes for Gujarati a synchronic stress system which I informally summarize in (25):

- (25) a. Assign stress to a heavy/sonorous syllable (STRESS-TO-WEIGHT), otherwise  
b. assign a trochaic foot at the right edge of the word.

Couched in OT, de Lacy’s proposal is explanatory in the sense that it predicts the possibility of the pattern in (23) from a set of ranked universal constraints.

The alternative historical explanation for the weight/stress solidarity seen in Gujarati might invoke the following a reasonable assumption about sound change (Blevins 2004):

- (26) Intrinsic acoustic prominence of sonorous vowels may be reinterpreted as stress in sound change.

The validity of generalization (26) is not at issue here; let us assume that it is correct as stated. The question is whether it provides a sufficient alternative to de Lacy’s proposal that (20), (21), and (22) are part of UG. Let us see what the criteria in (4) say.

The sonority hierarchy in (20) seems to be the same in all languages (this is the import of de Lacy’s result in (22)). But there are natural types of sound change that *could* reverse it. For example, a sound change  $[a] > [ə]$  (such as took place in Sanskrit, among other languages) could result in stress systems where  $ə$  functions as the *most* sonorous vowel, attracting stress in words like *\*tájetər*, contrary to (20)-(22). The result would be precisely a stress system with a rule/constraint which violates the proposed universal. The moral is that what sound change can create, it also can destroy. Therefore, if a generalization is exceptionless, then there must be something more than sound change that sustains it.

Note carefully what is at stake here. The claim is *not* that sound change cannot subvert the phonological regularities of a language. For example, sound change could presumably conflate or delete vowels in such a way as to destroy the phonological regularities of Gujarati’s stress system, with the result that it would have to be reanalyzed with lexically marked stress. What sound change apparently cannot do is to arbitrarily *reverse* the universal hierarchies. If so, then it follows sound change cannot be quite as “blind” as the neogrammarians thought. It must operate under the control of UG. Changes that subvert universals must either be blocked, or the system they give rise to has to be promptly reorganized.

The second criterion concerns multiple paths. Weight/stress solidarity is in fact implemented in very diverse ways across languages. E.g. in Finnish, it both makes unstressed diphthongs monomoraic in the lexical phonology, and prevents contraction of unstressed long vowels (Kiparsky 2003). Neither of these manifestations of it can be attributed to the perceptual confusion between weight and stress. This shows that the sound change theory of the origin of sonority hierarchy effects is not general enough.

Third: the generalization must be encoded in the grammar of Gujarati because it is productive. It locates stress in Gujarati loanwords, e.g. *sinemá* ‘movie theater’ (presumably from English *cinema*), which gets final stress in Gujarati by (23) because of the sonorous *a*. A similar point can be made for Finnish, where the sonority hierarchy determines fixed secondary stress in loanwords (Anttila 1997, Kiparsky 2003).

Fourth: the generalization underlies TETU (Emergence of the Unmarked) effects. In Finnish, stem-final syllables receive secondary stress optionally in the lexical phonology, with a frequency that is proportional to the sonority of the vowel. The presence or absence of this stress triggers far-reaching allomorphy effects on the stem and the ending (Anttila 1997, Kiparsky 2003). At the phonetic level, lexically unstressed syllables, regardless of their sonority, receive a rhythmic stress which is phonetically indistinguishable from the lexical stress — but which has no effects whatsoever on allomorphy. Therefore, the effect of relative sonority on lexical stress and on allomorphy cannot be attributed to misperception.

Fifth: the sonority hierarchy defines a pathway of analogical change. Anttila (1997, Ch. 3) documents the analogical spread of long genitives (the forms in the right-hand column) noun inflection over the attested history of Finnish. He shows that it follows the course in (27).

- (27) *-i, -u* stems    *líntuin*    >    *líntujen*    16th century  
*-e, -o* stems    *péltoin*    >    *péltojen*    19th century  
*-a, -ä* stems    *ákkain*    >    *ákkojen*    20th century

Recall the weight/sonority solidarity generalization: sonorous vowels (such as *a*) prefer to be in stressed and heavy syllables, nonsonorous vowels (such as *i, u*) prefer to be in unstressed and light syllables. From this perspective, the trajectory in (27) is understandable: the morphological analogy is implemented first where weight/sonority solidarity favors it most, and last where weight/sonority solidarity does not favor it at all. Here the sonority hierarchy governs the course of morphological change in a way which cannot have anything to do with the misperception of stress.

