Optimality and diachronic adaptation*

Martin Haspelmath

Max-Planck-Institut für evolutionäre Anthropologie, Inselstr. 22, 04103 Leipzig, haspelmath@eva.mpg.de

Abstract

In this programmatic paper, I argue that the universal constraints of Optimality Theory (OT) and the functional explanations of functionalists need to be complemented by a theory of diachronic adaptation. OT constraints are traditionally stipulated as part of Universal Grammar, but this misses the generalization that the grammatical constraints normally correspond to constraints on language use. As in biology, observed adaptive patterns in language can be explained through diachronic evolutionary processes, as the unintended cumulative outcome of numerous individual intentional actions. The theory of diachronic adaptation also provides a solution to the teleology problem, which has often been used as an argument against functional explanations. Finally, I argue against the view that the grammatical constraints could be due to accident, and I conclude that an explanatory theory of grammatical structure needs a theory of adaptation.

1. Preferences in competition: an old and new concept

There is a long tradition in theoretical linguistics which holds that structural patterns of grammar are determined by highly general preferences or constraints that may come into conflict with each other. Gabelentz (1901: 256) was very clear about the tension between the "striving for ease" (*Bequemlichkeitsstreben*) and the "striving for clarity" (*Deutlichkeitsstreben*). The neogrammarians were primarily concerned with the conflict between phonological tendencies (leading to exceptionless sound changes) and the tendency toward morphological

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analogy. Havers (1931 : 191 ff) discusses in great detail the interaction of various general "conditions and forces" in syntax.

With the advent of structuralism and its rigid synchrony/diachrony separation, this kind of thinking went out of fashion, as the focus was now on explicit and elegant descriptions of individual languages, rather than on highly general (if often vague) explanatory principles. But after several decades of abstention, linguists again began to become interested in highly general principles; and since principles can be formulated in a more general way if they are violable, this meant that the idea of conflicting preferences resurfaced. Within one tradition, such competing preferences were called *naturalness principles in conflict* (e. g. Dressler 1977 : 13, Dressler et al. 1987 : 7, 93); in another, *competing motivations* (Haiman 1983 : 812, Du Bois 1985, Croft 1990: §7.4). Langacker (1977 : 102) used the term *optimality*:

> "I believe we can isolate a number of broad categories of linguistic optimality. Languages will tend to change so as to maximize optimality in each of these categories... The tendencies toward these various types of optimality will often conflict with one another."

In this line of thinking, it has always been assumed that the competing preferences are not just highly general, but in fact constitute universal properties, or design features of human language. Prince & Smolensky's (1993) formal framework of Optimality Theory (OT) uses this idea also in synchronic descriptions of grammatical structures (originally, in phonology, but later also in syntax) by introducing the notion of language-specific preference ranking (or "constraint ranking", in their terminology).[1] Much work in Optimality Theory has shown that the availability of violable constraints often yields more elegant and appealing descriptions than accounts in terms of inviolable rules.

This general framework has been enormously successful and popular, perhaps not only because it allows linguists to formulate much more general principles than were hitherto possible, but also because many of the constraints are intuitively plausible, and because the description in terms of the "best" and "worse" candidates often corresponds to our pretheoretical feelings. Consider, as a simple example, the distribution of the plural allomorphs /-z/, /-əz/, and /-s/ in English. This may be accounted for in the OT framework by postulating the four constraints SameVoice ("Sequences of obstruents within a syllable must agree for voicing"), OCP(Sibilant) ("Sequences of sibilants are prohibited within the word"), DepIO ("Insertion of segments is prohibited"), and

1 Ranking of naturalness principles has been widely discussed in Natural Morphology, but mainly in terms of universal ranking (e. g. Wheeler 1993) and type-specific ranking (e. g. Dressler 1985 a). Language-specific differences were attributed to other factors by these authors.

Table 1

	SAME VOICE	OCP(SIB)	DEPIO	IDENT (VOICE)
input: kæt-z				
kæt-z	*!			
kæt-əz			*!	
☞ kæt-s				*
input: bʊʃ-z				
bʊʃ-z	*!			
☞ bʊʃ-əz			*	
bʊʃ-s		*!		
input: stoʊn-z				
☞ stoʊn-z				
stoʊn-əz			*!	
stoʊn-s				*!

IDENTITY(VOICE) ("Input and output are identical for voicing") (cf. Gussenhoven & Jacobs 1998: 48–49). Assuming an underlying /-z/ for the plural *-s*, we get the constraint tableau in 1, where the three relevant cases *cat-s* [kæt-s], *bush-es* [bʊʃ-əz], and *stone-s* [stoʊn-z] are shown.

Only *stone-s* [stoʊn-z] shows no constraint violation at all. In *bush-es* [bʊʃ-əz], DEPIO (the constraint against epenthesis) is violated, but SAMEVOICE and OCP(SIBILANT) are ranked higher, so [bʊʃ-əz] is the optimal candidate. In *cat-s* [kæt-s], IDENTITY(VOICE) is violated, but again, the two competing candidates [kæt-z] and [kæt-əz] violate higher-ranked constraints. In informal OT parlance, [kæt-s] is "better" than [kæt-z] and [kæt-əz], and this quasi-technical terminology coincides nicely with our feeling that indeed [kæt-s] "sounds better" than its competitors in that it is easier to pronounce.

However, this intuitive coincidence between "good" in the sense of "optimal with respect to OT constraints" and "good" in the sense of "good for the language user" has not been captured in mainstream versions of OT. I will argue in this paper that by capturing this correspondence between **grammatical optimality** and **user optimality**, we are able to reach a significantly higher level of explanatory adequacy.

2. Why are the constraints the way they are?

The OT framework does two things very well. On the one hand, it allows descriptions of language-specific facts that are more principled than those in the previous frameworks of generative linguistics. For instance, the constraint set in Tableau 1 is more general than phonological rules like "z → [-voice]/[-voice]__" and "z → əz/[+strident, +coronal]__", from whose formulation it is not immediately clear that they are by no means arbitrary.

On the other hand, the OT framework allows an elegant statement of typological options, called **factorial typology**. The typology of possible languages is given by the set of possible rankings of the constraints. Consider a simple example, again from phonology (Prince & Smolensky 1993: §6.1): The two widely applicable syllable structure constraints ONSET ("A syllable must have an onset") and NOCODA ("A syllable must not have a coda"), together with the constraint FAITHFULNESS ("The output must not contain fewer or more segments than the input") allow three types of languages, depending on their mutual ranking (X ≫ Y means 'X is ranked higher than Y'):

(1)	ONSET ≫ FAITH	FAITH ≫ ONSET
NOCODA ≫ FAITH	CV (e.g. Hua)	(C)V (e.g. Cayuvava)
FAITH ≫ NOCODA	CV(C) (e.g. Tharrkari)	(C)V(C) (e.g. Mokilese)

However, this cannot be the whole story yet. We must ask further: Why are there no constraints such as CODA or NOONSET, which are opposite to NOCODA and ONSET? Nothing in standard OT prohibits these constraints, so if it is true (as it seems to be) that they do not exist, this can only be achieved by stipulation. Such an account may be satisfactory for linguists who limit their goal to an elegant description of particular languages. But the theoretically minded linguist will be more ambitious and ask a further why question: **Why are the constraints the way they are?**

It could of course turn out that this question is unanswerable, and that the constraints are not more than accidents of history. The usual assumption is that the OT constraints are innate, and it might be that they arose as an accidental side-effect of some adaptive modification of the brain (cf. §7 below for further discussion of this possibility). But there seems to be a widespread feeling among OT practitioners that this is not the whole story. Otherwise there would be no need to justify new constraints with reference to non-distributional evidence. But this is what one commonly finds. For instance, Bresnan (1997) postulates a constraint PROAGR ("Pronominals have the referentially classificatory properties of agreement") and states: "The functional motivation for the present constraint could be that pronouns ... bear classificatory features to aid in reference tracking, which would reduce the search space of possibilities introduced by completely unrestricted variable reference." Similarly, Casali (1997: 500) justifies his constraint MAXLEX, which he uses to express the fact that vowel elision in hiatus contexts typically does not affect roots and content words, by noting that it "arises from a more general functional motivation, a preference for maintaining phonological material belonging to elements that typically encode greater semantic content". And Morelli (1998: 7) introduces the constraint *STOP-OBSTRUENT ("A tautosyllabic sequence containing a stop followed by any obstruent is disallowed") and states: "It is justified both phonetically and phonologically. Phonetically, it reflects the preference for stops to be released into more sonorous segments ..."

Strictly speaking, such justifications are irrelevant in a theory that assumes innate constraints. But the fact that they are mentioned by OT practitioners indicates that they have the intuition that the constraints are not arbitrary and are in principle susceptible of (or even in need of) further explanation. However, to my knowledge nobody has so far made an attempt to explain OT constraints in a systematic fashion. In the next two sections, we will see what such an explanation might look like.

3. User optimality and adaptation

What the justifications of constraints by Bresnan, Casali, and Morelli have in common is that they portray the constraints as being good for speakers and hearers in one way or another, i. e. as exhibiting **user optimality** (to use the term introduced in § 1). To see this more clearly, in (2) I reformulate some of the constraints that we have seen so far in terms of the language users' needs:

(2) User optimality of grammatical constraints

name	grammatical constraint	corresponding user constraint
MAXLEX (Casali 1997: 501)	"Every input segment in a lexical word or morpheme must have a correspon-ding segment in the out-put."	Preserving phonological material of elements with greater semantic content helps the hearer to identify the most important parts of a discourse.
IDENTITY (McCarthy & Prince 1995)	"Input and output are identical."	Input-output identity, i. e. uniformity of morphemes across environments, helps the hearer to identify mor-phemes.
SAMEVOICE (Gussenhoven & Jacobs 1998: 48)	"Sequences of obstruents within a syllable must agree for voicing."	Obstruent sequences with different phonation types are difficult to pronounce because the phonation is impeded by the obstruent occlusion.

Many further constraints that have been used in the literature, including the literature on OT in syntax, can be reformulated in terms of user optimality as well. Some further examples are shown in (3).

(3) User optimality of further grammatical constraints

name	grammatical constraint	corresponding user constraint
STAY (Grimshaw 1997, Speas 1997)	"Do not move."	Leaving material in canoni-cal positions helps the hearer to identify grammat-ical relationships and redu-ces processing costs for the speaker.
TELEGRAPH (Pesetsky 1998)	"Do not pronounce function words."	Leaving out function words reduces pronunciation costs for the speaker in a way that is minimally disruptive for understanding by the hearer.
RECOVERABILITY (Pesetsky 1998)	"A syntactic unit with semantic content must be pronounced unless it has a sufficiently local antecedent."	Omitting a meaning-bearing element in pronun-ciation makes the hearer's task of extracting the inten-ded meaning from the speech signal very difficult unless it can be inferred from the context.

There is probably no need to go into the details of what exactly makes language structures "good" for speakers and hearers, i. e. what constitutes user optimality. I take it as evident that the best option among a range of alternatives is the one which promises the highest net benefit to speaker and hearer. The most important cost factors are motor costs and cognitive processing costs, and the most important benefits are informativeness and persuasiveness (cf. Keller 1998: 189ff. for some general discussion).

Not all of the constraints that OT practitioners have been working with can be rephrased in terms of user optimality so easily. Sometimes more discussion is required. For instance, Hawkins (1999) gives compelling arguments for the view that filler-gap dependencies of the island type are difficult to process. There is thus good motivation for rephrasing Pesetsky's (1998) ISLAND CONDITION constraint in terms of user optimality, although this is not as straightforward as in (2)–(3). In other cases, a proposed OT constraint is so highly specific that it seems unlikely that a direct reformulation in user-optimality terms will ever be possible. Examples are Grimshaw's (1997: 374) constraint NO LEXICAL HEAD MOVEMENT ("A lexical head cannot move") and Pesetsky's (1998) constraint LEFT EDGE (CP) ("The first pronounced word in CP is a function word related to the main verb of that CP"). However, these constraints clearly have the flavor of theoretical constructs that help make the particular analysis work, but that would be the first candidates for elimination if this becomes possible. Sometimes OT analyses also posit language-specific constraints, and these clearly cannot have a counterpart in terms of user optimality: User optimality is necessarily universal. Finally, constraints that are the direct opposites of each other cannot be rephrased as user constraints, because opposite user constraints would cancel each other out and have no effect. But again, it seems that most OT practitioners consider analyses superior that avoid language-specific constraints and operate entirely with highly general, plausibly universal constraints. It is my impression that most of the widely used, non-ephemeral constraints can be reformulated in user-optimality terms in one way or another. I cannot of course demonstrate that this is indeed the case, but in addition to the OT constraints in (2) and (3), I will mention further constraints together with their user-optimality counterpart later in this paper. Readers who are not well-versed in the functionalist literature will in this way get an idea of why I am optimistic in this respect, even if I should not succeed in convincing them.

Thus, there is a generalization here that has not been captured so far: Loosely speaking, what is "good" from the point of view of the theory is good from the point of view of language users. Grammatical optimality and user optimality are largely parallel. The obvious way of accounting for this striking match between grammatical structures and speaker needs is the notion of **adaptation**. Grammatical structures are adapted to the needs of language users (cf. Croft 1990: 252). By making use of the notion of adaptation, we achieve two things. First, we can account for the parallels between grammatical constraints and constraints on speakers observed in this section. Second, we can answer the question of § 2, why the grammatical constraints are the way they are: The grammatical constraints are ultimately based on the constraints on language users.

The concept of adaptation is familiar from evolutionary biology. For instance, consider the fact that various fish species living in the Arctic and Antarctic regions have antifreeze proteins in their blood. These proteins constitute a structural fact about several unrelated species living far apart, which is obviously to the benefit of these fish. It would be completely mysterious without assuming either a benevolent Creator (the almost universally accepted view until the 19th century) or a historical process of adaptation. It was Charles Darwin's insight that a long-term evolutionary process of successive modified replications combined with environmental selection can account not only for the origin of species, but can also explain the highly complex adaptations found in biological organisms. In short: Arctic and Antarctic fish have antifreeze proteins in their blood because at some point antifreeze proteins arose accidentally (by random genetic mutation). This genetic feature spread because it allowed its bearers to enter a previously unoccupied ecological niche.

I argue in this paper that linguistic adaptation is in many ways analogous to biological adaptation.

4. A mechanism for adaptation: diachronic change

Although historical processes are typically associated with the social sciences and the humanities, they are in fact central to evolutionary biology. Evolutionary biology, in turn, is central to theoretical biology, as is expressed in Theodosius Dobzhansky's well-known remark that "nothing in biology makes sense except in the light of evolution". If biologists restricted their attention to purely synchronic phenomena (as they did well into the 19th century), they would understand very little of what they observe.

I will now argue that historical (or, as linguists say, diachronic) processes are of equally central importance for linguistic theory. Just like biological adaptation, linguistic adaptation requires time. We need to consider diachronic change if we want to understand why the OT constraints are the way they are, i.e. in what sense they are based on the user constraints of § 3.

Of course, I am not the first to argue that grammatical structures are "based" on "user constraints" (or "performance constraints", or "functional pressures"). There is a long tradition of functionalist thinking in linguistics that attempts to explain properties of language structure with reference to properties of language use (e. g. Jespersen 1894, Horn 1921, Hawkins 1994, Givón 1995). However, the functionalists have generally paid little attention to possible mechanisms for adaptation – they have usually taken adaptation for granted.

Consider as a concrete example Dik's (1997:30–34) discussion of Berlin & Kay's famous hierarchy of color terms (black/white > red > green/yellow > blue > brown > others), which embodies the claim that if a language has a basic term for a color somewhere on the hierarchy, then it also has terms for all the colors to the left of this color. Dik observes that this hierarchy is also relevant for the frequency with which color terms are used: 'black' and 'white' are the most frequently used color terms, followed by 'red', and so on. And he continues: "This suggests a functional explanation for the existence of hierarchies of this type: the more frequent the need for referring to some colour, the higher the chance that there will be a separate lexical item for indicating that colour." (Dik 1997:33).

This is an interesting suggestion, but it is not an explanation.[2] Useful or needed things are not sufficiently explained by their usefulness or the need for them. Again, biology provides the appropriate analogy: Antifreeze proteins are surely useful for polar fish, indeed necessary for their survival, but this does not suffice as an explanation for their presence. Taking functional statements as sufficient explanation can be called the **Teleological Fallacy**, which is just a special case of humans' general tendency to think in anthropomorphic terms. When speaking about human artifacts, functional or teleological statements are unproblematic: "A bicycle saddle is softer than other parts of the bicycle **in order for** cyclers to sit comfortably." This statement suffices as an explanation for the softness of the saddle because it can easily be converted into a purely causal statement: "A bicycle saddle is soft **because** the bicycle makers have made it soft **in order for** cyclers to sit comfortably." This can be considered a full explanation because the purpose clause depends on an action verb, and the purpose can be attributed to goal-oriented human design. Similarly, antifreeze proteins in polar fish can be fully explained with reference to goal-oriented, purposeful divine design, if one has no concept of evolution or rejects this concept ("Polar fish have antifreeze proteins in their blood **because** God created polar fish with antifreeze proteins **in order to** help them survive in freezing water").

For obvious reasons, neither human design nor divine design are available in linguistics to convert functional statements into full explanations. But linguists have often fallen victim to the Teleological Fallacy (if only in their rhetoric), and we often find statements such as those in (4) (emphasis added).

(4) a. "Case is formed for reasons of ambiguity, because at some point in history speakers must have talked without *cases* (*Cato interficit Caesar*). Then inflection was added, **in order for** the meaning of the sentence to become clear." (Scaliger 1584: Book 4, ch. 77:169–80, cited after Breva-Claramonte 1983:66)

2 I propose an explanation below in §6.6 (vi).

b. "[C]oding devices tend to be employed strategically [by grammars] **so as to** guarantee, at minimal formal expense, the distinguishability only of those grammatical relations which would otherwise be too difficult to distinguish by the addressee." (Plank 1987:177)

c. "[S]yntactically relevant morphemes tend to occur at the periphery, **in order to** be visible for the syntax." (Booij 1998b: 21)

d. "*Of* was introduced **in order to** Case-mark a NP/DP which would not otherwise be Case-marked." (Lightfoot 1999:121)

Critics of functionalism in linguistics have rightly pointed out that such explanations are not viable. Haider (1998:98) observes that "the fact that the design is good for a function is not the driving force that led to the design", and Tooby & Cosmides (1990:762) remark: "It is magical thinking to believe that the "need" to solve a problem automatically endows one with the equipment to solve it".

