# 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 main stimulus for incorporating historical linguistics into generative theorizing.

A widely shared assumption is that certain mutations occur in the transmission of language. Specifically, they occur when aspects of grammars based on incomplete data, or outputs of such grammars, can be retained from earlier stages of acquisition and become incorporated into the final system. This notion of "imperfect learning" has provided the basis for one 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). It is also commonly assumed in investigations of syntactic change. The theory of acquisition thereby becomes a crucial link between synchronic and diachronic linguistics.

The specific implementation of this approach will depend on the model of grammatical description that is adopted. Syntactic change, for example, has been treated as parameter-resetting (Lightfoot 1991), as grammar replacement (Kroch 1989), and as constraint reranking (OT, recently especially in its stochastic variety, Jäger & Rosenbach 2003, Clark 2004). Each comes with different commitments 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. 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). In a different framework, I have argued that structural position and inflectional morphology are alternative argument licensers, from which I derive, among other consequences, the Sapir/Jespersen generalization that loss of inflectional morphology entails fixed order of direct nominal arguments (Kiparsky 1997).

The leading idea behind this work, that properties of language change might be explained by the way language is acquired and structured in the mind, is of course by no means original to generative grammar. The neogrammarians, for example, had recognized the pervasive role of analogy as a regularizing force in change as a manifestation of the mechanism that underlies the
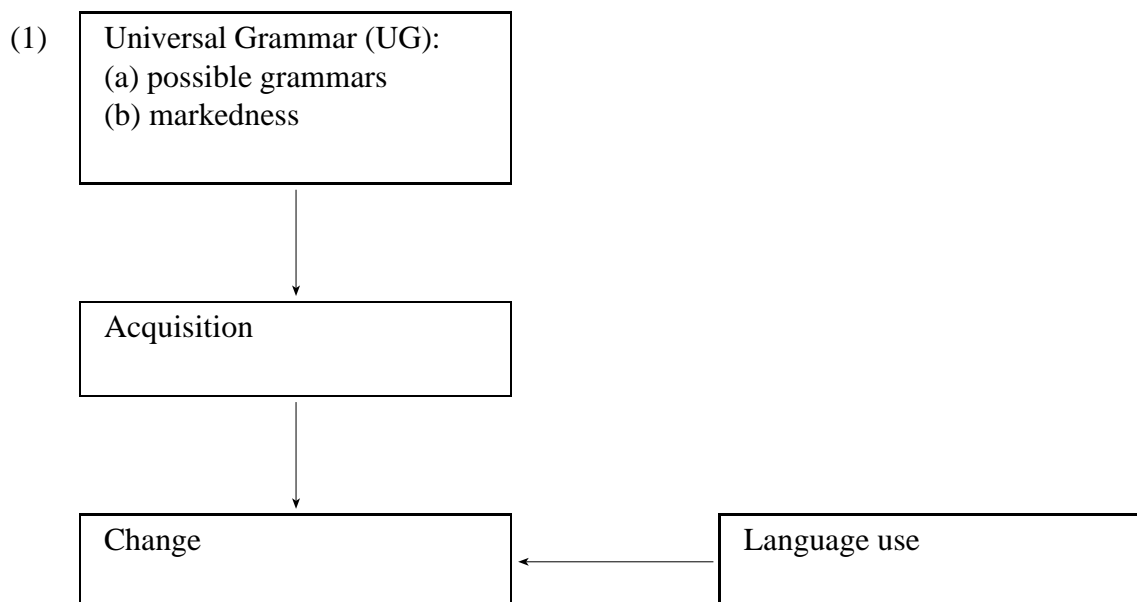
normal acquisition and creative use of language. The structuralists on their part sought to derive the empirical generalizations about langage change discovered by the neogrammarians from the design features of language. Indeed, the very origins of structuralism lie precisely in this attempt to ground historical linguistics in a new understanding of the language faculty.

One explanatory connection between linguistic change and the organization of language that emerged in this first round of structuralist theorizing was that language is a network of syntagmatic and paradigmatic relations which define the tracks of potential analogical changes. First articulated by the neogrammarian Hermann Paul, its larger theoretical consequences were worked out by linguists such as as Kruszewski, Baudouin de Courtenay, de Saussure, and later by Jakobson.