## 5 Ergative case systems

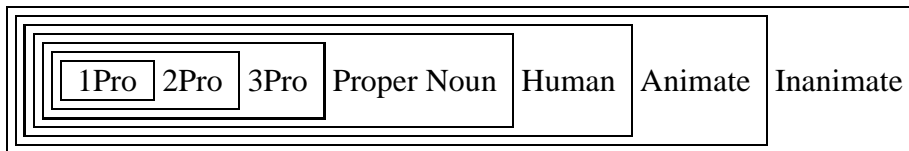
A case assigned to subjects of transitive verbs but not to subjects of intransitive verbs is called ERGATIVE. SPLIT ERGATIVE systems have ergative case marking under restricted conditions, most commonly depending either on the nature of the NP or on the tense/aspect of the verb. A classical example of an NP split ergative case system is Dyirbal, which has an ergative/nominative opposition in nouns, and a nominative/accusative opposition in pronouns (Dixon 1972). Dyirbal’s structural case system is shown in (28), where “A”, “O” and “S” denote the subject and object of a transitive verb and the subject of an intransitive verb, respectively.

(28)

	A	S	O
Nouns	ERG <i>-nggu, -ru</i>	NOM <i>-∅</i>	NOM <i>-∅</i>
Pronouns	NOM <i>-∅</i>	NOM <i>-∅</i>	ACC <i>-na</i>

When split ergativity is conditioned by the inherent category of the NP, the cases tend to be distributed according to the hierarchy in [29].

(29) “Animacy” hierarchy (Hale, Silverstein, Dixon)



Ergative is found in nominals on the right end (the “low” end) up to some cutoff-point on the hierarchy, and accusative in nominals from the left (the “high” end). In Dyirbal, the two case marking subsystems divide nominals cleanly into two groups, but in some languages the cutoff-points don’t coincide:

(30) Djapu (Morphy 1983):

	A	S	O
Non-human	<i>-thu</i>		
Human	<i>-thu</i>		<i>-nha</i>
Pronouns			<i>-nha</i>

The distribution of accusative case in some Australian languages illustrates the various possible cutoff-points (adapted from Blake 1977, 1987):

(31)

	Pronouns	Proper/Kin	Human	Animate	Inanimate
Thargari	Accusative				Erg
Arabana	Accusative			Ergative	
Gumbainggir	Accusative		Ergative		
Dyirbal	Accusative	Ergative			
Warlpiri	Ergative				
Djapu	Accusative		Ergative		

It is commonly claimed that ergative case marking, and specifically split ergativity of this type, is structurally or functionally unmotivated, but arises through understandable diachronic processes:

- (32) a. “NP split-ergative systems in fact have their striking synchronic features as a straightforward consequence of their ordinary diachronic source [instrumental case]” (Garrett, 1990).
- b. “The split nature of many ergative-absolutive case systems looks like another Rube Goldberg feature of grammars, but we can understand how they might have arisen historically.” (Lightfoot 1999:141)
- c. “These restrictions [split ergative case marking patterns] make little synchronic sense for an active-direct ergative clause in which the agent is the more topical — proximate — argument.” (Givón 1994:33).

Garrett 1990 argues that the lack of ergative marking on the high end of the D-hierarchy has (and needs) no synchronic explanation. It is only at an antecedent historical stage that it is motivated. Garrett assumes that those ergative cases which exhibit such a gap in their distribution are historically derived from instrumental cases, and proposes to explain the gap as an inheritance from this stage. The idea is that the gap is a reflection of the fact that instrumental case is for

pragmatic reasons most common with inanimate nouns, for it typically denotes instruments which are normally inanimate. After the instrumental-to-ergative reanalysis, the inherited restriction is transferred to the new ergative case. In its new form however the restriction no longer has any synchronic motivation, and therefore tends to get eliminated by analogical spread of ergative case to animate nouns. Pronouns, though, tend to be morphologically so different from nouns that they escape their analogical influence. The typical outcome of this scenario, then is a noun/pronoun split.

I believe that Garrett's purely historical account for the split ergativity patterns is not viable, for the following reasons.