However, the fact that functionalists rarely provide an explicit mechanism for functional adaptation in language structure does not mean that none exists and that functional explanation in adaptationist terms is possible only in biology. I will now argue that linguistic change is sufficiently similar to biological change that we can transfer some key notions of evolutionary biology to linguistics (see also Croft (1996) and (2000), Kirby (1999), Nettle (1999) for evolutionary accounts that are close in spirit to mine). That linguists have largely ignored this possibility may be due to the fact that in the 20th century the prestige of diachronic studies has not been very high. But as in biology, we cannot understand synchronic language structure without taking into account its diachronic evolution.

5. Variation and selection in language

Let us briefly recapitulate how adaptive explanations work in biology. In ordinary colloquial speech, quasi-teleological statements such as (5a) are very common. They are accepted because everybody knows how teleological statements are translated into purely causal statements in Darwinian evolutionary theory (cf. 5b).

(5) a. Giraffes have long necks **in order to** be able to feed on the leaves of high trees.

b. At some earlier time, there was genetic variation: There were giraffes with somewhat longer necks and giraffes with somewhat shorter necks. **Because** giraffes with somewhat longer necks had the additional food source of high trees, they had greater reproductive success. **Therefore** the long-neck gene spread throughout the whole population.

I propose that the translation from teleological to causal statements works very similarly in linguistics. The quasi-teleological, functionalist statement in (6 a) is insufficient on its own, but it becomes quite acceptable when we realize that it can be thought of as just an abbreviation of the purely causal statement in (6 b).

(6) a. In *cat-s* [kæts], the suffix consonant is voiceless **in order to** satisfy the SAMEVOICE constraint. (Or: ... **in order to** facilitate the pronunciation of this obstruent cluster.)
b. At some earlier time, there was structural variation: The suffix *-s* could be pronounced [z] or [s]. **Because** [kæts] required less production effort than [kætz], speakers chose it increasingly often (in order to save production energy). After some time, the form [kæts] had become very frequent and **therefore** was reanalyzed as obligatory, while [kætz] was no longer acquired and dropped out of the language.[3]

On the analogy of the biological term "natural selection", this process can be called "functional selection" (cf. Nettle (1999: 30–35) for this term and some discussion; Kirby's (1999: 36) equivalent term is "linguistic selection"). The application of the evolutionary scenario in linguistics presupposes three hypotheses: (i) Languages show structural **variation** in all areas of grammar, and language change is unthinkable without structural variation; (ii) frequency of use is determined primarily by the **usefulness** (or "**user optimality**") of linguistic structures; and (iii) **high-frequency** structures may become **obligatory**, and **low-frequency** items may **be lost** as a result of their (high or low) frequencies. In the remainder of this section, I will briefly motivate these hypotheses (a full justification is of course beyond the scope of this paper).

The insight that there is constant variation in species was one of the key ingredients in Darwin's evolutionary theory – before Darwin, species had been thought of only in terms of their properties, as immutable eternal essences (Mayr 1982). Only Darwin's shift to a population-based view of species, which allowed for variation and historical change, made evolutionary theory possible. That languages show constant variation has been commonplace in linguistics for a long time, and students of diachronic change routinely assume that every change begins with variation, both at the individual and at the social level. Like descriptive anatomists, descriptive grammarians have usually worked with idealized systems, for good reasons. However, one of the consequences of the present approach is that variation is highly relevant for the theoretical grammarian.

The second hypothesis probably does not need any further justification: That speakers use more user-friendly structures more often than less user-friendly ones can easily be derived from unchallenged common-sense knowledge about human nature.

The third hypothesis is perhaps not so obvious, but there is of course ample evidence for the crucial role of frequency of exposure in establishing cognitive patterns, and more specifically grammatical patterns. The establishment of grammatical structures in the mind is called **entrenchment** by Langacker (1987):

> "Every use of a structure has a positive impact on its degree of entrenchment, whereas extended periods of disuse have a negative impact. With repeated use, a novel structure becomes progressively entrenched, to the point of becoming a unit; moreover, units are variably entrenched depending on the frequency of their occurrence (*driven*, for example, is more entrenched than *thriven*)." (Langacker 1987: 59)

The psycholinguistic evidence for frequency as a relevant factor for mental representation is of course enormous. What is less clear is how high frequency of use can turn a linguistic variant into the only possible option. Here further research is needed, but in any event some such mechanism must exist (see also Kirby 1999: ch. 2 for discussion). Entrenchment due to frequency thus corresponds to selection in biology. Just like the useful genes spread in a species because of the greater reproductive capacities of their bearers, linguistic features may spread in a speech community because of their usefulness (combined with their social value), and they may become obligatory in grammars because of their high degree of entrenchment. Croft (1996) puts it as follows:

> "The proper equivalent [of the perpetuation of genes] is that the perpetuation of a particular utterance structure is directly dependent on the survival of the cognitive structures in a grammar that are used by the speaker in producing utterances of that structure. I suggest that the interactive-activation model used by cognitive grammar and by Bybee (1985) provides a mechanism by which cognitive structures can "survive" – become entrenched in the mind – or "become extinct" – decay in their entrenchment." (Croft 1996: 115–16)

Of course, the correlation between frequency of use and certain linguistic structures has often been noted, e. g. by Jespersen (1894), Horn (1921), Zipf

3 It must be admitted that this example is not ideal because [kæts] and [kætz] cannot have occurred as variants side by side for a very long time. [kætz] is very difficult to pronounce, so it was presumably eliminated very soon. I chose this example because it was mentioned in a different context earlier. (A better example would have been the choice between [stoʊnz] and [stoʊnəz], which presumably occurred side by side for a long time. However, since [stoʊnz] arose by vowel loss from the earlier [stoʊnəz], rather than the latter by epenthesis from an earlier [stoʊnz], the parallel with the OT analysis of Tableau 1 would not be so clear.)

(1935), Greenberg (1966), Du Bois (1987). However, linguists have generally been vague about the mechanism by which frequency of use influences language structure. Zipf (1935: 29) claimed that "high frequency is the cause of small magnitude", but he did not explain how frequency shrinks linguistic units. Du Bois (1987) observed that "grammars code best what speakers do most", but he did not explain how this marvelous fit of form to function comes about. If entrenchment, i. e. the establishment of patterns in speakers' mental grammars, is frequency-sensitive, we can actually explain such frequency-based generalizations.

Although language change is of course not intended by speakers, linguistic evolution as outlined above has intentional aspects. Speakers speak and listen intentionally, and their choices of specific expressions from a range of options can also be said to be intentional, although these are usually fairly automatic and unconscious (cf. Itkonen's 1983: 185ff. concept of "unconscious rationality"). But unlike (6 a), which cannot be literally true (and therefore has to be translated into (6 b)), a statement such as (7) can be taken as literally true.

(7) Speakers often chose [kæts] rather than [kætz] **in order to** save production energy.

Processes of language change like the one outlined in (6 b) are thus neither completely intentional processes (clearly languages don't change because speakers want to change them) nor completely mechanical processes in which human intention plays no role. Keller (1994) has exposed the frequent fallacy of dichotomizing all processes into the disjoint classes of human actions and natural processes. Processes of linguistic change and selection do not fit into either of these two categories: They are the cumulative outcome of a large number of intentional actions of type (7), an outcome that is not among the goals of these actions. A recent example of this fallacy is Haider's (1998: 97) characterization of the functionalist view as "the hypothesis that grammar might indeed be a human artifact, that is, a tool shaped by the human mind for a highly desirable function, namely effective and efficient communication". But on the present view, grammar is neither a human artifact nor a biological entity which can be studied in depth without any regard for human actions or choices. It is the unintended product of a complex but reasonably constrained and regular historical process, linguistic evolution.

There is one important difference between biological evolution and linguistic evolution that should be mentioned at this point: While the source of genetic variation in biology is restricted to random mutations, the source of linguistic variation, innovations in the speech of individual speakers, is often non-random. For instance, the introduction of the variant pronunciation [kæts] (*cats*) in addition to the older [kætz] was clearly motivated by the same user constraint that led to the increasing use of this variant and its eventual

obligatoriness. In this sense, the evolution of linguistic structures is in part "Lamarckian", like the evolution of other conventional mental structures (generically called "memes" by Dawkins 1976). This difference does not mean that linguistic evolution cannot be regarded as an evolutionary process (cf. Keller 1994: §6.1, Croft 1996). In biology, "Lamarckian" evolution does not work because acquired characters are not inherited, but in linguistic evolution, acquired features can evidently be passed on. The main argument I have made here, that synchronically adaptive structures can be understood in terms of a diachronic process of variation and selection, is not affected by this difference in the mechanism of replication.

One might even go so far as to attribute functional adaptation in language exclusively to the functional factors influencing speaker innovations. This is done implicitly by Croft (2000), who draws the analogy between biological and linguistic evolution in the following way: Mutation is analogous to innovation, and selection is analogous to propagation of a change. Croft maintains that linguistic variants are selected/propagated by speakers because of their social value, i. e. the social status and relationships of the people using the selected variant[4]. To use Nettle's (1999) terms, Croft attributes the propagation of linguistic features exclusively to "social selection" and sees no role for "functional selection". Even if this turned out to be correct, the main point of this paper would not be affected (Bill Croft, p.c.). Linguistic evolution would then be entirely "Lamarckian", but such an evolutionary scenario would be equally capable of transforming teleological statements into causal statements. I do not doubt that social selection is extremely important in linguistic diachrony. To a large extent, the fact that languages differ in their structure, i. e. conventionally assign different weights to different constraints, must ultimately be attributed to social selection. In order to avoid the difficult consonant cluster in [kætz], speakers could also have selected other options, e. g. they could have modified the stem consonant (yielding, e. g., [kædz]); that they did not do this must have been due to social selection. Whatever the precise roles of social and functional selection, structural adaptation in language must be due the effect of constraints on performance combined with a mechanism that turns preferred options of language use into structural patterns of grammar.

In the next section, I will make this general approach more concrete by examining a number of proposed (theory) optimality constraints and by showing how they can be understood as resulting ultimately from user optimality.

4 "[I]n general, differences in functional utility do not play a role in the propagation of a variant; only differences in social utility do." (Croft (in press), ms. p. 132–33)

6. Grammatical optimality reduced to user optimality

In the preceding sections, I proposed that the correspondence between grammatical optimality and user optimality can be explained in terms of a theory of diachronic adaptation. This is a very strong claim which can be falsified easily by showing that a particular synchronically adaptive structure could not have arisen through a diachronic process of adaptation as sketched in §5. In this section, I will examine a number of proposed OT constraints and show in each case how they have arisen from the corresponding user constraints. I should perhaps emphasize here that these case studies are not intended as substantive contributions to the respective areas of linguistics, and that I necessarily gloss over many controversies in the brief accounts given here. My only purpose in this section is to illustrate in a concrete fashion how the general program of diachronic adaptation in linguistics might work.

6.1 No Voice Coda

Let us begin with optimality constraints that have been proposed in phonology. German syllable-final devoicing is generally accounted for by invoking a constraint No Voice Coda (e.g. Golston 1996, Raffelsiefen 1998).

(8) No Voice Coda
Voiced coda obstruents are forbidden. (Golston 1996: 717)

The diachronic origin of this constraint is fairly clear. Old High German records show no evidence of this constraint: The spelling consistently has voiced obstruents in syllable-final position, e.g. *tag* 'day', genitive *tages* 'day's'. But by the Middle High German period, the spelling typically indicates that the pronunciation was voiceless (*tac*, genitive *tages*). So at some point in the Middle Ages, the devoiced pronunciation must have become an obligatory part of the grammar.

Obligatory devoicing was in all likelihood preceded by a period of variation in which both the voiced and the unvoiced pronunciation of obstruents in coda position was possible (as well as indefinitely many degrees of voicing in between). How this variation came about in the first place is clear: Voiced obstruents are difficult to pronounce in coda position for well-understood phonetic reasons (cf. Keating et al. 1983), so speakers of all languages with voiced coda obstruents have a tendency to devoice these in pronunciation, thus introducing phonetic variation. In German these devoiced pronunciations became prevalent at some point, and speakers came to treat them as part of the conventionalized grammatical pattern.

Thus, the user constraint corresponding to No Voice Coda can be formulated as in (9).

(9) "User-optimal No Voice Coda":
Coda obstruents should be pronounced voiceless in order to avoid articulatory difficulties.

6.2 MaxLex

The constraint MaxLex is proposed by Casali (1997) to account for the fact that vowel elision is less likely to affect roots and content words than affixes and function words:

(10) MaxLex
"Every input segment in a lexical word or morpheme must have a corresponding segment in the output." (Casali 1997: 501)

For example, in Etsako (Niger-Congo) vowel elision in hiatus contexts generally affects the first of two adjacent vowels, i.e. the initial vowel of the second word is preserved (e.g. /owa ɔda/'a different house' → [ow' ɔda]). But when the second word is a function word, its vowel is elided (e.g. /ɔna aru ɔli/ 'that louse (lit. the louse that)' → [ɔn' aru 'li]).

While no direct diachronic evidence is available in this case, it is easy to reconstruct how the current distribution must have come about. Originally, the underlying sequence /ɔna aru ɔli/ could be pronounced with all its vowels intact, and at some point speakers began to drop vowels to avoid the hiatus. Initially any vowel could be elided, but speakers more often elided the final vowel to aid word recognition (words are more easily recognized by their initial segments). However, in function words such as /*ɔli*/ 'that', speakers tended to elide the first vowel, because due to their high frequency and predictability, function words can be recognized more easily than content words. Thus, [ɔn' aru 'li] was used significantly more often than [ɔn' ar' ɔli], and as a result it became fixed (i.e. entrenched) in speakers' grammars. Thus, speakers make use of the user-optimal counterpart to (10):

(11) "User-optimal MaxLex"
Lexical morphemes should be pronounced fully because they are relatively rare and unpredictable, while functional morphemes can be reduced phonetically without a major threat to comprehensibility.

MaxLex is of course an old insight. Jespersen (1922: 271) observed that "[i]t has often been pointed out ... that stem or root syllables are generally better preserved than the rest of the word: the reason can only be that they have greater importance for the understanding of the idea as a whole than other syllables". Jespersen was also aware that this match between function and form must somehow lie in language use,[5] but like most other functionalists of the 19th and 20th centuries, he did not make the causal connection between constraints on language use and constraints on language structure explicit.

6.3 DropTopic

Let us go on to syntactic constraints now. The constraint DropTopic is proposed by Grimshaw & Samek-Lodovici (1998) to account for the fact that subject pronouns are omitted in many languages (e.g. Italian *ha cantato* 'he has sung', not *??lui ha cantato*) when they convey topical information.

(12) DropTopic
"Leave arguments coreferent with the topic structurally unrealized."
(Grimshaw & Samek-Lodovici 1998)

In non-null-subject languages like English, DropTopic is dominated by the constraint Parse (or MaxIO), which requires the underlying topical pronoun to be present overtly. Like the constraint MaxLex of the preceding subsection, DropTopic corresponds to speakers' tendency to use overt material economically. While MaxLex specifies that lexical (i.e. relatively unpredictable) information should be preserved, DropTopic specifies that topical arguments, i.e. relatively predictable information, should be omitted. A more general statement of this requirement is Pesetsky's (1998) Telegraph: "Do not pronounce function words." If one considers topical personal pronouns to be function words,[6] then Telegraph subsumes DropTopic.

In this case the diachronic scenario is so well known that I need not say much here: As a general (though not exceptionless) rule, languages with rich subject agreement do not allow a personal pronoun when it conveys topical information (cf. Gilligan 1987). However, the pronoun may be used occasionally for reasons of extravagance or "expressiveness" (cf. Haspelmath (to appear)), thus introducing variation. Now in languages that are losing their rich subject agreement morphology on the verb (as has happened in English and French, for instance), speakers will increasingly tend to choose the option of using the personal pronoun, because the verbal agreement does not provide the information required for referent identification in a sufficiently robust way. At some point the use of personal pronouns becomes so frequent that it is reanalyzed as obligatory and the frequent performance pattern comes to be reflected in a competence pattern. This scenario gives rise to the English and French situation, in which Parse dominates DropTopic. Conversely, speakers of older Italian did not use the overt pronoun much because the full subject agreement on the verb made this unnecessary, and as a result the pronounless pattern is (still) obligatory in Italian today. Thus, the user constraint corresponding to DropTopic in (12) is as shown in (13).

(13) "User-optimal DropTopic":
A topical subject pronoun should be omitted to save production energy when it is relatively predictable, e.g. in a language with rich subject agreement. (It should not be omitted when no robust information from agreement is available.)

6.4 Stay

The constraint Stay was proposed by Grimshaw (1997: 374) and is technically formulated as Economy of Movement ("Trace is not allowed."). For most purposes, this amounts to the same as (14), which is Speas's formulation.

(14) Stay
"Do not move." (Speas 1997: 176)

Grimshaw uses this constraint to account for the ungrammaticality of multiple *wh*-questions with multiple *wh*-movement in English (**What will where they put?*). Since I know too little about the diachronic evolution of this particular construction, I will choose as my example another construction where it would seem natural to invoke Stay as well. SVO languages with rigid word order (such as English) typically show NP-PP word order in postverbal position, i.e. a sentence like (15a) is the only possibility, and (15b) is ungrammatical.

(15) a. *I introduced Kostya to Toshio.*
b. **I introduced to Toshio Kostya.*

If (15a) shows the underlying order (V-NP-PP), then (15b) is ruled out because it violates Stay: The direct object NP has moved to the right of the PP (or conversely).