A second major idea that emerged at this time was that the regularity and exceptionlessness of sound change discoverd by the neogrammarians is based on the independence of phonology from morphology, syntax, and semantics. In Saussure's formulation, the reason sound change is regular is that the link between expression and meaning constituting the sign is arbitrary. Bloomfield's version of the explanation is 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 (read the arrows as "explains" or "constrains"):

(1)

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┌─────────────────────────────┐
│ Universal Grammar (UG):      │
│ (a) possible grammars        │
│ (b) markedness               │
└─────────────────────────────┘
              │
              ▼
┌─────────────────────────────┐
│ Acquisition                  │
└─────────────────────────────┘
              │
              ▼
┌─────────────────────────────┐        ┌─────────────────────────────┐
│ Change                       │◄───────│ Language use                 │
└─────────────────────────────┘        └─────────────────────────────┘
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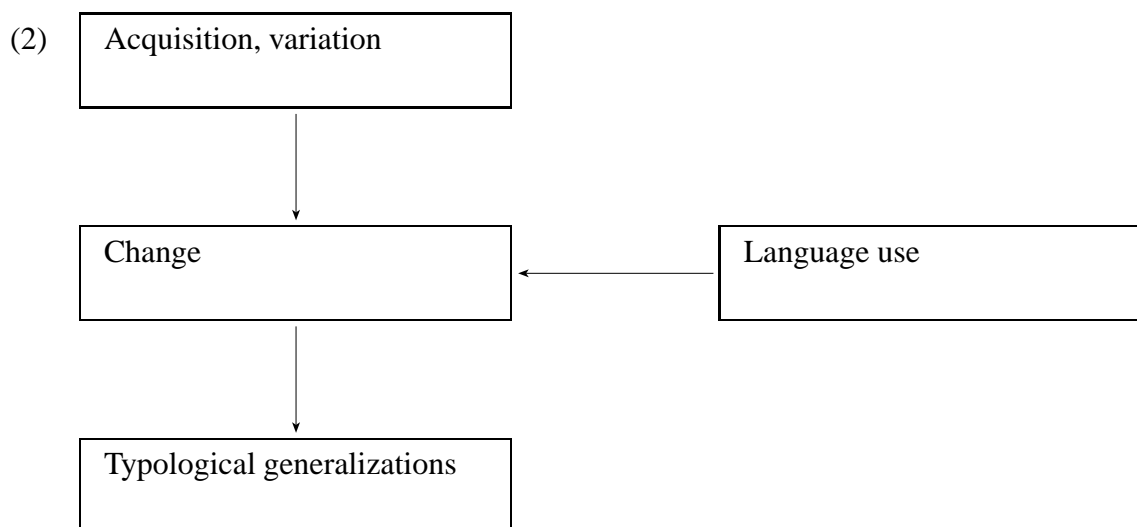
For the structuralists, UG (they never called it that, of course) tended to be very simple, and in principle derivable from a few quite general relations. Bloomfield and Saussure take distinctness as the basic relation in language, and connect it to the property of arbitrariness (Bloomfield

1933:144).[1] When seriously explored in descriptive practice by post-Bloomfieldian structuralists, this minimalistic program based on distinctness turned out to be problematic, as became clear first in phonology and then in morphology and syntax. Generative grammar therefore ended up positing a rather richly structured language faculty as an innate endowment.

This research program does not foreclose functional explanations for language change. Uncontroversially, they influence change at the level of language use. The generative program opens up the possibility that they might have become biologized within UG itself, thereby constraining change also via acquisition. For example, we could speculate that evolutionary pressures might have caused the innate learning mechanism to favor grammars that optimize perception, production, and/or stable transmission in certain ways. A language designed in modular fashion, with different levels of representation subject to their own constraints, may well be the most efficient for this combination of tasks.

## 1.2  Change explains structure

Seemingly at odds with the paradigm in (1) is an older, pre-structuralist idea which is recently regaining popularity. It views the direction of explanation as going 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).

(2)

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┌─────────────────────────┐
│  Acquisition, variation  │
└─────────────────────────┘
             │
             ▼
┌─────────────────────────┐        ┌─────────────────────────┐
│  Change                  │◄───────│  Language use           │
└─────────────────────────┘        └─────────────────────────┘
             │
             ▼
┌─────────────────────────┐
│  Typological generalizations │
└─────────────────────────┘
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In its more conservative forms, this program sees historical explanation as a kind of supplement or corrective to the formal theory of grammar. Most commonly, it is 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 even anomalous structures which fall outside the remit of UG.

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[1]As an example, here is Bloomfield's definition of *part of speech* (Bloomfield 1926, Def. 38): "The maximum word-classes of a language are the *parts of speech* of that language". To apply the definition, you have to go back to the definitions of *maximum, language,* and *word-class*. A *maximum* X is an X which is not part of a larger X (Def. 26). A *language* is the totality of utterances that can be made in a speech-community (Def. 4, with Defs. 1 and 3, and Assumption 1). A *word-class* is a *form-class* of *words* (Def. 37). A *form-class* is the set of forms having the same functions (Def. 33). A *form* is the set of vocal features common to same or partly same utterances (Def. 6). *Same* is that which is alike (Def. 5). A *function* is the positions in which a form occurs (Def. 32). A *position* is an ordered unit in a construction (Def. 29). A *construction* is a recurrent same of order (Def. 23, with Ass. 8). A *word* is a minimum free form (Def. 11). A *free* form is a form which may be an utterance (Def. 10). And so on, all the way down.