First, the historical scenario involves an unexplained shift from a *pragmatic* gap before the reanalysis to an *allomorphic* gap after the reanalysis. Instruments tend to be restricted to Low-D nominals, simply because tools are normally inanimate. Garrett assumes that when instrumental case is reanalyzed as ergative case, this restriction is carried over, resulting in split ergativity. The assumption that "the distribution of newly-created linguistic categories parallels that of their immediate antecedents" (Garrett 1990, 286) is per se reasonable. But taken literally, it would imply that after the instrumental to ergative reanalysis, high-D nominals would lack ergative case. The truth is that high-D nominals do *not* lack ergative case; rather, they have ergative/nominative syncretism, hence ergative forms with no overt *case marking* — a very different thing. The suffixless ergative pronouns have exactly the same syntax as overtly marked ergative nominals: in particular, they agree with them in case, and are treated as parallel with them in conjunction. The gap in ergative paradigms is this a matter of allomorphy, not a gap in the distribution of the category of ergative case (and certainly not a matter of pragmatics). What is missing from the historical account, then, is the causal link between the putative pragmatically motivated gap in the distribution of the former instrumental case and the zero allomorph that it leaves behind in the paradigm of the reanalyzed ergative case. In other reanalyses this kind of thing generally does not happen.<sup>4</sup> For example, instrumental case itself typically comes from comitative case, via a chain of development which runs from "in the company of" (*John ate cheese with Mary*) via "accompanied by" (*John ate cheese with wine*) to "by means of" (*John ate cheese with a fork*).<sup>5</sup> At the outset, inanimate nouns would not be used in the comitative (for "pragmatic" reasons), and yet we never find their instrumental offspring sporting zero allomorphs on inanimate nouns (or lacking instrumental case, for that matter). Similarly, ablative cases denoting source-type relations often originate as local cases with a separative meaning; a local separative case would be restricted to nouns denoting physical objects (for "pragmatic" reasons), and yet we don't find ablatives with zero allomorphs on abstract nouns.<sup>6</sup>

The second argument against a historical account of the case marking pattern is that it is not general enough. It addresses only the distribution of ergative/nominative syncretism. But the same hierarchy also determines the distribution of genitive/nominative syncretism, as in Yukagir, where

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<sup>4</sup>Indeed, it might be argued that the very possibility of reanalysis is inconsistent with the transfer of a pragmatic gap motivated by the original meaning. You cannot know about a pragmatic gap without having learned the semantics on which the pragmatic inference depends (in this putative case, the instrumental meaning), but once you have learned that meaning, you will not reanalyze that meaning as another meaning.

<sup>5</sup>An example of this development is Estonian *-ga*, etymologically from *kansa-ssa* "in the crowd".

<sup>6</sup>A third example is the reanalysis of possessive suffixes as object markers of definite NPs that has taken place in some languages belonging to the Permic branch of the Finno-Ugric family. Accusative case, which was lost in the nominal inflection of the Permic languages (a state of affairs preserved in Ostyak) was renewed in Komi and Votyak by reanalysis of the 3.p. possessive suffix as an accusative. Yet we do not find that those nouns which for pragmatic reasons rarely have a possessor (let us say "sky", "sun") lack accusative endings (let alone accusative case) in these languages.

there is no reason to suspect an instrumental origin for genitive case (Nichols 1992:53, Krejnovič 1958, 80, 63 ff.).

- (33) a. met nime  
 I house  
 ‘my house’  
 b. Beke ile  
 Beke deer  
 ‘Beke’s deer’  
 c. ile-n jawul  
 deer-GEN track  
 ‘(the/a) deer’s tracks’

In the opposite direction, the hierarchy also determines the distribution of accusative/nominative syncretism already mentioned in [31]. It also governs dative/accusative syncretism and ergative/dative syncretism, and for the same reason.

- (34) “Split dativity” (Spanish, Hindi. . .):  
 Pedro vio a una mujer (\*a un accidente).  
 Pedro saw Dat a woman (Dat an accident).  
 ‘Pedro saw a (specific) woman (an accident).’

- (35) “Split accusativity”  
 a. Lardil: “In imperatives, the direct object lacks *-intha* if it is third person, although if first person it takes that case. . . (Klokeid 1978).  
 b. Koira näh-tiin. Häne-t näh-tiin. (Finnish)  
 Dog-**Nom** see-PassPast He-**Acc** see-PassPast  
 ‘The/a dog was seen.’ ‘He was seen’  
 c. Finnish child language and certain dialects: accusative extended to proper names and appellatives, e.g. *Lauri-t, isi-t* ‘daddy’.