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5 Cf. Jespersen (1922: 271): "In ordinary conversation one may frequently notice how a proper name or technical term, when first introduced, is pronounced with particular care, while no such pains is taken when it recurs afterwards: the stress becomes weaker, the unstressed vowels more indistinct, and this or that consonant may be dropped." Here he refers to first mention vs. later mention of a rare word, but similar considerations apply to rare vs. frequent words.

6 At the very least, personal pronouns are normally omitted in "telegraphic speech", just like other function words.

Again, this constraint in English has its roots in earlier diachronic variation. And again, the facts are too well known to need much discussion: Word order in Old English was much less constrained than in modern English, and the equivalent of (15b), with V-PP-NP word order, was unproblematic. But as morphological case was being lost, there was an increasing need to identify syntactic relations of phrases by their surface positions.[7] What speakers did was to vary word order much less in performance (relying on other means to convey information-structural information), generalizing the most common order V-NP-PP until it became obligatory.[8] So again, frequent occurrence in speech gives rise to a grammatical pattern. The performance constraint analogous to STAY is formulated in (16).

(16) "User-optimal STAY":
Syntactic elements should not be linearized in a non-canonical way if that creates potential ambiguity for the hearer.

6.5 ANIMATE INANIMATE

The constraint ANIM(ATE) > INANIM(ATE) is used by Aissen (1997) to account for various animacy effects, for instance the restriction in Tzotzil (a Mayan language of Mexico) that prohibits the active voice when the patient is inanimate (cf. 17a).[9] In such cases, the passive voice must be used (cf. 17b), because the non-subject must not outrank the subject on the hierarchy "animate > inanimate".

(17) Tzotzil (Aissen 1997: 725, 727)

a. **I-x-poxta* *Xun* *li* *pox-e.*
ASPECT-3P.AGENT-cure Juan the medicine-ENCLITIC
'The medicine cured Juan.'

b. *Ipoxta-at* *ta* *pox* *li* *Xun.*
cure-PASSIVE by medicine the Juan
'Juan was cured by the medicine.'

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7 This is not the only possibility, as Bill Croft has reminded me. It is equally possible that word order became fixed "spontaneously" and that this in turn facilitated the loss of case distinctions. In this case, the functional, user-optimal motivation for the change would be much less obvious (cf. Lehmann 1992 for a proposed answer).

8 Why V-NP-PP rather than, say, V-PP-NP was generalized is a separate issue that is irrelevant here. See Hawkins (1994) for a theory of syntactic processing that explains the preference for NP-PP order over PP-NP order in VO languages.

9 Cf. also Müller (1997a: 15) for the use of a similar animacy-based constraint in a different context.

Constraints having to do with animacy are of course very familiar from the functionalist literature (cf. Comrie 1989: ch.9), so this is a particularly bad candidate for an innate constraint. A much more plausible scenario again begins with frequency in performance. Universally, there is a strong statistical correlation between topicality and animacy: We tend to talk about humans and other animates, and our sentences usually predicate additional information about them. In those languages that have a strong association between topicality and subjecthood, most subjects will therefore be animate, and most inanimates will be non-subjects. In some languages, these skewed frequencies may become categorical distinctions, i.e. the most frequent patterns may become the only possible ones. This is what must have happened in Tzotzil at some point in the past.

Thus, Aissen's competence constraint ANIM > INANIM corresponds to a very general preference of speakers to talk about animates more than about inanimates. The corresponding "user constraint" cannot really be called a constraint in the sense of a restriction put on speakers – it is what people naturally tend to do.

(18) "User-optimal ANIM > Inanim":
An animate referent should be chosen as topic because the hearer is more likely to be interested in getting more information about animates than about inanimates.

6.6 Further cases

It would not be difficult to continue this list of grammatical optimality constraints that can be shown to have arisen as a result of selection from the variation introduced through language change. As I observed earlier, not all constraints that have been used in OT analyses can be reduced to user constraints in a straightforward fashion, but it seems to me that most widely used constraints can be so reduced. This is of course particularly true of the most general constraints whose names evoke a long earlier literature, such as RECOVERABILITY (e.g. Pesetsky 1998), SALIENCE (e.g. Müller 1997a), SONORITY (e.g. Raffelsiefen 1998), OCP (e.g. Booij 1998a), ANIM > INANIM (e.g. Aissen 1997), (LEXICAL) INTEGRITY (e.g. Anderson 1997). They are most obviously adaptive, but these are also the constraints for which an innateness assumption is the least plausible. Their use in constraint tableaux is often very convenient, but it is clear that this cannot be the whole story of explanation. In each case we need a diachronic scenario of conventionalization that links the constraints on language use to the observed patterns of grammar.

The same is of course true for classical cases of functional explanations evoking a highly general theoretical construct which is intended to explain an observed grammatical pattern, but is not really sufficient as an explanation. Examples include the following:

(i) **Iconicity**: Haiman (1983) notes that there is an iconic relationship between the form and the meaning of, for instance, causative constructions: Causatives expressed by more closely bound items tend to express more direct causation. But this correlation becomes an explanation only if it can be shown that speakers are constrained by iconicity in language use and that patterns of use become grammatical patterns.

(ii) **Economy**: Many linguists have stressed the role of economy in explaining grammatical patterns, especially the shortness of frequent expressions, or the omission of redundant expressions (cf. Zipf 1935, Greenberg 1966, Haiman 1983, Werner 1989). But again, pointing out a correlation is not sufficient: We also have to show how frequent use leads to shortness (e.g. by increased diachronic reduction in frequent items).

(iii) **Phonetic efficiency**: Gussenhoven & Jacobs (1998: 34) note the tendency for phonetic inventories to lack a [p] in the series [p, t, k], and a [g] in the series [b, d, g], and they relate this to the relative inefficiency of [p] and [g]. For instance, [g] "is relatively inefficient from the point of view of the speaker, because the relatively small air cavity behind the velar closure causes the air to accumulate below it, thus increasing the supraglottal air pressure and diminishing the glottal airflow, and thereby causing voicing to stopà That is, à [g] is relatively hard to say." But the authors do not say how it might be explained that languages tend to lack inefficient stop consonants. They merely suggest that "languages somehow monitor the development of their phonologies", as if it were obvious what the literal translation of this metaphorical way of speaking should be.

(iv) **Compensation**: Nettle (1995) argues that languages with large phonemic inventories have the compensatory advantage of allowing shorter linguistic units. We thus have a tradeoff relation between paradigmatic costs and syntagmatic economy (and vice versa). Nettle notes that this is explained if "language is functionally adapted to the needs of efficient communication" (1995: 359), as if functional adaptation were not an explanandum itself. He also hints that languages should be seen as "dynamical, self-organizing systems" (1995: 365), but of course we need to know how the self-organization works (but see Nettle 1998, 1999, where Nettle does provide the needed background theory).

(v) **Early Immediate Constituents**: Hawkins (1990, 1994) shows that the principle of Early Immediate Constituents makes correct predictions both about the distribution of word order patterns in performance (where word order is mandated by grammatical rules) and about universals of grammatical word order rules. Hawkins vaguely talks about the "grammaticalization" of word order patterns, but he does not elaborate on this. Clearly, what is needed is a theory of how frequent word order choices in performance tend to become fixed in diachronic change (cf. Kirby 1994, 1999).

(vi) **Frequency**: We saw above that Dik (1997) attempts to explain the color term hierarchy with reference to the frequency of color terms. This correlation between frequency and cross-linguistic occurrence can be turned into an adaptive explanation in the following way: First, basic color terms that a language already possesses are the less likely to be lost from the lexicon the more often they are used by speakers, because high frequency of use leads to a high degree of entrenchment. Second, of the colors for which a language does not have basic terms, those that are the most frequently referred to by non-basic terms will be the most likely to acquire basic terms, for instance by change of a polymorphemic non-basic color term to a basic color term. The fusion of a polymorphemic word to a monomorphemic word is facilitated by high frequency of use. Thus, because of speakers' tendencies in language use, we obtain the universal hierarchy of basic color terms.

7. Are grammatical constraints due to accident?

I have argued so far that the grammatical constraints employed in Optimality Theory are the way they are because they arise from universal constraints on language use through a diachronic adaptive process. But of course, it is theoretically possible that the recurring correspondence between grammatical constraints and user constraints is "a mere coincidence, a serendipitous outcome that speakers may exploit" (to use Durie's 1995: 278 phrase). This would be an astonishing coincidence indeed (and I doubt that anybody would seriously defend such a view), but it is nevertheless possible. In fact, recently a number of linguists have tended to emphasize the dysfunctional aspects of language structure (e.g. Chomsky 1991: 448, Haider 1998, Uriagereka 1998, Lightfoot 1999: Ch. 9), and the view that OT constraints or all of UG are accidental properties of the human mind is more than just a straw man. "UG may have evolved as an accidental side-effect of some other adaptive mutation" (Lightfoot 1999: 249; cf. also Haider 1998: 106). Persuasive evidence for this view would be a widely attested OT constraint that is dysfunctional, but proponents of this view have so far only presented far less convincing cases.

Lightfoot (1999) mentions the example of the constraint that traces must be governed lexically, which prohibits complementizer deletion in (19b), but not in (19a).

(19) a. *Fay believes that/Ø Kay left.*
b. *Fay believes, but Ray doesn't, that/*Ø Kay left.*

Now according to Lightfoot the same condition also prohibits straightforward subject extraction in a variety of languages: (20) is a problem not just for English.

(20) **Who_i do you think e_i that e_i saw Fay?*

Lightfoot claims that this constraint is dysfunctional because clearly structures like (20) are needed by speakers, as is shown by auxiliary structures employed in diverse languages to "rescue" the structure.

But such a case shows nothing about the dysfunctionality of UG constraints. Lightfoot's fundamental error is that he does not distinguish the **functional** effects of the constraints from their **incidental** effects. This distinction has been widely discussed by philosophers (e. g. Wright 1973, Millikan 1984): For example, pumping blood is a functional effect of the heart, but throbbing noises are incidental effects. The heart both pumps blood and makes throbbing noises, but it is only the former effect that the heart has been designed by selection to produce. The throbbing noises may sometimes be inconvenient, but these incidental effects cannot be used as an argument that the heart is dysfunctional or is an accidental side-effect of some other adaptation. Lightfoot (1999: 249) admits that the condition on movement traces "may well be functionally motivated, possibly by parsing considerations", so the ungrammaticality of (20) in English only shows that grammatical constraints may have incidental effects, not that they may be non-adaptive or dysfunctional.

Even less impressive is Haider's (1998) case for the dysfunctionality of superiority effects in English *wh*-movement. Haider notes that in some languages (e. g. German)' the counterpart of (21 b) is grammatical.

(21) a. *Who bought what?*
b. **What did who order?*
c. *What was ordered by whom?*

However, as Haider notes, Kuno & Takami (1993) have proposed a usage-based explanation for the contrast between (21 a) and (21 b), which starts from the observation that sentences like (21 b–c), in which agents are sorted on the basis of themes, are "unnatural in normal circumstances". This is exactly the kind of situation in which we would expect a grammatical constraint ('WH-SUBJECT > WH-OBJECT") to arise in the process of diachronic adaptation (analogous to the constraint ANIM > INANIM of § 6.5). Haider's objection against the functional explanation is that not all languages show its effects, but this reveals a fundamental misunderstanding of the way in which user optimality and grammatical optimality work: In languages like German, the universal constraint is simply violated, and the counterpart of (21 b) is grammatical because other constraints are ranked higher. Thus, far from being dysfunctional, the constraint against (21 b) is functionally motivated, and the fact that it prohibits some potentially useful structures is in no way special (for instance, nobody would suggest that a constraint against morphological repetition is dysfunctional just because it rules out potentially useful words like **friendlily* or **monthlily*).

I conclude that the case for dysfunctionality of grammatical constraints is very weak. As we have seen, many grammatical constraints correspond directly to user constraints, and the likelihood that there is no causal connection between the two sets of constraints is infinitesimally small.

One possibility is of course that the grammatical constraints arose in some way as an adaptive response to the user constraints in biological evolution, not in diachronic linguistic evolution. This has been proposed by various authors (e. g. Pinker & Bloom 1990, Newmeyer 1991), and it is a possibility that must be taken very seriously. However, a full discussion of this possibility is beyond the scope of this paper. The main practical problem with the biological-adaptation scenario is that it is necessarily more speculative than my scenario of diachronic linguistic evolution. I think it is a sound methodological principle to try the more empirically constrained explanations first, before speculating about prehistoric events that have left no direct trace. Moreover, the violability of the optimality constraints makes them poor candidates for innate devices, whereas violability follows automatically if the constraints arise in diachronic adaptation. But even so, I expect the argument made in this paper to be challenged primarily from the direction of biological evolution, so theoretical linguists are well advised to watch developments in biological evolutionary linguistics closely.

8. Conclusion

My main argument in this paper has been that optimality constraints of the type postulated in Optimality Theory, which are usually conceived of as stipulated elements in a pure competence theory, need to be further analyzed in terms of constraints on language use. Otherwise it remains mysterious why the constraints that we find applicable in languages are the way they are, and why many logically possible constraints play no role in any language (e. g. NOONSET, OBLIGATORYCODA, DON'TSTAY, MAXFUNC, INANIM > ANIM, and so on, i. e. the exact opposites of the constraints we have seen).

The mechanism proposed here for linking grammatical constraints to user constraints is diachronic adaptation: In language change, variants are created from which speakers may choose. Being subject to various constraints on language use, speakers tend to choose those variants that suit them best. These variants then become increasingly frequent and entrenched in speakers' minds, and at some point they may become obligatory parts of grammar. In this way, grammars come to be adapted to speakers' needs, although speakers cannot shape language actively and voluntarily. Grammatical constraints are thus the way they are because they have arisen from user constraints in a diachronic process of adaptation.[10] Diachronic adaptation in language is in many ways analogous to adaptation in biological change.

That grammatical structures are typically adapted to language users' needs in a highly sophisticated way is an old insight, but how exactly this adaptation should be explained is rarely discussed even by functionalists. Croft (1993: 21–22) notes that "the philosophical analogy between linguistic functional explanations and biological adaptation is not always fully worked out in linguistics". The Teleological Fallacy appears to be so powerful that linguists have rarely seen the necessity of providing a theory of diachronic adaptation. But that such a theory is needed has been recognized by other authors as well (e. g. Bybee 1988, Kirby 1994, 1997, 1999, Durie 1995, Nettle 1998). Hall (1988) observes that in addition to finding "underlying principles, probably of a psychological or functional nature", we must

> "attempt to establish the mechanism by which the underlying pressure or pressures actually instantiate in language the pattern under investigation. This latter requirement will involve the investigation of diachronic change for some properties and of phylogenetic evolution for others." (Hall 1988: 323)

Diachronic adaptation provides an account of the paradoxical situation that intentional actions of individuals, which have nothing to do with grammatical optimality, can have the cumulative effect of creating an adapted grammar, consisting of constraints that are good not only in a theory-internal sense, but also from the language users' point of view. Situations of this kind, in which a large number of micro-events give rise to a macro-structure in a surprising way, go by different names in the literature: "emergence" (Kirby 1997, 1999), "invisible-hand process" (Keller 1994), "spontaneous order" (Keller 1997), "self-organization" (Lindblom et al. 1983), "synergetic process" (Köhler 1986). So far there is no unified conceptual framework and terminology in linguistics for such phenomena, but it seems clear to me that this is a very promising paradigm.

If my proposal is correct, then the grammatical constraints are not innate, and are not part of Universal Grammar. They arise from general constraints on language use, which for the most part are in no way specific to language. This does not, of course, mean that there is no UG, no innate mental organ that is

10 Note that I am not claiming that all of language change is adaptive and motivated by user optimality of one kind or another (contra Vennemann 1993). For instance, grammaticalization changes, which probably account for the great majority of morphosyntactic changes, can hardly be described as adaptive (cf. Haspelmath to appear). I tend to agree with Dahl (1999), who describes grammaticalization as a kind of counter-adaptive inflationary process in which forms lose their functions and thus need to be replaced. My claim here is only that whatever adaptive structures we find synchronically must have their origin in an adaptive diachronic change.

specialized for linguistic skills. Clearly, there are universal properties of language that probably cannot be derived from constraints on language use, e. g. the fact that grammars generally do not contain numerical specifications (e. g. "a word may be at most 15 segments long"); or indeed the fact that humans use fairly rigid grammatical rules to begin with, rather than arranging morphemes in a random way and leaving interpretation to pragmatics (cf. Durie 1995: 279). But these features of language are so general that they have little to do with the grammarian's everyday work.[11]

The language-particular aspects of grammar that occupy most linguists most of the time can largely be accounted for in terms of conventionalized constraints on language use. The highly general constraints of OT have thus opened up new possibilities of (functional) explanation that were not available earlier in generative grammar.

Thus, by incorporating a theory of diachronic adaptation, linguistics can answer why questions, and is not limited to how questions (cf. Nettle 1998: 460). In this respect, linguistics is more like biology than like physics, more Darwinian than Galilean. Ridley (1994) puts it as follows:

> "In physics, there is no great difference between a why question and a how question. How does the earth go round the sun? By gravitational attraction. Why does the earth go round the sun? Because of gravity. Evolution, however, causes biology to be a very different game, because it includes contingent history... Every living creature is a product of its past. When a neo-Darwinian asks 'Why?', he is really asking 'How did this come about?' He is a historian." (Ridley 1994: 16–17)

In much the same way, I argue, a linguist who asks 'Why?' must be a historian.[12]

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11 In OT, they correspond to the components GEN and EVAL; the former has been largely ignored, apparently because of the implicit presupposition that it is not very interesting. However, from an innatist perspective it is the most interesting part of the theory, because it is the part that is the most likely to be innate.

12 Of course, whether one finds why questions interesting or not is a subjective matter. Hoekstra & Kooij (1988) argue that explaining language universals is not an important goal of generative linguistics. But I have doubts whether one can reach the goal of generative linguistics (discovering the nature of UG, i. e. answering a how question) while ignoring the question why linguistic structures are the way they are.