Such is, for example, a common view of split ergative case systems, which have no straightforward analysis in GB and its successor theories (see section 3 below). Or, conversely, it is 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).

In its more radical form, the 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-categorial 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 fact that genitives and relatives are likely candidates for reanalysis as adjectives is itself a puzzle, not something self-explanatory. One plausible answer is based precisely on their common status as nominal specifiers.

## 1.3   The program

These opposing considerations lead to a research program which integrates historical and synchronic linguistics by demarcating in a principled way the explanatory role of each. 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 originate. 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, as mentioned above). Any structural feature that is caused by change is inherently unstable (vulnerable, as Saussure put it). It can be washed out by other changes, or replaced with the opposite feature. Therefore recurrent structural features that are caused by recurrent patterns of change are TYPOLOGICAL GENERALIZATIONS but 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)

| *Universals* | *Typological generalizations* |
| --- | --- |
| 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

## 2.1   Simple and complex 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 *sebja*. 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 advern that means "self" (German *selbst*, Swedish *själv*, French *même*, Italian *stesso*, Russian *sam*).

A typological generalization discovered by Faltz 1977 and theoretically explored by Pica 1987 (sometims called "Pica's generalization") 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 howto 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 obviative 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.

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, because a principled connection between the shape of an anaphoric expression and its binding properties has proved elusive so far. At least on lexicalist assumptions, the syntax has no access to the morphological composition of words. The putative correlation of the morphology with the categorical distinction between heads and maximal projections is stipulative, and empirically suspect besides: 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).

Therefore, in terms of our criteria in (4), what we have here is not a true universal, but a typological generalization with a historical explanation, because:

(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.

## 2.2 Nominative anaphors

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 languages 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 dauṛcmuna       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    dauṛcmunebia    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$_i$ John-laa$_j$ kaḷavle     ki aapaṇ$_{i,*j}$ turangaat aahot
Jane-Erg  John-Acc  informed-3Sg  that self-Nom  prison-Loc  was-3Sg
'Jane$_i$ informed John$_j$ that self$_{i,*j}$ 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 seems to have had long-distance binding of anaphors, nominatives included, as long as it has had the reflexive *aapan*. For *aapan* is derived from Sanskrit *ātman* "soul, self", which there functioned as a reflexive (or as the equivalent of one) in any case form, includeing the nominative:

(13) khaḍgena 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   Split ergative case marking

## 3.1   The D-hierarchy

For a case where the evidence seems to point in the opposite direction, let us turn to a characteristic asymmetry of case marking and the so-called animacy hierarchy that determines it.

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 (14), where "A", "O" and "S" denote the subject and object of a transitive verb and the subject of an intransitive verb, respectively.
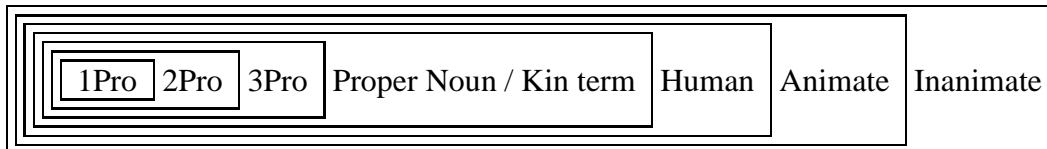
(14)

| | A | | S | | O | |
|---|---|---|---|---|---|---|
| Nouns | ERG | *-nggu, -ru* | NOM | *-Ø* | NOM | *-Ø* |
| Pronouns | NOM | *-Ø* | NOM | *-Ø* | ACC | *-na* |

When split ergativity is conditioned by the inherent category of the NP, the cases tend to be distributed according to the hierarchy in [15],[2] which I'll refer to as the D-hierarchy, since the more usual term "animacy hierarchy" is misleading.[3]

(15) The D-hierarchy



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:

(16) Djapu (Morphy 1983):

| | A | S | O |
|---|---|---|---|
| Non-human | *-thu* | | |
| Human | *-thu* | | *-nha* |
| Pronouns | | | *-nha* |

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

(17)

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

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[2]The hierarchy was extensively discussed by Kenneth Hale in lectures at M.I.T. in the late sixties; see Hale 1973. Silverstein 1976 and Dixon 1979 documented its application to ergative case systems.

[3]Wierzbicka 1981 shows that the hierarchy involves neither "animacy" nor "agentivity", which makes a direct functional explanation implausible. A category related to definiteness, such as individuation or "topic-worthiness" is a more likely candidate, as she points out and as I will also argue below. Let us note here that the hierarchy is actually not always so tidy. One somewhat widespread pattern groups kinship terms with the pronouns. Sometimes "Animates" are restricted to higher or intelligent animals, the others patterning with inanimates.