Furthermore, the same hierarchy also governs inverse systems, which are typically found with agreement rather than case, where instrumental to ergative reanalysis cannot be at stake. Therefore, we need a more comprehensive explanation for the different manifestations of the hierarchy than the instrumental-to-ergative reanalysis scenario by itself can provide.

The third objection is that the historical account is insufficiently general even for the distribution of ergative endings because the phenomenon to be explained has several historical sources. It is not even always true that instrumental case prior to its reanalysis as ergative is restricted to inanimates. More often than not, instrumental case has other functions than expressing instruments, such as marking demoted agents of passives, which are prototypically animate, in fact prototypically human. But such instrumental agents are also reanalyzed as ergative subjects in passive-to-ergative reanalyses, as in Indo-Aryan and Polynesian. And ergative cases with these origins can also lack an overt ergative case morpheme at the high end of the D-scale; this is the case for first and second person pronouns in a number of Indo-Aryan languages, including Marathi, Punjabi, Eastern Rajasthani, Assamese, and Siraiki (Masica 1991:252). The examples in [36] are from Siraiki (= Lahanda, Bhatia 1983:181):

- (36) a. māi axbaar ḍiṭṭhii  
 I newspaper.fs see-pst.3fs  
 ‘I saw the newspaper’

b. huu ne axbaar δit̪hii  
 he ERG newspaper.fs see-pst.3fs  
 'he saw the newspaper'

Since it is highly improbable that the Indo-Aryan instrumental case from which *ne* descends was at any stage restricted to inanimates, the high-D nominative/ergative syncretism in these languages has to be explained in some other way. It is in fact the result of ergative forms being extended to nominatives.<sup>7</sup> Similarly, Tibeto-Burman starts with a fully ergative case-marking system, and nominative case spreads in an orderly way down the hierarchy (Bauman 1979).

Nor are high-D unmarked ergatives always descended from former instrumental cases (or, for that matter, from any other case whose exclusion from animates could be motivated semantically or pragmatically). Ergative case can also originate as a generalized oblique case, or dative case. This seems to be the case in the Daghestanian (NE Caucasian) languages, and in those NW Caucasian languages which have developed a case system. Still, in several of them, pronouns (and sometimes in addition a class of high-D nouns) lack overt endings in the ergative, e.g. Adyghe:

(37) 'house' 'I'  
 Nom. vnə(-r) sə  
 Erg. vnə-m sə

In Adyghe and the fairly closely related Kabardian and Ubykh, several of the numerous functions of ergative case (recipient, locative, time) would be puzzling for a reanalyzed instrumental case. The instrumental (used mainly for instruments and paths) is alive and well in the declension, and there is no morphological gap connected with animacy.

High-D ergative/nominative syncretism seems to have yet another source in some Australian languages. The morphologically unmarked pronominal forms used in S and A function are thought to be ergative in origin, as in Indo-Aryan. Dixon (1980) reconstructs an original three-way nominative/accusative/ergative case opposition for Pama-Nyungan. Most of the daughter languages adopted a disyllabic minimal word requirement, as a result of which the monosyllabic nominative pronouns were either lost, or, in a few languages, augmented with an empty second syllable. In those languages where the nominative was lost, its function was taken over by the ergative. The original three-way case contrast survives in that minority of languages which either rescued the nominative phonologically by adding the extra syllable, or which never adopted the two-syllable word constraint in the first place. For Garrett's theory, this development presents the following puzzle. If the two-syllable word minimum is an innovation, then it must have ousted the original monosyllabic *nouns* as well: so why didn't *their* nominatives get replaced by the ergative forms? Again we have a paradigmatic skewing of ergativity marking along pronoun/noun lines. To make sense of the different manifestations of it we must assume that the paradigmatic skewing has a natural basis.

The upshot is that there is not just one path to nominative/ergative syncretism. All diachronic roads lead to the same synchronic Rome: ergative case is morphologically unmarked in high-D nominals. Far from explaining this syncretism pattern, the various changes themselves require a motivation for the pattern as part of their explanation. The "invisible hand" of historical evolution nudges morphological systems towards certain optimal states, and part of the job of morphological theory is to say what those states are. One of the most general principles of optimization in morphology is economy, and a special case of economy is the omission of predictable information.