Adaptation, Optimality and Diachrony

William Croft

Department of Linguistics, University of Manchester,
Manchester M13 9PL, UK
w.croft@manchester.ac.uk

Haspelmath's paper offers a potentially fruitful synthesis of ideas from functionalist theories of language and optimality theory, a recent but popular formal model for phonology and syntax. As such, Haspelmath's paper is more productive than the all too common out-of-hand dismissal or ignoring of the opposite theoretical camp on both sides of the theoretical divide.

The opportunity for the synthesis of formalist and functionalist ideas has been created by optimality theory. Optimality theory uses the general idea of competing principles ("constraints") determining linguistic phenomena. Not all the principles can be satisfied at once for any particular phonological sequence (e. g. word + inflection) or syntactic construction. Hence, every possible output is at most partially motivated by the principles underlying language. Partial motivation of linguistic forms as a result of competing motivations is one of the chief theoretical constructs of most functionalist approaches to language, as Haspelmath notes in his paper.

What is most interesting about Haspelmath's paper is his argument that optimality theory and functional competing motivation models share more than the abstract concept of competing motivations. Haspelmath argues that the substantive principles proposed by optimality theorists, at least the ones most widely invoked and assumed to be universal across languages, are in fact identical to the substantive principles proposed by functionalist linguists. I believe that on the whole, Haspelmath is correct in this observation, and I hope that both (formalist) optimality theorists and functionalists will take note of the important similarities between the two approaches.

Significant differences remain between optimalty theory and competing motivations models, of course. Rather than focus on these, I would like to discuss what I perceive as a more general problem with Haspelmath's proposals which ultimately bear on the functional principles that he invokes for the universal constraints of optimality theory.

Haspelmath argues that the functional/optimality constraints are best interpreted as applying to language change, not to the static linguistic system. Haspelmath invokes a biological evolutionary model of language change, where

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the positive "adaptive" value of more optimal outputs leads to them being selected (i. e. produced) by speakers at a greater frequency over less optimal outputs. As Haspelmath notes, I disagree with his interpretation of the evolutionary model for language change (see Croft 1995, 1996, 2000). I will briefly present my views here, in order to set the stage to present a further problem not discussed in Haspelmath's paper.

Clearly, one cannot adopt all of the theory of biological evolution into linguistics (or any other discipline which displays apparently evolutionary patterns). Much of evolutionary biological theory is domain-specific. This includes the mechanism for selection in biology, adaptation to one's physical environment and to other members of the species (in particular with respect to oportunities for reproduction). Although the functional principles invoked by Haspelmath are described by him as "adaptive", they are a very different sort of adaptation, and so only a general analogy to adaptation in biological selection can be made in these terms.

Also, not all parts of evolutionary biological theory necessarily hang together, and hence not all parts of the theory need be carried over. Most important of all, a generalized theory of evolutionary processes, independent of any scientific domain, must be used in applying evolutionary concepts to another domain. Hull (1988) has developed a generalized theory of selection, independent of biology, which he applies to conceptual change in science and which can also be applied to language change. Hull argues that evolution is a two-level process: (1) the creation of replicators by a process of replication, some identical to the original and some altered; (2) the interaction of interactors with their environment, in such a way as to cause replication of replicators to be differential (i. e. some replicators are replicated in a higher frequency than others; Hull 1988: 408–9). The first process gives rise to variation, and the second process is selection proper. Nothing is implied about the types of mechanisms that give rise to altered replication or to selection; these are domain-specific.

If the mechanisms of altered replication and selection are domain-specific, they need not be analogous across domains. In fact, that appears to be the case in language change. All of the empirical evidence gathered in sociolinguistics indicates that variants (altered replicates) of linguistic structures are selected for social reasons, not "functional" ones (see e. g. Labov 1994, or any textbook on sociolinguistics). That is, functional "adaptation" is not the mechanisms for selection in language change.

How is it then that there seem to be more "optimal" than "dysfunctional" linguistic systems across the world? This is possible even if functional principles are mechanisms for altered replication, that is, innovation, and not selection. If functional constraints operate to determine the frequency of innovations, and the novel variants undergo social selection, then the end result is going to be a preponderance of optimal variants in the long run:

> the pattern of favoring and disfavoring contexts does not reflect the forces pushing the change forward. Rather, it reflects functional effects, discourse and processing, on the choices speakers make among the alternatives available to them in the language as they know it; and the strength of these effects remains constant as the change proceeds" (Kroch 1989: 238).

And in fact, this is exactly what we would expect, since functional constraints do not change over the time course of a change.

The argument in the last few paragraphs indicates that it is possible to have an evolutionary theory of language change in which functional constraints operate on inovation but do not play a role in selection. (In contrast, Haspelmath appears to adhere to an invisible hand theory like that of Keller 1990/1994, which in evolutionary terms is a theory of drift – differential replication without interaction with the environment [Hull 1988: 443].) In fact, there is additional evidence, besides the well-known sociolinguistic evidence, that the latter theory of language change is preferable.

Not all language changes are in the "optimal", "adaptive" direction. Andersen (1988) brings together evidence that "dysfunctional" phonological changes, such as fortition of vowels into consonants, occur in small, isolated speech communities. Such communities also tend to retain older forms, that is, the less "optimal" forms that the rest of the speech community has replaced with the newer, more "optimal" variants. Trudgill adduces additional examples and argues the the "dysfunctional" changes are likely to occur in such communities because of their close-knit social network structure (Trudgill 1989, 1992, 1996). The changes we call "natural" are in fact characteristics of larger, high-contact, looseknit societies (ibid.). Trudgill warns us against assuming that "natural" language change – the "adaptive" ones – are universally "natural" (see especially Trudgill 1989). Instead, they are "natural" for only certain types of societies, for social reasons.

A model in which selection is functionally "adaptive" cannot account for the selection of "dysfunctional" variants. On the other hand, one could expect exactly this state of affairs if the mechanism for selection is social, while the mechanism for altered replication is functional (and that there is a degree of randomness in both mechanisms). In the latter model, a "dysfunctional" variant from a functional perspective – the etymologically older variant, or perhaps a rare random "dysfunctional" production – can be selected for social reasons.

Does adaptation really help us to explain language change?

Östen Dahl

Inst. för lingvistik, Stockholms universitet, S-10691 Stockholm, Sweden
oesten@ling.su.se

Haspelmath's account of the role of teleological explanations in linguistics is clear and illuminating, as is also his analysis of the not too explicit relationship between OT constraints and functional motivations. But the main point of OT lies, as far as I can understand, not in the constraints themselves but rather in the claim that variation between and within languages can be reduced to differences in the ranking of constraints. It ought to follow from this that language change has essentially to be modification of the weightings of one or more constraints. Haspelmath's paper does not really address the issue of the viability of such a view on language change. Instead, he brings in adaptation as an explanatory concept from evolutionary biology.

The application of adaptation to the theory of linguistic change is not unproblematic. Typically, biological populations change their genetic make-up as a response to changes in the environment. Such situations are also found in language change, as when new words are introduced to refer to cultural innovations, but attempts to explain grammatical and phonological change in a similar way have not been convincing. Adaptation does constrain linguistic change in the sense that new structures have to meet minimal demands on functionality to be accepted. But this will not, in the normal case, lead to a transition from a sub-optimal to an optimal stage in the development of the language. A case in point is Haspelmath's paradigm example, the assumed transition from [kætz] to [kæts]. As he himself acknowledges in fn. 3, this example is "not ideal" since [kætz] is "very difficult to pronounce" and "was presumably eliminated very soon". Indeed, it can be doubted whether there has ever been a stage of English where [kætz] was a normal pronunciation. Rather than language change proper we seem here to be dealing with "instant adaptation".

Another key concept in Haspelmath's account is variation. Its role in the story of language change, however, is also problematic. Darwin's notion of natural selection presupposes pre-existing variation in the population. But since natural selection inevitably eliminates some of the variation, there must be something that constantly "feeds" the pool of variation if the whole process is not going to

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eventually cease. In evolutionary biology, random mutations are assumed to be the source of new variation. Haspelmath acknowledges that the situation in linguistics is different: the source of linguistic variation is often non-random, he says, and the introduction of a variant may be motivated by the same user constraint that drives the further process of selection. But this undermines the explanatory role of variation in the process of change: variation is not a precondition but rather a (temporary) consequence of change.

The allowed variation in constraint ranking weakens the proposition that OT constraints are universal; it seems that nothing can prevent constraints from getting such a low ranking that they are rendered non-operative in a language. Haspelmath's constraints are not universal but "arise from general constraints on language use". However, to make his theory work, he also needs to allow for variation, which means that the output of the constraints has to be to some extent underdetermined. On the other hand, this variation is assumed to be partly eliminated over time by selection processes guided by user optimality. That is, the same forces that determine the constraints also determine the elimination of the residue not determined by these constraints. In other words, how can optimalization operate on an already optimized input, and in addition, give rise to different solutions in different languages? The solution apparently favoured by Haspelmath is to ascribe those differences to social selection, following a suggestion by Bill Croft. He assures us that this does not affect the main point of his paper – still, it seems to me that it is unclear whether there is any essential role left for adaption in such a scenario. Indeed, there is no guarantee that changes induced by social selection always maximize user optimality. In a footnote, Haspelmath acknowledges that the great majority of all morphosyntactic changes – viz. those subsumable under the notion of grammaticalization – may in fact be counter-adaptive. Curiously, however, he later discusses such a change – the introduction of obligatory subject pronouns in West European languages – without mentioning the possibility of it being counter-adaptive. Let us briefly look at the issue.

The constraint PARSE mentioned in this connection is one of many OT constraints that presuppose a thinking in which a speaker operates on a given input – something like the "underlying structure" of other theories, and several of them could be summarized under a super-constraint "Don't mess with the input". But where do you get the input from? That is, who tells you that there is an "underlying topical pronoun"? It seems to be in the spirit of most functionally-oriented theories to avoid such assumptions, but it is not at clear what happens to the whole OT machinery if you do.

Haspelmath proposes a scenario in which (a) English and French speakers increasingly use subject pronouns to compensate for the poor inflectional system, (b) this use "becomes so frequent that it is reanalyzed as obligatory". Notice that it is precisely changes like (b) that characterize grammaticalization and that what motivates calling it "counter-adaptive" is that something is done

whether it is communicatively motivated or not. That is, subject pronouns are used both when they are needed in order to identify the referent of the subject and when they are informationally redundant. This fact is obscured in (13) and the preceding discussion. However, to end on a more positive note, this does not exclude other functional motivations for the introduction of obligatory subject pronouns. Thus, a certain degree of redundancy is in fact always needed for ensuring that the message is conveyed; also, certain automatic routines may be advantageous for the speaker even if they lead to a loss in "production energy". It remains to be seen, though, if such considerations can be accommodated in a theory of differentially ranked user constraints.

Prerequisites for a Theory of Diachronic Adaptation

B. Elan Dresher/William J. Idsardi

B. Elan Dresher, Department of Linguistics, University of Toronto, 130 St. George Street, Toronto, Ontario M5S 3H1, Canada
dresher@chass.utoronto.ca
http://www.chass.utoronto.ca/~dresher/

William J. Idsardi, Department of Linguistics, University of Delaware, 46 East Delaware Avenue, Newark DE 19716-2551, USA
idsardi@udel.edu
http://www.ling.udel.edu/idsardi/

0. Introduction

Martin Haspelmath's programmatic article seeks to show that "observed adaptive patterns in language can be explained through diachronic evolutionary processes" and that "linguistic adaptation is in many ways analogous to biological adaptation". We understand that Haspelmath is not discussing the genetic evolution of the language faculty in the human species (another topic of current interest; see Hurford et al. 1998). Rather, he hopes to reconstitute evolutionary explanations in a non-biological context. Although the article raises a number of interesting issues which may well stimulate further thinking in this area, we will argue that it falls far short of demonstrating that adaptation is the major force behind language change. Secondly, we will argue that the proposed research program will still require a theory of universal grammar (UG) and of learning. Moreover, given that there already exist explanations of language change in terms of these concepts, there may be no need for further explanation in terms of adaptation.

A substantive program of diachronic adaptation must articulate precise analogs between language change and established evolutionary mechanisms in genetics. At a minimum it must provide analogs of the following three facets of evolutionary theory: 1) variation; 2) adaptation via reproductive fitness; and 3) heritability of traits. Once these goals have been met, actual evidence must be given showing that languages evolve using these mechanisms. Let us consider these points in turn.

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1. Variation

We take for granted that variation in language use exists, and plays an important role in language change. This variation must have a source. In biology, the usual source is random genetic mutation. Though Haspelmath does not discuss the sources of variation in language, several are familiar from the literature.

One source of variation is borrowing through language contact. Of more interest to our current discussion is variation generated by language learners in the course of acquisition. This kind of variation comes about through what has traditionally been called analogy, or overgeneralization, or imperfect learning (Berko 1958, Kiparsky 1982). For example, language learners who know the present tense form of the verb *bring* and who are not familiar with the past tense form may spontaneously produce *brang* or *bringed*, by applying general rules of the grammar. We need a theory of acquisition to account for this source of variation.

Turning to a case discussed by Haspelmath, we know of no evidence pointing to the existence of the form *[kætz] 'cats' in the history or prehistory of English or any Germanic language. Thus, the choice of the actual form [kæts] is not an example of variation leading to selective adaptation. We suggest that there is a principle of UG operating here, preventing codas from having more than one phonologically distinctive voicing gesture. But this principle does not emerge directly from phonetic factors such as articulatory effort, because low-level phonetic processes in English produce such clusters word finally, though they are non-distinctive; e. g. [bi:jds] 'beads'. There is, then, no selective adaptation here, only conformity to a UG principle that ensures a formal simplicity in certain phonological structures.

2. Fitness

Once variants are in circulation, adaptation through selection requires a measure of fitness. In biology, fitness is reproductive fitness, though substitute measures (foraging rates, bone strength, etc.) are often employed, supplemented with arguments as to how the substitute fitness measures relate to reproductive fitness. Haspelmath offers no externally validated measures of fitness, but rather appeals to intuitive ideas about what is good (or easy) for the speaker or listener. Indeed, he sees "no need to go into the details of what exactly makes language structures 'good' for speakers and hearers." Weighing the various "cost factors" against the "benefits" is of course a difficult problem, but without concrete proposals, the theory remains too vague to be properly evaluated.

One might suppose, for example, that Berlin and Kay's hierarchy of colour terms would be an excellent candidate for this type of account, since we know the physical and psychophysical bases of colour perception, and therefore we should be able to provide a well-grounded external motivation for the hierarchy. The problem is that the hierarchy red > green/yellow > blue does not directly follow either from properties of the retinal cones, which are tuned to approximately red, green, and blue, or from the two-dimensional colour space, which has a red-green and a yellow-blue dimension. It thus remains to be shown that the linguistic colour hierarchy has a functional motivation external to language. And if this case, which involves well understood language-external neural and physical mechanisms, eludes an adaptive functional explanation, what are the prospects for providing such an explanation in cases which are less well understood, and which have less obvious external correlates?

Many cases appear to simply resist explanation in terms of fitness. For example, Idsardi 1997 shows that much of the diachronic development of Hebrew has resulted in increased opacity in the surface patterns of the language (see also Ravid 1995, Bolozky 1978). It is hard to imagine how increasing opacity contributes to fitness of any kind, and yet diachronic developments commonly increase opacity. Principled and natural explanations of this phenomenon already exist in terms of generative phonology, as the addition of rules to the end of the grammar (Halle 1962) interacting with the simplicity metric (cf. Sober 1975 on generative phonology and Sober 1988 on evolutionary biology).

Looking further at Haspelmath's example of devoicing in coda, the historical record shows that Primitive Germanic had voiced codas that were devoiced in some dialects, such as Middle High German. In Yiddish, however, voiced codas were subsequently restored. Such oscillations have been observed in biology, but it is not at all clear what circumstances might have arisen, in some dialects but not others, to make devoicing adaptive at one point and maladaptive at the next stage. It remains to be shown that adaptation plays a role here. The Yiddish developments, for example, are understandable in terms of a theory of how learners acquire a grammar from available data. It can be shown that the loss of final schwas in Yiddish made the devoicing rule so opaque that learners were unable to acquire the rule (King 1976). A parallel case involving the loss of Middle English vowel-length alternations is discussed by Lahiri and Dresher (1999). Once again, the major source of explanation is the learning theory and UG, not diachronic adaptation.

3. Heritability

Inheriting traits from parents is not much of a topic in evolutionary biology because the answer is so simple – traits are inherited by sexual or asexual reproduction. For language this is another problem entirely. The next generation acquires its language by learning it from the evidence available in the

linguistic environment. This is obviously a much more complicated matter than sexual reproduction, as the child has many more "parents" in the speech community.

Since the advent of generative grammar, diachronic developments have been explained through the acquisition of grammars by successive generations of learners (Halle 1962). The evidence available to the child, along with the genetically endowed UG, interact to produce testable, empirically supported theories of diachronic development. Lacking a true analog to genetic heritability, the burden of explanation for sustained diachronic change again falls on transmission from one generation to the next by means of learning.

4. Conclusion

In summary, we have serious reservations about the viability of the program of diachronic adaptation as outlined by Haspelmath. To the extent that it can be developed into an explicit proposal, it appears that, rather than replacing generative models of diachronic change, it in fact must incorporate exactly these models. However, once explanations in terms of UG are taken into account, it is not clear that there is anything left for diachronic adaptation to accomplish.

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On the survival of the fittest grammar (theory)

Hubert Haider

Institut für Sprachwissenschaft, Universität Salzburg, Mühlbacherhofweg 6, A-5020 Salzburg/Austria
hubert.haider@sbg.ac.at

Grammatical optimality (in terms of OT) correlates with user optimality as a consequence of diachronic adaptation. This is the central claim of the paper under discussion. What is not evident, however, is whether the combination of two hitherto insufficiently understood notions – *grammatical optimality* and *user optimality* – is likely to further better understanding. This is what I want to point out in more detail below.