## 3.2 Is split ergativity an epiphenomenon of change?

It is often claimed, by 'formal' as well as 'functional' linguists, that ergative case marking, and specifically split ergativity of this type, is structurally or functionally unmotivated, but arises through understandable diachronic processes:

(18)   a. "NP split-ergative systems in fact have their striking synchronic features as a straight-forward 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).

Let us consider the arguments for this position, which are laid out in Garrett 1990. Garrett maintains that argues that the lack of ergative marking on the high end of the D-hierarchy neither has nor needs a synchronic explanation. It is only at an antecedent historical stage that the split case marking 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 *semantic/pragmatic* gap before the reanalysis to an *allomorphic* gap after the reanalysis. Actual instruments are restricted to Low-D nominals, simply because they *are* inanimate. (It does not matter for our ourposes whether this is a pragmatic restriction or an ontological one.) 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 nominals with no overt case marking* — a very different thing. The suffixlessless 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 conjoined noun phrases. The gap in ergative paradigms is thus a matter of allomorphy, not a gap in the distribution of the category of ergative case (and still less a matter of pragmatics, of course).

What is missing from the historical account, then, is the causal link between the putative prag-matically motivated gap in the distribution of the former instrumental case and the zero allomorph

that it supposedly leaves behind in the paradigm of the reanalyzed ergative case. In other reanalyses this kind of thing generally does not happen.[4] 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*).[5] 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.[6]

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

(19)  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 [17].

(20) Finnish "split accusativity": only pronouns get accusative case endings

    Koira    näh-tiin.    Häne-t näh-tiin.      (Finnish)
    Dog-**Nom** see-PassPast    He-**Acc** see-PassPast
    'The/a dog was seen.' 'He was seen.'

It also governs dative/accusative syncretism and ergative/dative syncretism, and for the same reason.

Finnish child language and certain dialects reportedly extend accusative marking further along the trajectory of (15), from pronouns to proper names and appellatives, e.g. *Lauri-t*, *isi-t* 'daddy'.

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[4]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.

[5]An example of this development is Estonian *-ga*, etymologically from *kansa-ssa* "in the crowd".

[6]A third example is the reanalysis of possessive suffixes as object markers of definite NPs that has taken place in some langages 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.

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 what the instrumental-to-ergative reanalysis scenario by itself can provide.

The third objection to Garrett's proposal 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 that are known to have 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 (= Lahanda, Masica 1991:252). The examples in [21] are from Siraiki (Bhatia 1983:181):

(21)  a. mãi axbaar       δiṭṭhii
         I    newspaper.fs see-pst.3fs
         'I saw the newspaper'

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

Since the Indo-Aryan instrumental case from which the ergative marker *ne* descends was at no stage restricted to inanimates, the high-D nominative/ergative syncretism in these languages must have arisen in some other way than by Garrett's scenario. It appears that it arose somply simply by ergative forms being extended to function as nominatives.[7] Analogous developments can be observed elsewhere. 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. In several of them, pronouns (and sometimes in addition a class of high-D nouns) lack overt endings in the ergative, e.g. Adyghe:

(22)        '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

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[7]This development also took place in Hindi, where however the original ergative forms were subsequently renewed by the addition of the suffix *-ne*.

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, and is not just an epiphenomenon of hiasory.

In sum, the third argument against Garrett's position is that there is not just one path to nominative/ergative syncretism. All diachronic roads lead to the same synchronic Rome, where ergative case is lacks a morphological mark 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.

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 of system most commonly attested, ergative case is unmarked in *pronouns*, or in first and second person pronouns. Garrett takes this to be the result of subsequent analogical generalization. Also, 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. In any case, 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 $1 > 2 > 3$.

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. The hierarchy in (15) must in some sense be part of the design of language.

Summarizing the discussion so far, the hierarchy (15) is a linguistic universal and SHOULD be expressed in the synchronic theory of grammar because:

(23)   a. The hierarchy is inviolable.

   b. There are multiple sources of split ergative case marking.

   c. The hierarchy is a pathway of analogical change.

d. The hierarchy is manifested spontaneously in child language.

e. 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).

## 3.3   Other manifestations of the D-hierarchy

If the hierarchy (15) 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. It interacts with other categories in an extremely revealing way.

The marking of NUMBER morphology reportedly follows the same hierarchy exactly:

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

Thus, some languages distinguish singular from plural only in pronouns, or only in first and second person pronouns.

Also, any morphological marking of DEFINITENESS follows the hierarchy, for categories that distinguish definiteness (this excludes personal pronouns, which are inherently definite). Thus, a language may formally distinguish between definite and indefinite humans but not nonhumans (for an example, see Old Georgian below), or between definite and indefinite animates humans but not inanimates.