<sup>7</sup>This development also took place in Hindi, where however the Ergative forms were subsequently renewed by the addition of the suffix *-ne*.

A fourth objection to the notion that split ergativity patterns are side-effects of the historical change from instrumental case to ergative case is that it predicts the wrong split. Any pragmatic restriction on instrumental case would have to do with animacy, so that ergative case in its pristine, native state should lack an ending in animate nouns. But in the kind of system more commonly attested, ergative case is unmarked in pronouns, or in some subset of pronouns. Garrett takes this to be the result of subsequent analogical generalization. Conversely, lack of ergative marking in inanimate third person pronouns is unexpected on his view, and would require additional assumptions, perhaps analogy in the other direction within the pronominal system. But why do the putative analogical changes go precisely along the D-scale? Garrett's appeal to morphological differences as a barrier to spread from nouns to pronouns, but that leaves unexplained the pronoun-internal hierarchy  $1 > 2 > 3$  and the Singular  $>$  Plural asymmetry found in both nouns and pronouns. Indeed, there is reason to believe that the "animacy" hierarchy in reality reflects a scale of topic-worthiness, (individuation, saliency, referentiality, etc.). But it is hard to see why this scale would have any pragmatic connection to the use of instrumental case.

This in no way detracts from Garrett's account of the origin of split ergativity in Anatolian, which is perfectly convincing, and still less from his methodological plea for a historical perspective in typological study. But this very historical perspective requires the appropriate theoretical underpinnings. Historical mechanisms by themselves cannot explain why languages undergo the particular kinds of reanalyses that result in split ergativity but not other, a priori equally imaginable kinds of reanalyses. What is needed in addition is a principle of language design, of which the hierarchy in (29) is one special manifestation.

Summarizing the discussion so far, the hierarchy (29) is a linguistic universal and SHOULD be expressed in the synchronic theory of grammar because:

- (38)
- a. The hierarchy is inviolable.
  - b. There are multiple sources of split ergative case marking.
  - c. The hierarchy is a pathway of change.
  - d. The hierarchy must be encoded in the grammar because it intersects with other hierarchies (notably definiteness) and because it plays a role in the distribution of other morphological categories (notably number and agreement).

If the hierarchy (29) is genuinely part of UG, it might be expected to play a role in other aspects of grammar than case marking. This proves to be the case. The marking of number morphology reportedly follows the same hierarchy exactly:

- (39) "The singular-plural distinction in a given language must affect a top segment of the Animacy Hierarchy" (Corbett 2000, Ch. 3,4).

A subtler manifestation of the hierarchy can be seen even in English. Anttila & Fong 2003 establish the following *Pronoun generalization*:

- (40)
- a. Pronouns are preferred in the Specifier position and dispreferred in the Complement position.
  - b. Non-pronouns show the reverse pattern.

See the following contrasts:

- (41)
- |                     |                         |
|---------------------|-------------------------|
| Its removal         | ?The removal of it      |
| ?The tree's removal | The removal of the tree |

It has also been argued that preference for adjectival possessors follows the hierarchy (29) exactly (Koptjevskaja-Tamm).

Still more strikingly, *agreement* follows the hierarchy (29) exactly. The generalization is that it is more important to agree with 1.person subjects than with 2.person subjects and more important to agree with 2.person subjects than with 3.person subjects. Shown by conjunction (*ego et tu sumus* etc.), and various types of agreement systems (including, but not restricted to, inverse systems).

These additional generalizations are quite interesting because they pose a problem not only for purely historical accounts of split ergative case marking, but for functional accounts. Recall that the functional explanation for why (29) governs split case marking is that 1.person subjects, 2.person subjects etc. occur more frequently as subjects and hence are left unmarked for economy reasons.

It is completely mysterious why economy would demand the same hierarchy for number marking, or why the preference for specifier position would follow the same hierarchy. As for agreement, the facts are the exact opposite of what the functional economy-based explanation would predict. It turns out to be *more* important, not *less* important, to agree with the types of arguments which (according to the functional hypothesis) are most commonly used as subjects.

These data suggest that there may, instead, be a *structural* basis for the hierarchy. There are some indications that nominative vs. ergative case marking is structurally determined by the DP/NP divide: ergative case is assigned to N but not to D. The structural link between case, number, and agreement would then be the categorial distinction between nouns and pronouns/determiners.