Presently, OT is notoriously wanting a theory of possible constraints and a theory of possible rankings (see example discussion below). A demonstration to the extent that for a given set of phenomena there is at least one hierarchy of constraints that yields the desired output is inconclusive, if there are no constraints on constraints and rankings. If one may recruit constraints for free, the method proves the creativity of its user. So, the reference to OT cannot be essential for the points made in the paper. GB-, HPSG-, or LFG-constraints could have been used with the same right.

Second, universal user optimality is an informal concept with appeal to common sense. The wording used in this context (sect. 3, tb. 3) refers to *ease* or *difficulty*, *cost* and *help*. But presently, there is no independent *theory of cognitive processing* which would tell us whether a particular linguistic expression E is cognitively more easy or more difficult to process and whether a grammar G that requires the particular form of the expression E is preferred to a grammar G' with E' instead of E. Moreover, user optimality is a heterogeneous issue because the user is either recipient or producer. The result is often a conflict: "Leaving material in canonical positions helps the hearer", but "Leaving out function words reduces pronunciation costs for the speaker" (see sect. 3, tb. 3). Should one leave material in its canonical position or leave it out?

Third, the claim that "whatever adaptive structures we find synchronically must have their origin in an adaptive diachronic change" (fn. 10) is precarious: It is void, or begging the question, or trivial. It is void without a theory of selection (see below). It is begging the question if it is conceded that only a subset of diachronic changes is adaptive: If, as the author admits, there are changes that "can hardly be described as adaptive" (fn. 10), adaptivity is in the eye of the beholder. A change may happen to seem 'adaptive' by accident, and we could not tell. The corresponding problem in evolutionary biology is the circularity of the notion 'survival of the fittest'. Eventually, it is trivial, if a change is a triggered change: If change C_i provokes a subsequent change C_b because C_i produces an ill-formed side effect, C_b is 'adaptive', or rather corrective. Without C_b, the grammar would be ill-formed, that is, in conflict with universal principles, and therefore a defective system.

So, it seems, the real issue of the paper is not OT, and not diachronic change, but only their potential role for *functional explanation* in terms of 'adaptive changes': "Grammatical structures are typically adapted to language users" needs in a highly sophisticated way" (sect. 8). My doubts have been anticipated by Wittgenstein [PU §97]: If someone says ‚Had our language not this grammar, then these facts could not be expressed" [*that easily* $_{H.H}$] – then one should ask oneself, what 'could' could mean here.

The functionalist perspective is a sibling of the unreflected everyday life perspective. The design of an object is seen and described in terms of its present function. Questions of the kind "What is X good for" are potentially misleading in scientific contexts, however, if the respective answer is mistaken for an explanation of a property of X. The author acknowledges this fallacy (sect. 4) of functional reasoning and suggests an analogy instead: "As in biology, observed adaptive patterns in language can be explained through diachronic evolutionary processes as the unintended cumulative outcome of numerous individual intentional actions" (s. abstract).

What is crucially missing in this discussion is a precise notion of *selection*. Adaptability is relative to the selection mechanisms and these are diverse. Without a precise characterization of *selection, adaptation* is as insignificant a notion as percentage figures without baseline, as the following 'syllogism' demonstrates: (i) a is *adaptive* $=_{def.}$ α serves the user's needs. (ii) If the user uses the grammar G for his needs, the users' needs are served by G. (iii) So, G is adaptive if used. Obviously, the notion of serving needs is too vague.

Let us examine two examples in this context. One is topic drop, in sect. 6.3. An empirically inadequate OT-constraint (*leave arguments coreferent with the topic structurally unrealized*) is translated into an equally inadequate 'user-optimal' version (*save production energy and drop a predictable subject pronoun*). These suggestions confound *topics* (topic drop) and *subjects* (pro drop), overlook that pro drop is contingent upon cliticization and not upon rich agreement (see Haider 1994: Icelandic has as rich a subject agreement paradigm as Italian, but it is not pro-drop) and completely ignore structural conditions: German allows topic drop for subjects or objects (1 a), but only in the operator position (cf. 1 a vs. 1 b), but German is not pro drop (1 b).

(1) Question: Wie findest Du sein OT-Argument? – How do you find his OT-argument?

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a. $\emptyset_j$ kann ich e_j leider nicht akzeptieren. $\emptyset_i$ habe e_i mich aber gut amüsiert damit.
b. *Damit habe $\emptyset_T$ mich gut amüsiert

'*Saving production energy*' as a user constraint is best fulfilled if wireless telephone is abolished, but it cannot replace principles of grammar, neither in structural nor in functionalist theories. In my view, the driving force of elisions is not energy saving but sheer ennui. The brain simply hates being bored with redundant information.

Another illustrative case is the constraint on the order of wh-elements discussed in sect. 7. For the author, these restrictions are an expected outcome of diachronic adaptation. For me, this is rather the adaptation of an incorrect functionalist argument: The claim that in our universe of discourse it is '*unnatural in normal circumstances to sort purchasers on the basis of purchased items*' (Kuno & Takami) is just ad hoc and patently incorrect: (21 c) – *What was ordered by whom?* – would be ruled out. 'The fundamental misunderstanding' is on the author's side if he thinks any insight has been gained if one is told German and Japanese differ from English because some unmentioned constraints are ranked differently. It is a fundamental misconception within OT to treat constraints as a set of black box primitives. There is no *superiority constraint*. This is just the surface appearance of a *complex interaction* of structural and semantic conditions (see Haider to appear for details). Shuffling descriptive statements called constraints is not a valid alternative for systematic analyses of grammatical dependencies.

As for a reasonable cognitive selection device in the context of grammar, an answer is suggested in Haider (1997). The device is the human language faculty (HLF): The grammar variants 'compete' for the brain. The winner is the grammar that happens to end up as a copy in the brain of the L_1-learner. The analogy to biology is close: Evolutionary success is reproductive success. A grammar is like a cognitive 'virus'. It reproduces itself in the respective host system: The core grammar G_i is selected by the HLF. G_i determines L_i (minus periphery), which is the basis for the acquisition of a new copy of G_i. Like in biology, the copy-mechanism is not perfect. This is one of a number of sources of variation that feed the pool of variation of grammars for L. HLF is the selection device which sets the standard of success: The grammar that fits HLF best wins, and survives (see title). Note: No adaptation without selection. No consistent grammar without HLF.

Functionalism yes, biologism no

Esa Itkonen

Department of Linguistics, Henrikinkatu 2, FIN-20014 University of Turku, Finland
eitkonen@utu.fi

As I see it, Haspelmath pursues two distinct goals in his paper. First, he wishes to reinterpret the constraints of Optimality Theory (= OT) in functionalist terms, more precisely in terms of 'user optimality'. Second, he wishes to interpret user optimality as a form of 'linguistic adaptation' (= „grammatical structures are adapted to the needs of language users"), with the understanding that „linguistic adaptation is in many ways *analogous* to biological adaptation" (emphasis E. I.).

It seems obvious to me that Haspelmath achieves his first goal perfectly well. His functionalist reformulations of OT constraints are convincing, and he even manages to show that the representatives of OT have themselves been groping for some sort of functional motivation for their constraints. By contrast, it is doubtful whether Haspelmath achieves his second goal. It is this aspect of his paper that I will discuss in what follows.

There is a natural urge to view a less well known (or less prestigious) phenomenon as *analogous* to a better known (or more prestigious) one. The history of science is full of examples of this type of analogy-making. Some such attempts have been successful, others less so. As instances of unsuccessful analogies I mention the following. Aristotle extended the notion of purposive action (e. g. house-building) to inanimate nature (cf. Itkonen 1991: 181–182); Spinoza extended Euclidean axiomatics to ethics; Hobbes and Hume extended, respectively, Galilean and Newtonian mechanics to the explanation of human behavior (cf. Itkonen 1983: 298–302); Toulmin (1972) extended the notion of natural selection to the explanation of scientific progress. Today Haspelmath, Croft, and others (mentioned in References) would like to apply the biological analogy to diachronic linguistics.

For my part, I cannot see how they could succeed. As biological analogues to linguistic change, Haspelmath offers the development of antifreeze proteins in some fish species and the development of long necks in giraffes: in both cases "the useful genes spread in a species because of the greater reproductive capacities of their bearers". In just the same way, Haspelmath argues, in the linguistic domain "frequency of use is determined primarily by the *usefulness* (or '*user optimality*') of linguistic structures". There is, however, a fundamental

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*dis*analogy between the two types of cases. In the case of fish or giraffes the 'usefulness' (of genes) is not experienced, but in the case of speaker-hearers the usefulness (of linguistic structures) *is* experienced, at least unconsciously. Now, it is precisely this experience, and nothing else, which *explains* why people change (or forebear to change) their language in certain ways and not in others.

Saying that a given linguistic structure is (experienced as) 'useful' really amounts to saying that it is (experienced as) a *means* to achieve some *goal.* (Haspelmath mentions the goals of 'saving production energy', 'avoiding articulatory difficulties', 'eliminating threats to comprehensibility', 'avoiding ambiguity' etc.) Thus, we have here the means-end schema characteristic of human behavior in general (for a very extensive discussion, cf. Itkonen 1983). Notice also that this is how 'causal' and 'teleological' aspects of human behavior are reconciled: *Because* (= 'causal') I have the *goal* (= 'teleologial') G and believe that the action A is a *means* (= 'teleological') to achieve G, I set out to do A; and in language, in particular, social control determines whether A will be accepted (or imitated) by the community (cf. Itkonen 1983: 49–53, 201–211, and 1984).

There is, in other words, an *application of intelligence* in linguistic change which is absent in biological evolution; and this suffices to make the two domains totally disanalogous. This becomes even more obvious when, instead of OT-type constraints, we consider such a prototypical diachronic-linguistic process as grammaticalization (which may produce structures conforming to OT-type constraints as end results). There is today a general consensus to the effect that grammaticalization is a two-stage process consisting of reanalysis and extension. The former is an instance of *abduction* whereas the latter is an instance of (analogical) *generalization.* It is impossible to deny that abduction and generalization are cognitive processes, ultimately serving the goal of problem-solving, which intelligent entities like humans *must* perform all the time, but which biological entities like genes *cannot* perform. Trying to eliminate this basic difference leads to confusion.

This is how Cohen (1986: 125) refutes Toulmin's (1972) Darwinist explanation of scientific change/progress: "Hence no evolutionary change of any kind came about through the application of intelligence and knowledge to the solution of a problem. That was at the heart of Darwin's idea. ... And that is why Darwinian evolution is so deeply inappropriate a model ... for the understanding of scientific progress – as if scientific progress could occur without the application of intelligence and existing knowledge to the solution of new problems." In just the same way I am trying here to refute the Darwinist explanation of linguistic change.

An analogy between genes and humans could be understood in one of two ways: either genes behave like humans, i.e. they perform abductions and generalizations; or humans behave like genes, i.e. they lack the capacity to perform abductions and generalizations. Both options should be rejected. Why? – because they are *false*.

Haspelmath is not unaware of these problems. Thus, having offered a Darwinist account of linguistic change, he wants to take it back: he admits that linguistic change also contains intentional elements; and he adds that innovations in the speech of individual speakers are often (?) non-random. However, no consistent picture emerges from this (apparent) compromise. The inconsistency becomes explicit in the abstract of the paper, where it is claimed that biology deals with intentional actions. This is most emphatically not the case.

In sum, linguistic change as well as its results (which conform to OT-type constraints) can be exhaustively characterized in psychological and social terms. To put it roughly, innovation (not 'variation' in the sense of 'mutation') is psychological whereas acceptance (not 'selection') is social. Adding an account in biological terms brings no new information; instead, it either distorts or eliminates existing, fully validated information. So why should anyone advocate the biological analogy? – because biology is thought to be more prestigious than linguistics (or psychology, or sociology). Unlike traditional Indian or Arab linguistics, modern Western linguistics has always suffered from an inferiority complex vis-à-vis the 'hard' sciences (for documentation, see Itkonen 1978).

To avoid any misunderstandings, it needs to be emphasized that in my view Haspelmath certainly establishes what I take to be his main point, namely a vindication of the functionalist point of view, as opposed to either 'neutral' or openly formalist interpretations of OT constraints.

The role of I-language in diachronic adaptation

Simon Kirby

Language Evolution and Computation Research Unit, Department of Theoretical and Applied Linguistics, University of Edinburgh, 40, George Square, Edinburgh EH8 9LL
simon@ling.ed.ac.uk
http://www.ling.ed.ac.uk/~simon

In this excellent paper, Haspelmath highlights the importance of understanding the mechanism that links, on the one hand, functional pressures on language use, and on the other, patterns of grammaticality within and across languages. The author argues that there is a close mapping between these two disparate aspects of language and demonstrates this within the framework of Optimality Theory. This is particularly interesting since OT might be considered to be a typical generative theory of language. As such, there is no theory-internal reason to expect OT constraints to map well onto pressures from language use. Haspelmath argues convincingly, however, not only that OT constraints do indeed match functional pressures, but even that OT theorists actually justify particular constraints with reference to function.

The mechanism that links pressures from language use to OT constraints that Haspelmath proposes is diachronic adaptation. In this perspective, the fit of patterns of grammaticality to functional pressures is due to a historical process of *selective persistence of linguistic variation.* Although I believe that he is right to appeal to this type of mechanism, I think we should be very careful about what Haspelmath claims this reveals about the nature of an innately specified UG. To see why, we need to examine exactly what has to be in place for an adaptive model to work.

In a dynamical view of language, information is continually being transmitted through two different domains of representation: the internal domain (competence, or I-language) and the external domain (performance, or E-language). Any variants that appear in performance can only persist over time by being transformed into I-language via acquisition. Similarly, any variants that exist as alternatives in the competence grammar of a language can only persist over time if they are transformed into E-language by actually being used by speakers. These transformations, then, act as *bottlenecks* on the transmission of language over time and impose selective pressures on the diacronic evolution of language (see e.g. Kirby, in press; Hurford, in press for discussion of bottlenecks). It seems reasonable to assume, as Haspelmath argues, that user optimality affects the selection of linguistic variants over time in this way and thus leads inevitably to the adaptive fit of I-laguage grammars to the pressures of performance acting on E-language utterances.

But there is something wrong with this picture. User optimality constraints work by affecting the relative frequency of use of different variants (either in production, through speaker choice, or possibly during acquisition through filtering by the parser). Adaptation occurs when these relative frequencies become entrenched as fixed grammatical patterns. In general, however, this will only be possible where there is a way for grammars to express the relevant patterns. We have no reason to suppose that this will always be the case. In other words, why should we assume that the domain of I-language is a blank slate onto which any useful constraints can be written? For example, consider dative shift in English:

(1) I gave $_{NP_{ACC}}$[a copy of our latest CD] to $_{NP_{DAT}}$[the man].

(2) I gave $_{NP_{DAT}}$[the man] $_{NP_{ACC}}$[a copy of our latest CD].

Hawkins's (1994) processing theory suggests that variants such as 2 are easier to parse than 1 whenever there are fewer words in NP_{DAT} than NP_{ACC}. We do not expect, however, that this preference will end up being coded in grammars as a constraint that a construction like 2 is *only* grammatical when length of NP_{DAT} is lower than NP_{ACC}. For example, the sentence 3 is certainly grammatical.

(3) I gave $_{NP_{DAT}}$[the boss of the record company] $_{NP_{ACC}}$[our CD].

This is because grammars do not encode constraints that explicitly count numbers of words. Instead grammatical constraints typically seem to refer to formal *types* of construction rather than quantitative properties of those constructions. Of course, if we were to find that, for some reason NP_{DAT}s were typically shorter than NP_{ACC}s then we might expect that constructions like 2 might become the only option for expressing the dative in English. It is this sort of "recoding" in grammatical terms of functional pressures that we should be on the look-out for when assessing the effect of diachronic adaptation.

To be fair, Haspelmath himself is clearly aware of this problem. As he says, "there are universal properties of language that probably cannot be derived from constraints on language use, e.g. the fact that grammars generally do not contain numerical specification", but believes these "have little to do with the grammarian's everyday work". It may be that these I-language constraints on possible constraints have relatively little impact as the author suggests, but I feel we cannot know how pervasive they are at this stage. For instance, Kirby (1999) gives an example where grammatical patterns of relativisation seem only

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partially to match up with observed processing pressures. Whilst this might seem initially to be evidence against an adaptive account, a closer look at the way in which grammars can encode relativisation constraints reveals that not all the preferred processing constraints can properly be expressed in I-language. Similarly, the dysfunctional examples from Lightfoot (1999) that Haspelmath criticizes in the paper again appear to be related to the ways grammars can express constraints on movement. Haspelmath calls these *incidental* effects, but surely in so doing he is impicitly suggesting that grammars must be constrained *a priori* (i. e. innately[1]) to take a form that forces these particular incidental effects and not others.

A complete theory of diachronic adaptation therefore needs to take into account both the selection pressures on linguistic variants that arise from E-language and the structure of I-language into which these variants must be coded. Perhaps linguistics is in a similar stage now to biology before the neo-Darwinian synthesis; before the bringing together in the mid 20th century of an understanding of the genetic representation of phenotypes (analogous to grammars) and the selection pressures that operated on those phenotypes (analogous to utterances).[2]

The puzzle is how to tease apart the effects of functional pressures and the innate constraints on adaptation in a truly explanatory theory of grammatical variation. Haspelmath mentions Newmeyer's (1991) paper on the evolution of functional innate constraints. If the constraints on I-language themselves had evolved biologically to improve our ancestors ability to communicate (if you'll pardon the teleological shorthand), then this makes things even more difficult. How can we tell if a grammatical constraint evolved diachronically or phylogeneticaly?

Haspelmath's answer is methodological. Diachronic explanations are more proximate in time and are therefore subject to easier empirical testing than biological ones. However, another approach may also be available. Evolutionary modelling (e. g. Kirby and Hurford, 1997; Briscoe, to appear and papers in Briscoe in press) allows us to accurately test the effects various communicative pressures might have had on the evolution of innate constraints. From this modelling effort it is not actually at all clear that the kind of adaptation that Newmeyer envisages (i. e. a direct adaptation of the language acquisition device to the needs of speakers) could have been possible. Instead, the suggestion from this work is that an evolutionary mechanism known as genetic assimilation (or sometimes, the Baldwin Effect) must be implicated. In this view, prior diachronic adaptation affects a subsequent shift in the evolutionary trajectory of the innate constraints on language acquisition. Modelling of this kind is in its infancy, and the plausibility of genetic assimilation in human evolution is a controversial topic in biology. However, the results suggest that diachronic adaptation could have had a role in determining constraints even if some are now innate.