Still more strikingly, AGREEMENT follows the hierarchy (15) 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. This is shown by conjunction (*ego et tu sumus* etc.), and by various types of agreement systems (including, but not restricted to, inverse systems).

It has also been argued that preference for adjectival POSSESSORS follows the hierarchy (15) exactly (Koptjevskaja-Tamm 1993). An English example is the preference for -*s* genitives of pronous and *of* genitives of nouns (Anttila & Fong 2003):

(25)   Its removal         ?The removal of it
       ?The tree's removal    The removal of the tree

The categories of number, definiteness, agreement, and possession are all related to the DETERMINER SYSTEM. Specifically, if these features are overtly marked in a language they are marked at least on its pronouns (and on its articles, if it has them). For example, any feature which participates in subject or object agreement in a give language is marked on the pronoun system in that language.

These additional generalizations involving the relationship of the D-hierarchy in (15) to the determiner system are interesting because they pose a problem not only for purely historical accounts of split ergative case marking, such as Garrett's explanation based on the derivation of ergative case from instrumental case, but also for functional accounts.

Recall that the functional explanation for why (15) governs split case marking is that high-D nominals occur more frequently as subjects and hence are left unmarked for economy reasons. It is 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 mark subject agreement with the types of arguments which (according to the functional hypothesis) are most commonly used as subjects.

## 3.4 The basis of the D-hierarchy: a proposal

These data suggest that there may be a *structural* basis for the hierarchy that covers *all* its manifestations. It appears that ergative case marking may be incompatible with certain features of the determiner system. In the simplest case, the structural link between the marking of case, number, possession, definiteness, and agreement is just the categorial distinction between nouns and pronouns/determiners.

The posited link between ergative morphology and the determiner system predicts that there should be languages where both ergative case and determiner features are overtly marked but may not co-occur. In what follows I present four examples which confirm this prediction.

In Old Georgian, ergative case marking was incompatible with morphological definiteness marking, including both number and specificity (Boeder 1979:448).

(26)

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

This correlation between case marking and definiteness suggests that ergative case was assigned to NPs but not to DPs.

In Koryak, a paleo-Siberian language, ergative case marking is incompatible with morphological definiteness marking, which only occurs on human nouns. In (27), the distribution of the ergative marker *-a* can be seen reflects this grammatical restriction. Under these circumstances, ergative case is replaced by locative case, instrumental being unavailable because it is restricted to inanimate nouns. Instead of ergative case, DPs are assigned instrumental case.

(27)

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

If articles and pronouns belong to the same category D, and what is directly relevant to case assignment is category membership and feature content rather than semantics, then we would expect articles to group with the pronouns with respect to split ergativity. Ergative languages usually do not have definite articles, but Arrernte (Australia) is a rare case which does. An article *re* (identical with the third person pronoun) is postposed to NPs in a definitizing function. As expected, a transitive subject then receives an ergative case ending on the nominal but no case ending on the article (Andrews n.d.):

(28)  a. kngwelye re(*-rle) ker  arlkwe-ke
      dog        the(*-Erg) meat eat-Past
      'The dog ate the meat.'

   b. artwe re*(-nhe) kngwelye-le uthwe-ke
      man    the-Acc    dog-Erg      bite-Past
      'The dog bit the man.'

My final example comes from Wargamay, also an Australian language. According to Dixon 1981:39-40, Wargamay regularly marks number only on pronouns which refer to humans. As Corbett 2000:55 summarizes:

> "The first and second persons, singular, dual, and plural, and the third dual and plural are 'strictly specified for number' and are available only for reference to humans (and occasionally tame dogs). The form filling the third singular slot can range over all persons and all numbers (it can have non-human well as human reference) but its basic sense is third person singular."

Nouns can be specified for number by reduplication, but this is restricted and not usual in the language; probably noun pluralization is not an inflectional process but a derivational one.

The case marking pattern is closely correlated with number marking. Nouns and the third singular pronoun (precisely the items which are inherently unspecified for number) are marked for ergative case only. The bare form without an overt case affix functions as an object and as a subject of intransitive verbs. Dual and plural pronouns in the first and second person are marked for accusative case only. The bare form without an absolutive is used in subject function. Finally, singular pronouns (which have no overt mark for number) have distinct ergative and accusative forms.

The generalization for Wargamay case assignment appears to be this:

(29)  a. Elements that are inflected for number do not get ergative case endings.

   b. Elements that cannot be inflected for number do not get accusative case endings.

## 3.5   Extending the account

The assumption that ergative case is assigned to Ns and NPs, and not to Ds and DPs, suffices to account for what is by far the most common type of split ergative marking, which is between nouns and pronouns, on the usual assumption that pronouns and articles are members of the category D (Postal 1966, and much later work).