Here are two suggestive examples of the predicted skewing. In Old Georgian, ergative case was incompatible with morphological definiteness marking, including both number and specificity (Boeder 1979:448). This follows simply if we assume that ergative case was assigned to NPs but not to DPs.

(42)

	Non-definite	Definite	
		Sing.	Plural
Nom.	∅	-i	-ni
Dat.	-s	-sa	-ta
Erg		-man	

In Koryak, a paleo-Siberian language, ergative is incompatible with morphological definiteness marking; this grammatical restriction is reflected in the distribution of the ergative marker *-a*. Instead of ergative case, DPs are assigned instrumental case.

(43)

	Nonhuman	Human	
		Indef.stem	Def.stem
Instr.	-a	—	—
Erg.	-a	-a	-k
Loc.	-k	-k	-k

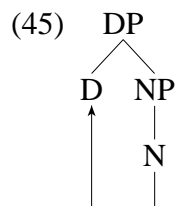
The table also shows that (the Instrumental use of) instrumental case is restricted to nouns denoting nonhumans (obviously a pragmatic restriction).

In order to account for the hierarchy in (29), we can assume that DP can be headed not only by determiners and pronouns, but also, in some languages, by high-D nominals. There is independent

evidence that in some languages proper nouns head DPs. Longobardi (1994, 2001) points out that kin terms and certain inalienably possessed nouns optionally have the distribution of determiners in Italian:

- (44) a. Noi ricchi.                      We rich. (\*Rich we.)  
      b. La mia mamma.                La mia tavola.  
      c. Mamma mia.                    \*Tavola mia.

Longobardi accounts for these data by head-to-head raising from NP to DP.



This process, extended down the hierarchy in (29), will account for the observed “animacy” effects on the marking of case and number and on agreement.

## Appendix

A formal justification of the points made in the last section of this paper comes from the case theory presented in Kiparsky 1997, 2001. The following is a brief summary of that work.

The prejudice that ergativity is marked or unnatural has deep roots in the descriptive and theoretical literature on case, from the earliest analyses that depicted it as a passive construction down to more recent efforts in various syntactic frameworks. My argument begins by establishing its normality. The ratio of ergative to non-ergative case systems in languages of the world is about 2:3 (Nichols, Siewierska). An often repeated claim is that ergative case systems, unlike accusative case systems, are always ‘split’. This is simply false (e.g. Basque is 100% morphologically ergative). Ergative case fits into the inventory of structural case. In fact, if such a case didn’t exist, it would have to be excluded by stipulation.

Semantic and its articulation by abstract Case features do the work that is usually assigned to configurational properties at d-structure. Abstract Case is defined by the positive values [+H(ighest)R(ole)], [+L(owest) R(ole)], which are assigned to the Th-roles according to their relative depth in the predicate’s semantic decomposition.

- (46) a. [+HR] is assigned to the highest role.  
b. [+LR] is assigned to the lowest role.

For example, (46), assigns the three ordered Th-roles of the verb *show* in [??a] (shower, showee, thing shown) the abstract Case features in [47].

- (47)  $\left[ \begin{smallmatrix} \lambda z \\ [+LR] \end{smallmatrix} \right] \left[ \begin{smallmatrix} \lambda y \\ \end{smallmatrix} \right] \left[ \begin{smallmatrix} \lambda x \\ [+HR] \end{smallmatrix} \right] [x \text{ CAUSE } [CAN [y \text{ SEE } z ] ] ]$

The effect of [46] is that the sole role of an intransitive verb is assigned both [+HR] and [+LR], and that the middle role of a three-place predicate is assigned neither [+HR] nor [+LR]. The result is an inventory of four abstract structural Cases; “A”, “S”, “O”, “D”:

- (48) Abstract structural Cases (grammatical relations):  
a. S: [+H(ighest)R(ole),+L(owest)R(ole)]  
b. O: [-HR, +LR]  
c. A: [+HR, -LR]  
d. D: [-HR, -LR]

Th-roles are assigned to arguments bearing morphosyntactic cases under unification.

- (49) Morphosyntactic structural cases:  
a. [ ]: nominative (including “absolutive”)  
b. [-HR]: accusative  
c. [-LR]: ergative, genitive  
d. [-HR,-LR]: dative

Because the case features are intrinsically relational, there can be at most one [+HR] role and at most one [+LR] role assigned to a given Semantic Form.