Ultimately, Haspelmath's work is exactly the kind that linguistics needs in order to tackle these questions. A fully worked-out theory of diachronic adaptation will form the cornerstone of an explanatory linguistics, but it should be embedded within a theory of the *a priori* possible states of the objects that are adapting – grammars.

1 It is worth pointing out that stating that there must be innate constraints on the forms grammars can take does not necessarily mean that these constraints must be domain specific or modular; these aspects of the generative approach to language must be assessed separately of claims of innateness.

2 We must, however, be careful of analogies such as these. As Haspelmath points out, diachronic adaptation is a different kind of mechanism from evolution by natural selection (he claims it has closer parallels with Lamarckian evolution). One of the reasons for this is that whereas grammars have to be reconstructed every generation through learning or acquisition, DNA sequences do *not* (they are physically passed on and copied).

Proximate mechanisms vs. causal explanations

Donka Minkova

Department of English, UCLA, 405 Hilgard Ave., Los Angeles, CA 90095/USA
Minkova@HUMnet.UCLA.edu
http://www.english.ucla.edu/minkova/

Haspelmath's paper highlights the overlap between constraints formulated and used in OT and the more familiar functional notions of ease/clarity, or economy/redundancy governing language use. The overlap, he argues, cannot be explained as a natural consequence of the OT constraints, as these are currently conceptualized, nor is it predictable within traditional functionalism. Taking the Darwinian evolutionary pattern of generation, testing, and selection of variants as a conceptual and methodological model, he seeks to identify ways in which language structure and language change can also be viewed as the result of functional selection. The paper calls for a theory of diachronic adaptation and provides a concise emergent view of what such a theory should address and on what terms. The thesis is thought-provoking and appealing from a general philosophical point of view, and particularly relevant to the work of linguists who endeavor to reconstruct earlier language states and explain language change.

My brief comments will focus (a) on the similarities bewteen the goals of the proposed theory and current work in OT, (b) on the practical application of the principle of functional selection in language, and (c), on the difficulty of drawing a clear-cut line between *proximate mechanisms* and *causal explanations* in diachronic linguistics.[1]

The first point is prompted by Haspelmath's statement that "the intuitive coincidence between "good" in the sense of "optimal with respect to OT constraints" and "good" in the sense of "good for the language user" has not been captured in mainstream versions of OT" (§ 1), and that "to my knowledge nobody has so far made an attempt to explain OT constraints in a systematic

1 "A proximate mechanism is an immediate direct cause, while an ultimate explanation is the last in the long chain of factors leading up to that immediate cause" (Diamond 1997: 108).

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fashion" (§ 2). Rhetorically, these may be justifiable stage-setting remarks, and the qualification "mainstream" allows for individual interpretations. However, bridging the gap between the formal approach of OT on the one hand, and vague functionalism on the other, is not a novel idea; in phonology it has never been out of fashion. The paper's message of a new theoretical direction in diachronie studies can be augmented, and possibly modified, by recognizing the rapprochement between the study of quantifiable external functional factors and the modeling of language structure in OT, as developed in the work of e.g. Kaun (1995), Flemming (1995), Hayes (1999), and the rich and enlightening acquisitionist work within OT. Since OT provides for incorporation of the principles of markedness into the theoretical model, it is easy to see how user-based "goodness" has in fact been taken very seriously by the theoreticians of the last decade.

The approach defended in Hayes' ground-breaking phonetically-driven phonology (1999)[2] bridges the gap between functionalism and formalism which Haspelmath is also aiming to do. The important point about recognizing this type of work is that it goes far beyond the more general theoretical functional constructs of *Economy, Phonetic Efficiency, Frequency* (§ 6.6); the work accomplishes synchronic descriptions that conform fully with Haspelmath's vision of how grammatical OT constraints arise. Ideally, therefore, a well-defined synchronic constraint would not be just parallel to, but identical with, the *proximate cause* for linguistic change.

In that context, it is somewhat surprising to read Haspelmath's assertion that "we cannot understand synchronic language without taking into account its diachronic evolution" (end of § 4). As a practicing historical linguist I applaud the appeal to the profession to look more closely into diachronic data and accounts. However, taken literally, the statement runs the risk of going overboard; synchronic models in the social sciences do not have to rely on evolutionary information.[3] Moreover, the inverse proposition is also true: we cannot understand diachronic adaptation without reference to the synchronic language properties. Establishing the external, physical, cognitive, or psychological parameters underlying the constraints defined by OT *synchronically* may not be sufficiently explanatory in terms of language evolution in the most general sense of "evolution", yet in practice we know, and this is confirmed by the methodology and argumentation used by Haspelmath, that the study of

2 The paper was written for the 1996 Milwaukee Conference on Formalism and Functionalism in Linguistics and has been available on ROA since October 1996. I am using this publication as a representative of a whole series of research projects carried out in the 90ies aiming to define the phonetic substance of OT constraints.

3 Once again, the statement may be taken as a purely rhetorical one; my point is then not to criticize, but to use the statement as a reinforcement of the position that the proposed theory and current empirically grounded work in OT share their goals and should recognize and share their results.

adaptive historical change must rely on the well described synchronic structural properties of language. Since grammatical constraints "are ultimately based on the constraints on language users" (§ 3), the theory aims to account for the historical options and choices made by the speakers, and the assumption is that these "lost" speakers share user optimality with us in the best uniformitarian tradition. In that sense empirically grounded and testable *post-hoc* models of synchronic OT constraints are also the best tool for investigating language history.

The programmatic character of Haspelmath's proposal naturally leads to some unanswered questions. One such cluster of questions has to do with the practical application of the proposed adaptive theory. How are we equipped to distinguish between likely and unlikely evolutionary routes? For phonology, the example of possible historical structural variation between the [-z] and [-s] realization of the plural suffix *-s*, (§ 5), admittedly spurious, attributes the selection of [kæts] over the heterovoiced [kætz] to energy-saving assimilation. This is straightforward, though admittedly reductionist – even if it is "good" reductionism in the sense of Dennett (1995: 80–85). But how justified are we to posit an impossible form such as [kætz] at any historical time in the first place?[4] One answer would be: when tracing the evolutionary process we should use reasonably reliable reconstructed language data.

If our recourse is reconstructed language data, however, as in the example of No Voice Coda (§ 6.1), (OHG [tag] vs. MHG [tak]), the difference between the evolutionary, adaptive account and the familiar phonetically-based accounts disappears, and with it the difference between the intended *explanation* and the identification of a *proximate mechanism*, namely the difficulty of pronouncing voiced obstruents in coda position. In other words, adding the evolutionary dimension to No Voice Coda, while certainly reassuring in terms of the solidity of the constraint and the correctness of our reconstructions, has not explained why the non-optimal [tag], generated through random variation in the first place, survived for a long time. What allowed No Voice Coda, to be violable for a long historical period?[5] The logic of the situation dictates that there should be

4 In fact, the reconstructable form is [katəs]. Poking further into the details of the example we might ask how and why a violation of DepIO (Tableau 1) is tolerated for *bushes* but appears fatal for *stones*? It is not immediately clear how a theory of adaptation will handle the problem without the crutches of local reranking, but that again would take us into the realm of *proximate* mechanisms, and not ultimate causation.

5 Another way of dealing with the OHG situation would be to posit two contradictory constraints, No Voice Coda and Voice Coda, the latter reflecting the randomly generated and apparently well integrated [tag]. Haspelmath rejects the possibility of contradictory constraints, but such constraints have, in fact been posited and used in OT. More specifically, the Onset constraint referred to in the paper has been argued to co-exist with No Onset (Hammond 1997: 52–54), and their ranking with respect to each other determines the winning form.

an opening gambit in the process of selection of options, but the real conundrum is the postulation of an option which runs against the *post-hoc* knowledge we have accumulated. If user optimality is universal, how are we to deal with the coexistence of two conflicting forms without invoking constraint ranking and re-ranking? The two phonological examples I have cited do not make it clear how the adaptive approach proposed in the paper elucidates the constraints; it is rather the case that the constraints and their violability help us understand the historical process. Or is this just a benign loop helpful to both the synchronic and the diachronic enterprise in linguistics? One would like to think so.

My last point is a reprise of the observation that the theory needs to be more specific about the line between *proximate mechanisms* and *causal explanations*. A charge often leveled against practitioners of other fields who take evolutionary theory as a model, such as biology, history, human behavior, is that the distinction between the two is overlooked, if it is present in the researcher's mind at all (Diamond 1997: 108–109). Haspelmath presents his theory with the intention of providing causal explanations, yet in practice, user optimality is accounted mainly in terms of proximate physiological, cognitive, psychological, and even social mechanisms. Saying that No Voice Coda arose because of the physical hardship involved in producing a voiced obstruent in the coda does not tell us why a number of other options were not taken to remedy the situation, e.g. vocalization and dipthongization or the addition of a vowel. The difficulty we still face, I believe, is that of drawing the line between Darwinian type evolution and what we know as individual mechanisms and patterns of language change. However, for most of us, practitioners of language reconstruction, projecting present linguistic knowledge into the past, identifying and theorizing about the proximate mechanisms of change is a respectable professional undertaking. This is not to say that the formulation of a coherent theory of diachronic adaptation which offers empirically verifiable explanations of linguistic evolution is not a most desirable goal.

On Common-Sense Justifications of Optimality-Theoretic Constraints

Gereon Müller

Universität Tübingen, SfS, Wilhelmstr. 113, 72074 Tübingen
gereon.mueller@uni-tuebingen.de

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§1 Haspelmath notes that optimality-theoretic constraints that have been proposed in the literature often seem to lend themselves to a functional explanation; sometimes, they are in fact explicitly justified by invoking functional notions. He correctly points out that "strictly speaking, [functional] justifications are irrelevant in a theory that assumes innate constraints" (§ 2), as is the case in standard optimality theory.[1] Haspelmath concludes from this that grammatical constraints result directly from functional constraints on language users, via a process of diachronic adaptation. A different conclusion must be drawn if a direct functional motivation of optimality-theoretic constraints turns out to be neither necessary nor possible. On this view, grammar is autonomous, and a constraint qualifies as "good" if it meets the requirement of theory-internal elegance. This concept includes generality of application and simplicity of formulation (e.g., avoidance of Boolean operations in constraint definitions), but not functional, common-sense justification. I will try to defend this latter view here, based on evidence from syntax.

§2 First, the user constraints that Haspelmath postulates as functional motivations for optimality-theoretic constraints are typically very different in formulation and scope. This holds, e.g., for Grimshaw's (1997) STAY and Haspelmath's corresponding user constraint: The former prohibits any instance of movement, and is violated regularly in well-formed sentences; the latter blocks non-canonical linearization that creates potential ambiguity (not movement per se), and is thus violated less often. Clearly, the two constraints make different predictions: Both are (fatally) violated in examples involving multiple *wh*-movement in English (**I wonder who what gave to Mary*). However, whereas STAY also successfully rules out the same structure in German, the correspon-

1 This clear statement of the "pseudo-functionalist fallacy" that seems to be fairly widespread among (non-functionalist) grammarians (and perhaps particularly those working in optimality theory) is spot-on, and should be taken to heart, independently of what one thinks of Haspelmath's own proposal: There is no room for functional, common-sense motivations of constraints that one believes to be innate.

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ding user constraint does not: Because of explicit Case morphology, there is no ambiguity (**I frage mich, wer wem (daß) ihn vorgestellt hat*). Moreover, we expect that string-vacuous movement that does not create non-canonical linearization does not violate user-optimal STAY, and this would fail to exclude string-vacuous raising of the *wh*-subject and the auxiliary in subject-initial questions in English (*Who$_1$ will$_2$ t_1 t_2 leave?*), in contrast to Grimshaw's original STAY. Finally, Haspelmath holds user-optimal STAY responsible for the absence of free word order in English (cf. his (15-ab)). Given rich Case morphology, free word order structures in languages like German, Old English, Russian, and Korean do not violate this functional constraint. Thus, Haspelmath accounts for variation with respect to free word order without recourse to different constraint rankings in syntax – middle field-internal violations of user-optimal STAY are assumed to be fatal. This then raises the question of why Bulgarian, which has an impoverished Case system that is very similar to that of English, exhibits free word order structures of roughly the Russian type (Molxova (1970), Rudin (1985)). Here, it looks as though the only solution might be to assume that violations of (user-optimal) STAY are not fatal in Bulgarian after all – but if this is so, (user-optimal) STAY as such cannot be the reason for the absence of free word order structures in English.

Problems of this kind abound. E.g., the functional reconstruction of Grimshaw & Samek-Lodovici's (1998) DROP TOPIC in effect makes this constraint inviolable: Languages with rich subject agreement respect DROP TOPIC by omitting a topical subject pronoun, and other languages respect it vacuously by not omitting a topical subject pronoun. This is not compatible with the use of DROP TOPIC in optimality-theoretic analyses, where this constraint is counterbalanced by a requirement to overtly realize subject pronouns – so English can violate DROP TOPIC to fulfill the latter requirement.[2] Since discrepancies between standard optimality-theoretic constraints and their functional reconstructions occur systematically, we face a dilemma: If a given grammatical constraint is directly equated with a postulated user constraint, the analyses which originally motivated the grammatical constraint must be abandoned; but if the grammatical constraint is merely viewed as the indirect consequence of a postulated user constraint that "underlies" it, there must be an additional, unexplained mechanism that translates user constraints into grammatical constraints.

§3 Haspelmath states that "there is probably no need to go into the details of what exactly makes language structures 'good' for speakers and hearers, i.e.,

2 Similarly, the idea that Pesetsky's (1998) TELEGRAPH might subsume DROPTOPIC (§ 6.3) is incompatible with actual optimality-theoretic analyses because TELEGRAPH forces PF-deletion of existing syntactic material, whereas DROPTOPIC forces the absence of syntactic material.

what constitutes user optimality ... The most important cost factors are motor costs and cognitive processing costs, and the most important benefits are informativeness and persuasiveness" (§3). I am not convinced that a precise characterization of what counts as a functional explanation is unnecessary. In the absence of clear, independently verifiable criteria, functional explanations will often simply be common-sense justifications and thereby run the risk of being post hoc (it is usually not hard to contrive *some* functional motivation for almost any given constraint) and arbitrary (in the case of grammatical constraints that are rejected on functional grounds). Indeed, it turns out that psycholinguistic evidence may very well contradict our common-sense view regarding "processing costs." Consider again STAY. In its original formulation, this constraint blocks movement. A straightforward functional reconstruction would be to assume that each movement operation is costly for the parser. This, essentially, is the derivational theory of complexity of the 60's, which was not strongly supported by psycholinguistic evidence. Another functional reconstruction is the one envisaged by Haspelmath: What is costly for the parser on this view is a non-canonical linearization of (Case-) ambiguous elements. However, this hypothesis is not confirmed by psycholinguistic experiments. Schlesewsky et al. (1997) show that locally unambiguous object-initial *wh*-questions exhibit higher reading times than locally unambiguous subject-initial *wh*-questions; i.e., we find the same pattern as with locally ambiguous *wh*-questions. This suggests that what is costly for the parser is in fact path length (or non-minimal links), and whereas such notions may indeed play a role in grammatical theory (cf. Chomsky (1995), and Fanselow (1999) for a direct application), it is shown in Müller (to appear b) that a version of STAY that is based on path length is fundamentally incompatible with the optimality-theoretic analyses given in Grimshaw (1997), Müller (1997b), Costa (1998), Legendre et al. (1998), Vikner (to appear), and elsewhere. Again, the problem is more general: There will often be incompatibilities of common-sense justifications and the actual psycholinguistic evidence.

§4 Despite Haspelmath's claim that "most of the widely used, non-ephemeral constraints can be reformulated in user-optimality terms" (§3), there are many cases where this is far from obvious. This holds for the whole class of (left- and right-) alignment constraints in syntax that have been taken over from phonology. Such constraints are already present in Grimshaw (1997) and Pesetsky (1998), and they have come to play a crucial role in much recent work (cf. Costa (1998), Legendre (1998), Samek-Lodovici (1998)).[3] The same goes for Grimshaw's (1997) OB-HD that requires the presence of visible heads in projections; for Legendre et al.'s (1998) constraints on absorption and adjunction that regulate *wh*-dependencies; for the binding-theoretic constraints in Wilson (to appear), the Case and Person constraints in Woolford (to appear) and Aissen (to appear), respectively (one may claim that 1./2.Pers. is treated differently from 3. Pers. in functional terms, but this does not yet explain why there are constraints against both); and so on. An important class of constraints that lack obvious user constraint counterparts are markedness constraints that trigger displacement. A constraint like the WH-CRITERION (Grimshaw (1997), Müller (1997b), Legendre et al. (1998), Ackema & Neeleman (1998)) forces *wh*-phrases to be in SpecC. Over the years, a substantial body of evidence has been accumulated that argues against a semantic/functional motivation of *wh*-movement, and for a purely formal, grammar-internal motivation: *Wh*-phrases are not necessarily operators (Pesetsky (1987), Berman (1991)); some cases of *wh*-movement must partly be undone semantically (Beck (1996)); all *wh*-phrases can in principle be interpreted in situ (Groenendijk & Stokhof (1982)); *wh*-phrases may respect the WH-CRITERION by undergoing partial movement to an embedded non-scope position (van Riemsdijk (1982)), etc. A SUBJECT constraint (Grimshaw (1997), Grimshaw & Samek-Lodovici (1998), Costa (1998)) forces subjects to be in SpecI, with no grammar-external motivation in sight. A V/2-CONSTRAINT triggers filling of left-peripheral specifiers in V/2 languages, without any functional restriction on this position: SpecV/2 can be interpreted as topic, as contrastive or completive focus, via reconstruction, or not at all (expletive insertion). Even the PRONOUN-CRITERION (Müller (to appear a)), which forces unstressed pronouns to be in the Wackernagel position (*Ich habe (es) gestern dem Fritz (*es) gegeben*), does not necessarily provide evidence for a functional motivation. At first sight, the constraint appears to be derivable from Behagel's *Gesetz der wachsenden Glieder*, which Hawkins (1990) considers a parsing strategy (also cf. Primus (1994)). However, there is psycholinguistic evidence both in favour of such a principle (cf. Hawkins (1990) and references cited there), and against it (cf. McDonald et al. (1993), a study that minimizes intervening factors).