To explain the full hierarchy in (15), we can assume that DP can be headed not only by determiners and pronouns, but also, in some languages, by high-D nominals. In some languages proper nouns and kin terms clearly function as Ds, either nonprojecting or heading DPs. The evidence comes from inflectional morphology and from syntax. The morphological evidence comes from shared inflections between proper/kin nouns and pronouns (as in the above mentioned Finnish child language data). The syntactic evidence is a recurrent pattern of shared positional properties between proper/kin nouns and pronouns. Longobardi (1994, 2001) points out that kin terms and certain inalienably possessed nouns optionally have the distribution of determiners in Italian:

(30)  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, and argues that the same pattern holds in a number of other languages.

(31)
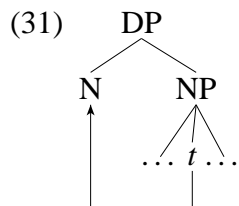


Note however that the fundamental shared property on my proposal involves category membership; this may be reflected in syntactic positioning as Longobardi argues, but does not have to be.

Let us suppose that this process extends down the D-hierarchy in (15). This would not only account for the observed "animacy" effects on the marking of case, but unify them with parallel asymmetries involving number and on agreement.

At the same time, it gives us a new way to understand the apparent exceptions to the hierarchy in (15).[8] In Arrernte, the first person singular pronoun, unlike all other pronouns of the language, receives the ergative case ending, like nouns (Andrews, *ibid.*). This is surprising since first person singular should be the very apex of the hierarchy; in Dhalandji, ergative/nominative syncretism is reportedly restricted to just this pronoun (Austin 1981). The key observation is the familiar one that some "pronouns" are really just nouns. There is no reason to suppose that the translational equivalents of the English personal pronouns in some other language are actually morphologically and syntactically pronouns in that language, any more than the translational equivalents of auxiliaries or prepositions are necessarily auxiliaries and prepositions. The apparent pronouns may very well be nouns or noun phrases, as in Japanese (even English has "yours truly" or "the undersigned"). Nouns and pronouns are respectively lexical words and function words and so should be distinguishable by morphological, syntactic, and perhaps even phonological criteria. I venture the guess that the first person singular of Arrernte is a noun. If this account for the exceptions turn out to be right, it would support the interpretation suggested here over the traditional approach of tying the hierarchy directly to meaning or reference, which predicts that syntactic and morphological category should play no role.

# 4  Coda neutralization

## 4.1  Final devoicing

Turning to phonology, let's begin with the robust phonological generalization that marked feature values tend to be suppressed in certain prosodic positions. Perhaps the best-known example of this process is CODA NEUTRALIZATION, the suppression of place and manner contrasts in syllable

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[8]For some examples, see Blake 1977, and Goddard 1982.

codas (or in word-final position), with the neutralized features typically taking their unmarked values. For concreteness let us consider the special case of devoicing of obstruents, as in German, most Slavic languages, Catalan, Turkish, Korean, and in a number of dialects of English.[9] As a sound change as well as synchronically. it seems to be irreversible, in that clear cases of the converse process of final voicing are not attested. 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. 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. It can be decomposed into two separate constraints. One says that onsets have at least as many place and manner contrasts as codas; which is really a special case of a family of constraints which differentiate between strong and weak positions. The second constraints says that neutralized features assume their unmarked value (voicelessness, in the case at hand). This constraint too is more general. When voicing contrasts are neutralized elsewhere, the same generalization seems to apply. (Of course, we have to set aside contextual effects such as voicing assimilation, which override the default value and on some analyses take a neutralized archiphoneme as input). The limiting case is context-free neutralization, i.e. the lack of a contrast in the phonemic syste, Here the generalization seems to be that languages that have no manner contrasts realize their stops as voiceless and unaspirated (Maddieson 1984:27).[10]

To say that the constraint is part of the design of language does not mean that it is is arbitrary or unmotivated. For example, it has been suggested that the reason certain feature distinctions are liable to be suppressed in codas is that those feature distinctions are perceptually less salient in those positions (Steriade 2004.)[11] as for why neutralization would favor unmarked feature values, the reason might be the greater economy of the relevant articulatory gestures. More effortful articulations would be used positions where a contrast must be marked.

The second possible answer locates the explanation in the diachronic plane, along the lines of Blevins (MS). Suppose 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. It could them be claimed 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 answers are in many ways similar, and must ultimately converge 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 arbitrary fact. It has no synchronic rationale, no 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. by having a final voicing process) would be not just

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[9]Australia, South Africa (Wissing and Zonneveld 1996). Many U.S. speakers, including the current president of the U.S.A., devoice word-final fricatives.