- (50) *Uniqueness*: A predicate has at most one [+HR] role and at most one [+LR] role.

Uniqueness is thus a definitional property of the features. A representation which violates it is ill-formed, in the same sense as, for example, a constituent with two heads or a feature matrix with incompatible feature specifications is ill-formed. Lexical operations must preserve Uniqueness. For example, when causative morphology supplies a new [+HR] Th-role, then the causee of the derived predicate must lose its [+HR] feature.

On the other hand, a predicate may have no [+HR] role or [+LR] role at all, if it projects no Th-roles into the syntax (i.e. impersonal verbs). And it can have than one [-HR] and [-LR] role, or none. The

Uniqueness condition (50) and the asymmetry of the positive and negative feature specifications will play a role in sections ?? and ?? below.

Abstract case features are assigned configurationally to the Th-roles in a predicate’s Semantic Form. They cannot be idiosyncratic lexical properties of specific Th-roles, and their assignment cannot be specific to particular predicates. This means that the abstract Case feature [+HR] always defines the highest syntactically visible Th-role of a predicate, its “subject”. Given the previously introduced assumption that Th-hierarchy reflects semantic depth, it follows that Th-role reversals, such as a verb “kill” with victim as subject and killer as object, are excluded in principle from any language.<sup>8</sup> Another corollary is that “quirky case” cannot be a lexical association of abstract case, but the idiosyncrasy must rather be accounted for at the level of morphosyntactic case. In section ?? provide empirical evidence in favor of this idea.

At the level of abstract Case, the proposed decomposition into features has two principal advantages.

The first advantage of the feature decomposition is that it makes it possible to individuate exactly the class of grammatical relations which play a role in syntactic constraints, such as binding, control, and parallelism in coordination. For example, the feature [+HR] picks out “A” and “S” in any language, irrespective of its case system, and thus universally defines the relation of grammatical subject, or “external argument”.

(51) *Def:* An argument/Th-role pair bearing the feature [+HR] is a *grammatical subject*.

Note that Uniqueness excludes the possibility that a single predicate might have multiple subjects.<sup>9</sup>

Categories requiring negative features are syntactically accessible in so far as those features are assigned by the appropriate licensors. For example, transitive subjects acquire the feature specification [–LR] by unifying with a bearer of morphosyntactic ergative case. Consequently, transitive subjects (“A” as opposed to “S” and “O”) can be singled out by the feature bundle [+HR,–LR] in ergative languages, and only in them. This point will be developed in section ??.

Secondly, the features provide the appropriate representation on which valency-changing operations are defined. I assume that these operations are triggered by verb morphology in the lexicon. In particular:

- (52) a. Passive: demotes [+HR].  
 b. Antipassive: demotes [–HR].  
 c. Causative: adds/promotes [+HR].  
 d. Applicative: adds/promotes [–HR].

	ditransitive			transitive		intransitive
Th-roles:	$\lambda x$	$\lambda y$	$\lambda z$	$\lambda x$	$\lambda y$	$\lambda x$
Abstract Case:	[+HR]	[ ]	[+LR]	[+HR]	[+LR]	[ +HR +LR]
ERG/NOM (Ngiyambaa)	[–LR] (ERG)	[(-LR)] (NOM)	[(+HR)] (NOM)	[–LR] (ERG)	[ ] (NOM)	[ ] (NOM)
ERG/ACC/NOM (Wangkumara)	[–LR] (ERG)	[–LR] (ACC)	[(+LR)] (NOM)	[–LR] (ERG)	[ ] (NOM)	[ ] (NOM)

The system generalizes correctly to four-place predicates (Donohue 2004).

<sup>8</sup>This means, for example, that inverse constructions are not passives (contra Perlmutter and Rhodes’ 1988 treatment of Ojibwa but in agreement with Dahlstrom 1985/1991). Some published treatments of psych-verbs are also inconsistent with this assumption. See section ?? below for further discussion.

<sup>9</sup>Again there are partly unresolved empirical issues at stake. There are at least two types of cases where linguists have been tempted to speak of multiple subjects. One is probably to be analyzed as a multiple topic construction, e.g. Korean (refs.); another consists of derived predicates such as causatives, where the embedded subject (the “causee”) sometimes has “subject properties”. These are cases of multiple predicates, for which we provide layered representations, which permit reference to the abstract Case features of basic verbs to be visible in their derivatives, as discussed in section ??.

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