§5 So far, I haven't said anything about the model of diachronic adaptation. If the considerations in §§2–4 are on the right track, the issue of direct adaptation does not arise because grammatical constraints cannot directly be based on user constraints (and many grammatical constraints cannot have a functional basis at all). This leaves as possibilities indirect adaptation of a subset of grammatical constraints, and complete independence. At present I take it to be an open question whether an indirect influence of user constraints on some grammatical constraints does exist. For the sake of argument, suppose that it does (perhaps in the case of ANIM > INANIM, assuming that a functional counterpart can be

3 The complex version of Pesetsky's complementizer alignment constraint LE(CP) that is criticized by Haspelmath as a theoretical construct may also fail to meet the requirement of theory-internal generality and simplicity, but this does not hold for the simple version that suffices for most of the data.

motivated independently of intuitive plausibility).[4] Then, diachronic adaptation may indeed be a better model than biological adaptation (it evades the teleological fallacy and is less speculative). On this view, an optimality-theoretic grammar would consist of a core of innate constraints without functional motivation, and a periphery of constraints with a functional basis, with both constraint types interacting through violability and ranking. Needless to say, though, this is pure speculation; it may just as well be the case that no grammatical constraint has any functional basis.[5] Given the lack of decisive evidence, my conclusion is that, for the time being, the (generative) grammarian is well advised to concentrate on how questions, and postpone why questions until our understanding of cognitive issues has deepened, and these questions can be answered (or even asked) properly.

4 In contrast, RECOVERABILITY seems more like a meta-constraint on grammars than like a genuine grammatical constraint; so the question of a functional justification does not arise. Compare Chomsky's (1965) statement that this constraint "need never be stated in the grammar, since it is a condition on the functioning of grammars" (p. 255), and Pesetsky's (1998) assumption that, unlike other optimality-theoretic constraints, RECOVERABILITY can never be violated by a well-formed sentence.

5 This would not preclude looking for "optimal design specification" (Chomsky (1998)) in grammar, as long as the goal is maximal generality and simplicity, and not maximal reduction to functional considerations.

Adaptation, optimality and functional explanation: Two serious problems

Frederick J. Newmeyer

Department of Linguistics, University of Washington, Seattle,
WA 98195-4340 USA
fjn@u.washington.edu

I am a formal linguist who has long taken the position that there is no fundamental incompatibility between the program of constructing autonomous models of grammar and that of advancing functional explanations for why grammars have the properties that they have (see, for example, Newmeyer 1991, 1998). I therefore read the target article with great pleasure. In broad outline, Haspelmath puts forward a position quite congenial to mine. Formal principles ('Stay', 'Telegraph', 'Recoverability', and so on) interact to characterize the well-formed sentences of the language. These principles are rooted historically in what Haspelmath calls 'user optimality'. In short, 'grammatical constraints are ultimately based on the constraints on language users' (§ 3). Haspelmath chooses the framework of Optimality Theory (OT) as his grammatical model for two reasons. First, because OT constraints seem to be rooted functionally in a more transparent way than are constraints provided by other frameworks. Second, because the OT mechanism of ranked (and rerankable) constraints is well suited to capturing typological variation.

My feeling, however, is that Haspelmath overestimates the ability of OT to serve the purposes he wishes to attribute to it. After defending this point, I will raise a more general problem pertaining to functional explanation that arises if one adopts the assumptions of the target article.

OT analyses, particular in the realm of syntax, have, in general, been rather circumscribed in their domain of application. Typically, they focus on some little corner of the syntax, such as clitic order, auxiliary inversion, and so on. The machinery intrinsic to the OT approach works beautifully in such situations. But grammars are tightly integrated wholes, so decisions about how one process should be handled are likely to have repercussions for another, seemingly unrelated, process. As I will now argue, this fact leads ultimately to a much greater disparity between grammatical principles and their functional roots than Haspelmath believes to be the case.

Let's posit two hypothetical OT constraints for English, FORM-MEANING ALIGNMENT (FMA) and HEAVY-LAST:

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FMA: Syntactic constituents reflect semantic units.
HEAVY-LAST: Heavy constituents follow light constituents.

FMA can be illustrated by the fact that, in every formal account, adjectives are generated under the same phrasal node as the noun that they modify. HEAVY-LAST can be illustrated by the fact that within the verb phrase, sentential complements are positioned after phrasal complements. The functional roots of these constraints are so obvious that no discussion is necessary.

Now, then, which constraint is ranked higher for English, FMA or HEAVY-LAST? Well, it depends. In some cases, we have identical grammatical elements in variant orders with no meaning difference, each option corresponding to a different ranking of the two constraints. For example, both of the following are grammatical English sentences:

(1) a. A man who was wearing a silly-looking red hat dropped by today.
b. A man dropped by today who was wearing a silly-looking red hat.

Sentence (1 a) reflects a ranking of FMA over HEAVY-LAST; sentence (1 b) the reverse ranking. In some cases, however, only a higher ranking of FMA is possible. Simple adjective phrases cannot be extraposed from the nouns that they modify, no matter how heavy they are:

(2) a. An extremely peculiar-looking man dropped by today.
b. *A man dropped by today extremely peculiar-looking.

And in other cases, HEAVY-LAST seems to be ranked over FMA. When comparatives are used attributively, the adjective is separated from its complement by the head noun, despite the fact that together they serve to modify semantically that head noun (3a – b). Their structural unity can be obtained only in a manner that is consistent with HEAVY-LAST (3 c):

(3) a. That's a more boring book than any I have ever read.
b. *That's a more boring than I have ever read book.
c. That's a book more boring than any I have ever read.

What is the solution to this ranking paradox? Only, I would say, to abandon FMA and HEAVY-LAST as constraints of English grammar. Rather, what we need are something much more like the 'parochial' rules, principles, and constraints of standard models of generative grammar that interact to yield the grammatical sentences of the language. There is a serious problem with such a solution, however, from the point of view of the target article. Consider the relationship between detailed grammatical statements, such as those, for example, that position the constituents of NP and their functional motivations.

This relationship tends to be vastly more indirect than is the case for general constraints such as FMA and HEAVY-LAST. In other words, we are left with the sort of view advocated in Newmeyer (1998), in which, in a global sense, grammars reflect external forces, but without each language-internal grammatical statement being tied to a particular functional motivation (see, for example, the diagram in Newmeyer 1998: 163).

I will close by discussing briefly a consequence of the conclusions of the target article that appear to have serious implications for the program of functional linguistics. Let us suppose that Haspelmath is correct to endorse Croft's idea that 'the propagation of linguistics features [is] exclusively [due] to 'social selection', [rather than] 'functional selection' (§5). Given that social selection and functional selection are independent variables, it follows that in about 50 % of the cases in which two variant forms 'compete', the more functionally motivated variant will be propagated. Now consider *innovations* in speech. Haspelmath suggests that that they are frequently functionally motivated by user-based criteria and I am sure that he is right. But it must be acknowledged that they are often *not* functionally motivated. Haspelmath himself (footnote 10) describes grammaticalization changes, which are rampant in language history, as 'counter-adaptive'. Social selection must also play a major role in innovations, as when speakers adopt a form or construction from a more prestigious dialect or language with which they are in contact. Many such borrowings have dysfunctional consequences, viz. the cumbersome rule for word stress in English that followed the entry into that language of hundreds of Old French words following the Norman Conquest. And many other types of innovations appear to be utterly nonfunctional as well. Consider, for example, the unconditioned fronting of [u] to [ü] in French and a parallel development in Scottish English. So let's say (arbitrarily) that 60% of all innovations are functionally adaptive from the point of the language user. Multiplying the 60 % of user-friendly innovations by the 50 % of user-friendly propagations yields the conclusion that only about 30 % of language changes are ultimately user-based.

Could that be right? It seems to contradict the conclusions of decades of functionalist theorizing (not to mention the premise of the target article) that grammars are, a certain amount of arbitrariness aside, 'designed' for the user. And if it is right, then it would appear that there are important foundational issues in functional linguistics that need to be addressed. If fewer than a third of language changes are functionally motivated, then one would be forced to conclude, along with William Labov (1994) that there has been an 'overestimation of functionalism'. If functionalism has *not* been overestimated, then there is a flaw somewhere in the reasoning that led to my pessimistic conclusion. Haspelmath is capable of pinpointing the flaw, if anybody is.

From a diachronic perspective

Elizabeth Closs Traugott

Department of Linguistics, Stanford University, Stanford,
CA 94503–2150, USA
traugott@turing.stanford.edu
http://www.stanford.edu/~traugott/traugott.html

As a historical linguist I welcome Martin Haspelmath's proposal that "[w]e need to consider diachronic change if we want to understand why OT constraints are the way they are".[1] Haspelmath argues that grammars come to be adapted to speakers' needs in ways that do not suffer from some of the worst aspects of the Teleological Fallacy. This is an argument that deserves to be seriously pursued. But if this pursuit is ultimately to be successful, a number of empirical questions will need to be answered.

(1) What motivates innovation and change? First and foremost is the question „How is it that variants are created from which speakers may choose?" It is a given of OT that differences among dialects and stages of a language can be accounted for in terms of different rankings. But what leads to the creation of the variants in the first place? And what motivates the different rankings? What is it in a particular cognitive and communicative situation that leads to speakers choosing "those variants that suit them best"? In other words, how do speakers' choices interact with the long-recognized mechanisms such as reanalysis, analogy/extension and borrowing (e.g. Harris and Campbell 1995), and the motivations of speed and clarity (e.g. Langacker 1977)? Most especially, what is it that leads to innovation by the individual (which may not lead to change) and spread to the community (which does lead to change)?[2]

1 I am grateful to Brady Clark for stimulating discussion of the issues, and for comments on an earlier version of these remarks.

2 The importance of the distinction between innovation/actuation and spread was highlighted in Weinreich, Labov and Herzog (1968), and has been reemphasized by Milroy and Milroy (1985), Croft (In press).

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(2) Who originates change and who passes it on? Haspelmath does not specify what age the innovators and leaders of change are. One may infer that they are not small children, since Haspelmath appears to conceptualize them as fully able to adapt language to their needs; they may be preadolescents and even adults, on the assumption that the grammars of adults are subject to modification. One may also infer that they are conceptualized as producers/speakers rather than as interpreters/hearers, since they are active users of language (talkers engaged in Keller's maxims of action), not perceivers of it. These perspectives are widely held by functionalist historical linguists (e.g. Hopper and Traugott 1993, Bybee, Perkins and Pagliuca 1994, Croft in press), and also by sociolinguists (e.g. Labov 1982) and psycholinguists (e.g. Slobin 1994). They are, however, diametrically opposed to claims made by some formal generative linguists such as Lightfoot (e.g. 1999), who build on a tradition going back to Halle (1964) and far beyond, that only young children can innovate, and that they do so because they do not necessarily infer (or "abduce", see Andersen 1973) the same system as that of the input. Presumably an adaptive theory focuses not on innovation in the individual young child (who is acquiring a ranking), but on change, and spread through the community of older children and adults.

(3) If constraints are adaptive how should we think about non-change? In some ways the most interesting question for a historical linguist is precisely why change does not happen. One particular phenomenon is that a great deal of language gets sedimented as residue or "junk", only some of which gets to be reused in later changes in a process called "exaptation" (Lass 1990). Sedimentation is a problem for synchronic linguists because it tends to be non-systematic (e.g. morphophonemic alternations like those found in *house-houses* or *foot-feet*). It is a problem for diachronic, especially functional, linguistics, because such "junk" typically does not undergo change: Lass (1997) compares it to the non-functional persistence of male nipples in human biology. It is most particularly a problem for any theory of change that privileges simplification, whether construed as "therapeutic" change (see Lightfoot 1979) or optimization by rule extension/analogy (see Kiparsky 1968). From an adaptive OT perspective such inertia presents a challenge that must be addressed.

(4) Are any changes not adaptive? In Ft. 10 Haspelmath suggests a positive answer: "I am not claiming that all of language change is adaptive and motivated by user optimality". Which changes are to be excluded, and why would it be that they are? When in this footnote Haspelmath

proposes that grammaticalization is a change that is not adaptive and at the same time acknowledges that it "probably account[s] for the great majority of morphosyntactic changes" he potentially undermines his whole enterprise: how effectively can a theory account for "why OT constraints are the way they are" if it does not address the "majority" of some set of changes.

It may be useful to consider under what set of assumptions grammaticalization might be considered not to be adaptive. One possible assumption may be that selection of morphosyntactic categories such as AUX or of rules such as V to I raising is not subject to the constraints discussed in the paper. Another possibility is that despite the concept of competing motivations discussed in Section 1, in fact the assumption is that "less is more", and therefore any increase in choice of members of a category (e.g. increase in the number of modals available), or in structure (e.g. the development of the category COMP) is "inflationary" and therefore not adaptive from an OT perspective. But surely morphosyntactic changes are as much subject to speakers choosing variants that suit them best as any other changes if one takes seriously the hypotheses that (a) some version of the competing motivations "be quick" (for the sake of speaker production) and "be clear" (for the sake of communication with the addressee) is correct, and that (b) internalizing a grammar does not involve only learning a lexicon with salient semantic and phonological components, but also morphosyntax, which provides the "glue" and the hierarchies that signal linguistic as opposed to dictionary meaning.

Consider, for example, the development of the CP in later Germanic and of the DP in Romance as outlined in Kiparsky (1995) and Vincent (1997) respectively. Let us suppose Kiparsky's analysis is correct that Indo-European had two left-peripheral operator positions, the inner hosting focal elements like *wh*-phrases and demonstratives, and an outer position binding a resumptive pronoun in the argument position; in later Germanic as adjunct clauses were reanalyzed as arguments/embedded clauses, indeclinable complementizers were innovated that, though derived from nominal elements such as demonstratives (e.g. *þæt* 'that') and quantifiers lost all their nominal properties. Let us also suppose that Vincent's analysis is correct that in Classical Latin there were no definite articles, only deictic or emphatic demonstratives; that in Romance there was a division of labor between the second person deictic *ipse* and the third person distal deictic *ille*, such that the former came to express focus/contrast and the latter topic/given information; that the weak pronoun *ille* became a clitic; and that finally a new category, DP arose. In both cases, important functional categories were introduced. In both cases there was grammaticalization from a fuller, more lexical element to indeclinable operator function (most instances of grammaticalization involve not just decategorialization of a full lexical item in a construction, but rather the further decategorialization of an already grammaticalized element). In both cases the data demonstrate uncontroversially that the new structure did not arise out of nothing, or as replacement of some putatively "lost" category marker, but out of the discourse uses of semantically susceptible elements.[3] Under what understanding of adaptation is it reasonable to exclude this sort of change from the kinds of phenomena that should be accounted for?

Grammatical operators may not be phonologically salient (hence MAX LEX), but they appear to be informationally and communicatively highly so in languages with certain kinds of word order and morphology. The innovation of grammatical, i.e. functional categories, is an issue that a theory of adaptive OT constraints must come to grips with. Otherwise we will not be able to understand why OT constraints are the way they are or implement a program of research that gives priority to understanding "synchronically adaptive structures ... in terms of a diachronic process of variation and selection".

3 For arguments against recursive cycles in grammaticalization in which a form (e.g. a case marker) becomes zero and is "replaced" by a fuller one (e.g. a preposition) see Lehmann (1985).

Principles of Evaluation, Change and Related Issues

Wolfgang Ullrich Wurzel

Zentrum für Allgemeine Sprachwissenschaft, Typologie und Universalienforschung, Jägerstraße 10/11, 10117 Berlin
wurzel@zas.gwz-berlin.de

Martin Haspelmath's article brings to the forefront the currently much-discussed connection between grammatical structure and grammatical change, that especially in the last decades has proven again and again essential to a general theory of grammar. He is therefore to be commended for explicitly raising this topic of discussion. These issues, as the title shows, are discussed in the framework of Optimality Theory (OT). But unfortunately (and here the criticism begins), the author limits his perspective from beginning to end such that, aside from OT and parts of other generative theories, only certain functionalist approaches are taken into account. All so-called "natural" approaches to grammar, for example the natural phonology of Stampe (NPh) as well as the various versions of natural morphology (NM) and Vennemann's preference theory (PT), remain completely unexamined in terms of content. This is even more dismaying considering the fact that all these approaches have dealt in large part directly with these problems (the references given in this article are proof of this). If the proposals of these approaches contribute to the discussion of these theoretical issues in OT, one should not ignore them. This holds of course in particular for an article that the author himself terms "programmatic" (abstract).

In this paper, I will discuss some central points of the theory Haspelmath proposes and the consequences for this theory in light of previous proposals made by non-OT "natural" theories of grammar, although the discussion will also generalize to the non-theory specific. Other interesting issues, among others the relationship between OT in general and the theory of markedness, cannot unfortunately be disscused here (cf. Hurch 1998 however).

1. Constraints

As is well known, the constraints embody the specifics of OT. They are in the same position as rules were in previous theories, yet have a broader scope than

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rules. They do not simply output the correct forms: they determine as well how 'good' or 'bad' the actual and possible forms are; in other words, they assign an evaluation to them. In this last respect, they are similar to the natural phonological processes of Stampe, where depending upon their application or nonapplication, the evaluation is determined (Stampe 1969, 1972: 1 ff.), as well as the markedness principles of NM (among others, Wurzel 1994: 37 ff.) and the preference laws of PT (Vennemann 1988: 1 ff.) that deal with the direct evaluation of forms couched in terms of markedness or preferences.[1] Although these concepts have a different implementation in their respective theories (i.e., constraints filter out, phonological rules generate, markedness principles and preference laws assign a value), it should be noted that as devices computing the valuation of correct forms, they are equivalent. Thus in what follows, they will all be considered evaluation principles.