[10]This is *not* the same as saying that languages must have at least a voiceless unaspirated series of stops. In fact, they don't. Approximately 8% of languages in Maddieson's sample (1984:27) are listed as lacking a series of plain voiceless stops; this includes English, where the voiceless series is enhanced by aspiration (as predicted by dispersion theory, see Steriade 2004, Flemming 2003). Of course not all contexts in English have aspiration; it seems that Maddieson's analyses of phonological systems take something like the most common allophone of a phoneme as basic.

[11]Steriade however argues that syllable structure is not the correct characterization of the relevant positions. This is an important issue but I will have to set it aside here.

historically unattainable, but synchronically complex, in the sense of being hard to learn, or hard to use, or both. Versions of OT phonology that posit that all constraints are universal and do not allow contradictory constraints even make the very strong claim that such systems are impossible.

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, consider a language with a single series of obstruents with a gemination (or aspiration) contrast, neutralized in final position by degemination (or deaspiration, as the case may be). Let a lenition process then transpose the geminate/singleton (or aspiration) contrast into a voiceless/voiced contrast. Both these changes are common enough (the Romance languages offer many examples). With both sound changes taking place in sequence, the result would be final voicing:

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

|  |  | Medial |  |  |  | Final |  |
|---|---|---|---|---|---|---|---|
| Stage 1: | tatta | : | tata | tat | : | *tatt | (final degemination, voicing not distinctive) |
| Stage 2: | tata | : | tada | tad | : | *tat | (lenition) |

The synchronic grammar at Stage 2 has a which neutralizes voiced and voiceless obstruents as *voiced*.

Many other potential scenarios should be capable of producing the same result. Both voicing and devoicing are possible sound changes, and together they can reverse voicing.[12] In a language with an ordinary final devoicing process, such voicing reversals could theoretically produce synchronic final voicing processes, but that seems not to happen.

In sum, final voicing *could* originate by everyday sound changes by a variety of plausible hypothetical scenarios. If it in fact never arises, *some constraint on the design of language must prevent it*.

## 4.2  Does Lezgian have final voicing?

Yu (2004) has suggested that there is at least one language that does have a syllable-final voicing process in its synchronic phonology, Lezgian (based on data in Haspelmath 1993). The synchronic situation is that there are four distinct series of stops, of which three are invariant and the fourth alternates between a long voiced stop in coda position and a plain voiceless stop in onset position. This alternating stop series occurs only before the main stress, which falls on the second or only syllable of the word, it never occcurs anywhere after the main stress.[13] The pattern is summarized in (33), where D = a voiced stop, D: = a long voiced stop, T' = an ejective, and T$^h$ = an aspirate.[14]

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[12]Classical Armenian (and modern Eastern Armenian) T and D correspond respectively to modern Western Armenian D and T$^h$, (e.g. *Tigran* vs. *Dikran*). Germanic /θ/ and /d/ correspond respectively to German /d/ and /t/ (e.g. *Tod* 'death', *tot* 'dead').

[13]"In a position following the stressed vowel, voiceless stops are always aspirated, never unaspirated, except [immediately after a voiceless obstruent, when they are always unaspirated] (Haspelmath 1993:47). Haspelmath (p. 59) cites the form [t'ʷek], which is inconsistent with the verbal rule just quoted; I assume it is a misprint for [t'ʷek'] or perhaps for [t'ʷek$^h$].

[14]There is also an alternation between onset T' and coda D:, which reduces to the fourth set by a process which spreads the ejective feature, viz. ∼ [T'VTV-] → [T'VT'V-] (alternating with [T'VD:-]).

(33)          \_\_\_V    V\_\_\_]$_\sigma$

|  | \_\_\_V | V\_\_\_]$_\sigma$ |
|---|---|---|
| /D/ | D | D |
| /T'/ | T' | T' |
| /Tʰ/ | Tʰ | Tʰ |
| /ʔ/ | T | D: |

There is, then, a four-way manner contrast in pretonic position, and a three-way manner contrast elsewhere. The the fourth stop series here labeled /ʔ/ (the one which is restricted to pretonic position) is a plain voiceless stop in onsets and a long voiced stop in codas (where it contrasts with the non-alternating short voiced stop). The question is what /ʔ/ is phonologically. Yu simply assumes without argument that /ʔ/ is /T/, and that /T/ becomes voiced and lengthened in coda position. But there is really no reason why it could not equally be /D:/ which is conversely shortened and devoiced in onset position. The moraic theory of length tells us that /D:/ is really /DD/), so this solution amounts to positing onset degemination and onset fortition for Lezgian, which are both eminently natural processes. The three- series stop system /D/ : /T'/ : /Tʰ/ posited by this analysis is identical to that of Lezgian's close relative Georgian (according to some analyses at least), and is not uncommon in the languages of the Caucasus (e.g. Kabardian), and for that matter in Native American languages (Klamath, Kwakw'ala, Yana, Acoma).

This alternative, then, is typologically unexceptionable. Unless it can be excluded on compelling language-internal grounds, there is no case for coda voicing in Lezgian. And that means that the proposed universal is exceptionless as far as known.

## 4.3    Further evidence for the universal

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 the sound change of coda devoicing. In Konni (northern Ghana), regressive voicing assimilation (cf. (34a)) is blocked just when it would give rise to a voiced coda, in which case an epenthetic vowel is inserted instead (as in (34b)) (Cahill 1999).

(34)    a. /tig-ka/ → *tikka* 'the village'

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

Similarly, in Meccan Arabic (McCarthy 2003), voicing assimilation (cf. (35a)) is blocked precisely when it would give rise to a voiced coda (as in (35b)), even though voiced codas are not in general prohibited in the language (see (35c)):

(35)    a. /mazku:r/ → *masku:r* 'mentioned'

b. /ʔakbar/ → *ʔakbar (*agbar)* 'older'

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

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 certain languages with strict CV syllable structure, final devoicing has been reported to apply spontaneously in when speakers attempt to pronounce

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

No argument is needed to show that coda neutralization is a process which must be encoded in the grammar. It functions as a productive rule or constraint in numerous languages, interacting with other rules/constraints and principles within their phonological systems.

Thus, coda neutralization would seem to satisfy the requirements for a true linguistic universal.

# 5   Stress/weight solidarity

Finally 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.

(36)  *a > e, o > i, u > ə.*

The relative sonority defined by (36) 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 is the stress/sonority solidarity generalization stated in (37).

(37)  Stress seeks heavy syllables and sonorous vowels, where sonority is defined by the scale (36).

De Lacy (2002a) extensively documents the generalization stated in (38), and argues that it is true of all universal hierarchies, at least in phonology.

(38)  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 (36) 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 (39).

(39)     • 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 > ə,* and

        • the final syllable is stressed if it is the only syllable with *a.*

The data in (40) show how stress is assigned in accord with (39).

(40)     a. *azádi* 'freedom'

        b. *ekóteɾ* '71'

        c. *pəddhə́ti* 'guide'

        d. *tájetər* 'recently' (*a > e*, attracts stress to the antepenult)

        e. *kójəldi* 'little cuckoo' (*o > ə*, attracts stress to the antepenult)

        f. *ʧɔpərá* 'girls' (*a >* other vowels, attracts stress to the final syllable)

De Lacy proposes for Gujarati a synchronic stress system which I informally summarize in (41):

(41)　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 (39) 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):

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

The validity of generalization (42) 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 (36), (37), and (38) are part of UG. Let us see what the criteria in (4) say.

The sonority hierarchy in (36) seems to be the same in all languages (this is the import of de Lacy's result in (38)). But there are natural types of sound change that *could* reverse it. For example, a sound change [ɑ] > [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 *\*tɔ́jetər*, contrary to (36)-(38). The result would be precisely a stress system with a rule/coonstraint 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 appear to give rise to must be reanalyzed.

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 beteeen 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 (39) 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

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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) in noun inflection over the attested history of Finnish. He shows that it follows the course in (43).

(43) | *-i, -u* stems | *líntuin* | > | *líntujen* | 'birds' | 16th century |
| *-e, -o* stems | *péltoin* | > | *péltojen* | 'fields' | 19th century |
| *-a, -ä* stems | *ákkain* | > | *ákkojen* | 'old women' | 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 (43) 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.

# 6   Conclusion

An increasingly popular research program seeks the causes of typological generalizations in recurrent historical processes, or even claims that all principled explanations for universals reside in diachrony. Structural and generative grammar has more commonly pursued the reverse direction of explanation, which grounds the way language changes in its structural properties. The two programs can coexist without contradiction or circularity as long as we can make a principled separation between true universals, which constrain both synchronic grammars and language change, and typological generalizations, which are simply the results of typical paths of change.

The following criteria should converge to identify true universals. (1) Universals have no exceptions (for what does not arise by change cannot be subverted by it either). (2) Universals are process-independent. (3) Universals can be manifested in "emergence of the unmarked" effects. (4) Universals are embedded in grammars as constraints and can interact with other grammatical constraints. (5) Universals constitute pathways for analogical change.

Choosing as testing grounds Binding Theory and split ergativity in morphosyntax, and voicing neutralization and sonority in phonology, I argued that these criteria do converge rather cleanly in each case. Pica's Generalization about the binding properties of simple vs. complex anaphors and Everaert's generalization that there are no nominative anaphors are not universals, but diachronically explicable typological generalizations. The D-hierarchy that governs split case assignment, number marking, and agreement is a universal, as is the direction of voicing neutralization and the sonority hierarchy in phonology.

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