Haspelmath starts with the requirement that every individual constraint be independently motivated in order to prevent the use of *ad hoc* constraints found in a number of recent analyses. To this end, he attempts to lead "grammatical constraints" (that he allows a special place for) back to "user constraints" for "what is good from the point of view of the theory is good from the point of view of language users. Grammatical optimality and user optimality are largely parallel" (§ 3). This connection, he claims, has not been previously discussed "in mainstream versions [?] of OT."[2] Haspelmath assumes (and here his own theoretical assumptions come into play) that grammatical constraints develop out of "user constraints" in a diachronic process of adaptation (more regarding this below).

I point out, however, that the connection between 'grammatical' and 'user' optimality (or naturalness/markedness) has not only been recognized for a long time, but has actually been sorted out theoretically, such that 'grammatical' optimality is identical to 'user' optimality (speaker/listener optimality). This is shown, for example, in Stampe's definition of phonological processes:

(1) "A phonological process is a **mental operation** that applies in speech to substitute, for a class of sounds or sound sequences presenting a specific common difficulty **to the speech capacity of the individual**, an alternative class identical but lacking the difficult property" (Stampe 1972: 1; emphasis mine).

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1 The markedness conventions of Chomsky & Halle (1968) should also be mentioned here.

2 I will not discuss this point in any detail, although – "mainstream" or not – in at least the case of Hayes "theory of phonetic grounding" there is indeed a clear justification of phonological constraints in articulatory simplicity and perceptual distinctiveness and therefore the needs of the users as well; cf. Hayes (1999). – I thank my colleague Marzena Rochon for bringing this to my attention.

In addition, also the markedness principles in NM are strictly oriented to the speaker/listener and thus in this way also grounded (Wurzel 1994: 32ff., 55ff.; 1998a). This notion is actually not that novel; theories of grammar (at least since Chomsky 1965) have treated the grammar of a language as the grammar of the speakers/listeners of this language, and of course the universals of UG characterize the language competence of users and only this. Evaluation principles are only conceivably reasonable when oriented to the user. If one wants to adopt additional language-oriented constraints or other evaluation principles, in addition to constraints based on the speaker/listener, then very convincing reasons would be required, reasons that in this article do not become clear.

The crucial point is that every evaluation principle has to be independently justified. It is of little theoretical relevance whether these justifications should be directly incorporated into individual constraints or markedness principles (as in Haspelmath's 'user constraints') or not (as in his 'grammatical constraints') and how exact a motivation for a constraint or principle has to be, for example:

(2) (i) Nasalized vowels are marked in relation to oral vowels.
(ii) Nasalized vowels are marked in relation to oral vowels, because they are more difficult to articulate.
(iii) Nasalized vowels are marked in relation to oral vowels, because they require an additional articulatory gesture for opening the velum.
(and so forth).

The real issue is much more whether the predictions made by a evaluation principle are compatible with a related theory, for example whether the prediction of a phonological principle is compatible with phonetics (articulation and perception), or the predictions of a principle of morphology with semiotics (for a phonetic motivation of phonological principles, see Stampe 1969, 1972; for semiotic motivation behind principles of morphology, see Dressler 1982, 1985b: 279ff.). In this way, principles of NPh and NM are systematically explained, contrary to Haspelmath's criticism of OT ("... nobody has so far made an attempt to explain OT constraints in a systematic fashion" §2), and such an explanation functions of course in the framework of OT too.

In Haspelmath's opinion, a motivation for constraints would be irrelevant for a theory that assumes the innateness of constraints (§2). Furthermore, he claims that the violability of the constraints would make them a poor candidate for "innate devices" (§7). This appears to be a misunderstanding, however: if (for example) nasal vowels are said to be marked in relation to oral vowels, the motivation for this fact would presumably be based on the articulatory make-up of the human vocal tract, that oral vowels are easier to articulate than nasalized vowels. And if non-transparent complex words are more marked than

transparent words, this fact would be based on the semiotic make-up of the human mind, on the basis of the fact that transparent combinations of signs are easier to perceive and compute than non-transparent signs. Both are clearly independent of a particular language. In this sense, evaluation principles are absolutely innate, but also quite explicable.[3] If constraints are to be universal but not innate, then one would have to provide an explantion as to where they might come from, as e.g. Comrie (^{2}1989 [1981: 25f.]) does in his section on "Functional and pragmatic explanations" of universals.

But what are we to make of the argument for constraint violability? This (apparent) violability comes not from the nature of the constraints themselves, but rather from the (in my opinion) generally unfortunate manner in which OT constraints are formulated. So, for example, the constraint "voiced codas are forbidden" does not really mean that voiced obstruents in codas are banned, but rather that they are less optimal or more marked than their voiceless counterparts.[4] As such, they express (like all evaluation principles) preferences and thus at the same time tendencies of language change. And statements of preferences would not be violated if forms with nonoptimal properties arise in a language.

Without a doubt one can agree with Haspelmath that language-specific constraints are impossible: "User optimality is necessarily universal" (§3). If, however, differently structured forms are, for different languages, optimal or preferable, then this could of course be explained in many cases by a different ranking of the relevant constraints. But in fact there are cases where languages prefer certain forms that apparently cannot be justified by a universal evaluation principle. For example, there was the tendancy in Old High German (OHG) in neuter nouns that had a N./A.Pl. form with category marker, to delete this marker and thus adapt to the N./A.Sg. forms (complete transition type: *fazu* > *faz*; partial transition type: *herzun* > *herza* and *lembir* > *lamb*; cf. Braune 1987: 183, 187, 207), in (it should be noted) a strictly synthetic flexional system. This change won out despite running counter to the principle of constuctional iconicity (that states that the more complex concept plural should be represented by a more complex symbolization than the singular), and the principle of form-function (paradigms should symbolize different categories through different forms). The new forms thus appear simply dysfunctional. In what respect are then the new N./A.Pl. forms "better" than the old ones? The answer lies in the language-specific details: the OHG neuters in their overwhelming majority show already before this change a formal correspondence between the N./A.Sg. and N./A.Pl. (type *wort*). This correspondence can be characterized as a system-defining structural property of OHG (Wurzel 1984: 81ff.). The system defining

3 Not explicable, however, are the oft-employed *ad hoc* constraints, whose use Haspelmath justifiably criticizes.

4 Truly banned in comparison would be codas such as [bçw], for clear reasons.

structural properties are referred to by the markedness principle of system appropriateness established in the framework of NM:

(3) A morphological form is less marked according to system-appropriateness the more it corresponds to the system-defining structural properties of the respective morphological system (Wurzel 1994: 64ff.).

This markedness principle thus favors forms that structurally and typologically fit into the appropriate morphological system. It is crucial that the markedness principle has a universal character, but that it favors forms with different properties in different languages. So this principle favors, e.g., in New High German (NHG), noun plural forms, including the neuters, that are formally distinct (without taking into account the articles) from the singular forms, due to the changes in morphological structure since OHG (cf. OHG N/A.Pl. *wort, faz* NHG *Wörter/Worte, Fässer*, but also recent developments in colloquial German such as *Mädchen Mädchens, Fräulein Fräuleins*). This universal markedness principle explains exactly that which the proposed, though not accepted, language-specific OT constraints attempt to explain.

2. Adaptation and Change

A central concept of Haspelmath's theory is that of adaptation: "Grammatical structures are adapted to the needs of users" (§ 3). This is clearly inarguable and – without reference to biology – has been known at the latest since Stampe (1969; Haspelmath cites only Croft 1990). But what does this mean in terms of constraints? Haspelmath asserts that the grammatical constraints "... have arisen from user constraints in a diachronic process of adaptation" (§ 8). Yet the 'grammatical' constraints of each individual language are actually (as Haspelmath emphasizes) universal. They can therefore neither emerge in individual languages (as postualted in the quote above), nor disappear from these languages, if a new generation of speakers acquire the grammatical structure of language and adapt it to their needs. There is no adaptation to the needs of the "user" available if "user constraints" of the type "coda obstruents should be pronounced voiceless in order to avoid articulatory difficulties" (a motivated constraint) are translated into "grammatical constraints" of the type "voiced coda obstruents are forbidden" (a constraint without a motivation). Even if such a translation were possible, this clearly could not lead to language change.

The situation is naturally different if one views the complete system of the constraints. In the OT framework, it is indeed plausible that individual languages and language-states differentiate themselves via different constraint rankings and that there is no universally prescribed hierarchy. This means in addition that the speaker of a language can adapt the constraint hierarchy to their user requirements during language acquisition by reranking. When they do this language change can occur. It is in this sense that one can talk (with justice) about a "diachronic process of adaptation", unlike the case for individual constraints.[5]

Haspelmath's article attempts to explain language change by analogy to biological evolution through the principles of variation and selection. Haspelmath argues that the starting point of language change is variation; selection would then follow. But how does this variation occur? Obviously via change. The speakers/listeners, as the agents of language change, develop new forms that up to now have not existed. These new forms are not however created through random variation as in biology; rather, this process of formation is directional although unconsciously formed (i.e., not planned) by speakers (Wurzel 1997: 297f.; cf. Keller 1990: 181). Change leads away from markedness. The speakers/listeners as active subjects behave economically and avoid marked structures in communication. They replace (more strongly) marked forms with unmarked/more weakly marked forms. Unlike variation in biological evolution, linguistic variation is thus created primarily by selection of new, and in a certain respect "better" forms. Grammatically initiated change (the type of change under discussion here) causes a local improvement of language structure.[6] It should also be pointed out that Haspelmath himself limits the ability of the biological model to explain language change, acknowledging "one important difference" between both: "While the source of genetic variation in biology is restricted to random mutations, the source of linguistic variation, innovation in the speech of individual speakers, is often nonrandom" (§ 5). More precisely, one must say that every grammatically initiated change is nonrandom. Given this, does one really want to claim that "linguistic change is sufficiently [!] similar to biological change"? (§ 4). Indeed, what is at all accomplished given this fundamental difference between the biological paradigm and language change?

5 In Stampe's NPh, these connections are presented differently. Here (somewhat simplistically put) the speakers of a language must repress innate phonological processes or partial processes during acquisition that are not present in their language in order to arrive at the correct language norms. If they do not, then language changes. So, for example, the speakers acquiring of OHG had to (among other things) repress the process of devoicing obstruents at the syllable edge. Around the transition to Middle High German (MHG), speakers ceased to do this: e.g., OHG *lob, leid, tag* > MHG *lop, leit, tac*. The speakers of MHG have then (in Haspelmath's terms) adapted the structure of their language for this process to their "user"-needs.

6 These relationships are discussed in detail in Wurzel (1997), where I show how the concepts of natural language change (cf. Wurzel 1994), the 'invisible' hand (cf. Keller 1990), and language economy (cf. Werner 1989) interact with each other in the explanation of language change. This paper would have been useful for Martin Haspelmath's work, since it (as his article) presents a model of language change.

Theoretically and pratically relevant, yet in large part unanswered, is the question Haspelmath poses regarding the role "social selection" plays in language change. Of course, frequently the different ranking of constraints (or in general of evaluation principles) cannot be grammatically (Haspelmath would say: functionally) explained in the individual languages. Yet here also, one should follow the old principle of the grammarians and first look for a grammatical solution before claiming that social factors are responsible for grammatical relationships. It becomes evident in many cases that the different ranking of segmental constraints in phonology is determined by different suprasegmental structures (that is, a different ranking of suprasegmental constraints). So for example in stress-timed languages like German there is a strong tendency towards reduction in unstressed syllables (cf. Wurzel 1994: 53ff.), but not, on the other hand, in syllable-timed languages like Italian; this fact is not mere coincidence (Hurch 1998: 125). Another example of possible grammatical explanation is given (unknowingly) by Haspelmath himself (§ 5). If speakers, to use Haspelmath's English example *cats*, want to avoid the difficult consonant cluster [kætz], then the choice between [kæts] and [kædz] is in no way based on "social selection" (i.e., grammatically arbitrary), but rather determined via the constraint "No Voice coda" Haspelmath exhaustively discusses. There is no competing constraint imaginable that would favor the form [kædz] over [kæts].

3. Dysfunctionality and Contradiction

Starting from the observation that there are clearly dysfunctional aspects of language structures, Haspelmath disputes the claim that based on the language facts, one also has to accept dysfunctional OT constraints (Lightfoot 1999 and Haider 1998). He points out that the presumed dysfunctionality of constraints disappears when one makes a distinction between the "functional effect" and "side effect" of constraints (20ff.). As a defense of this claim, he deploys two philosophers and one not very convincing biological argument, yet does not even mention the fact that these problems, in a general sense known as "contradictions between evaluation principles" or "conflicts of naturalness", have been a central point of discussion in linguistics for decades.

Haspelmath claims in another part of the article that constraints that are "the direct opposite" of each other, cannot be reduced to user constraints and are therefore to be excluded (§3). Although correct, this is only half true. In fact, constraints stand, as with evaluation principles in other theories, in many cases in contradiction to each other and necessarily so. This was systematically developed earlier by Stampe in the framework of his theory of NPh. Stampe (1969: 443ff.) discussed the problem (among others) on the basis of the contradiction between the context-free phonological process of voicelessness of

obstruents (in general voiceless obstruents are preferable over voiced obstruents) and the context-sensitive process of voicing of obstruents in voiced contexts (in voiced contexts, voiced obstruents are preferable over voiceless obstruents). This results in a unavoidable conflict for obstruents in a voiced context, since a segment can be either only voiced or voiceless. If all obstruents are to become voiceless according to the context-free process, this would contradict the context-sensitive process; if according to the context-sensitive process all obstruents in a voiced context are to become voiced, this would contradict the context-free process. A truly optimal resolution, that is, one without negative "side effects", is simply not possible here. Examples like this are by no means exotic exceptions; they in fact reflect an innate property of natural languages: languages as complex systems cannot be optimal in all respects. From this it follows that "... any meliorative move may have bad consequences. Improvement in such a system can only be improvement on a given parameter of evaluation" (Vennemann 1989: 14).[7] These insights are for OT clearly relevant too, for the individual constraints always fix the optimality in reference to a given parameter; as such, it is not surprising that this can entail dysfunctional consequences. It would have especially suited an article about "Optimality and diachronic adaptation" to take this into account.

4. Synchrony, Diachrony, and the Biological Paradigm

Haspelmath emphasizes repeatedly in the article – and here I agree with him completely – the value of diachrony, in other words the historical dimension, for linguistic theory. In doing so he refers (again and again) to the relationships in biology; compare, for example, "... 'nothing in biology makes sense except in the light of evolution'. If biologists restricted their attention to purely synchronic phenomena ..., they would understand very little of what they observe. I will now argue that historical (or, as linguists say, diachronic) processes are of equally central importance for linguistic theory" (§4). Again based on biological justification, he expresses his final conclusion: "In much the same way, I argue, a linguist who asks 'Why?' must be a historian" (§8), however only based on biological justification. This all (to put it carefully) could at the least be read as if new insights are presented here that are only possible on the foundation of a biological paradigm. We know that this is not the case.

7 The analysis and the theoretical classification of conflicts of naturalness is wideranging in the literature, not only in the work of Stampe and Vennemann. This applies to conflicts between morphological and phonological principles, discussed as early as Paul (1908: 189ff.) using traditional terminology, as well as conflicts between various morphological principles; cf. in this regard Mayerthaler (1981: 43ff.), Wurzel (1984: 29ff., 110ff.; 1994: 76ff.) and Dressler (1985: 260ff.).

Here a famous quotation from Paul (naturally!) comes to mind, in which he ascertained many decades ago, without any reference to biology, that "Es ist eingewendet, dass es noch eine andere wissenschaftliche Betrachtung der Sprache gäbe als die geschichtliche. Ich muss das in Abrede stellen ... So bald man über das blosse Konstatieren von Einzelheiten hinausgeht, sobald man versucht den Zusammenhang zu erfassen, die Erscheinungen zu begreifen, so betritt man den geschichtlichen Boden, wenn auch vielleicht ohne sich darüber klar zu sein" (1909: 20). Of course, today not every linguist would agree with this, but there is in fact a long tradition in modern grammatical theory that starts with Kiparsky (1968: 174): "What we really need is a window on the form of linguistic competence that is not obscured by factors like performance, about which next to nothing is known. In linguistic change we have precisely such a window". The insight that one can only pursue an explanatory theory of grammar when taking into account diachrony, has been since thirty years part and parcel of the theory of naturalness and preference and and again has been again explicitly discussed in many books and papers (especially in the work of Stampe, Vennemann and Wurzel; see the References). We can therefore welcome Martin Haspelmath on board as a new fellow combatant; a pioneer he certainly is – Q.E.D – not. And as a last note: the notion of a biological paradigm is (for this point and indeed for the entire article) in the best case in my opinion nothing more than decorative padding.

Some issues concerning optimality and diachronic adaptation

Martin Haspelmath

Max-Planck-Institut für evolutionäre Anthropologie, Inselstraße 22, 04103 Leipzig
haspelmath@eva.mpg.de

All the eleven commentators raise relevant and interesting issues that will eventually have to be dealt with by a research program in which synchronic patterns are explained with reference to something akin to diachronic adaptation.[1] I have to restrict myself to a few comments on some of the key issues.

1. Are grammatical patterns functionally motivated?

The majority of commentators agree that the answer to the question in the heading is basically yes, but **Haider, Müller, Newmeyer**, and **Dresher & Idsardi** do not. This question needs to be addressed first, because the whole issue of adaptation arises only if one recognizes the functional motivation behind much of grammatical patterning. I think that it is undeniable that Optimality Theory has greatly reduced the prominence of non-functional patterns even within generative grammar: Most constraints belong to the markedness and faithfulness families, and these simply make sense functionally. Highly unnatural rules such as "k → s" (*electricity*) seem to have disappeared from the agenda since the advent of OT. And some prominent OT practitioners are interested in further explanation of the constraints.[2] This makes it harder to argue against functional motivation, but some commentators still do just that.

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1 **Dresher & Idsardi** seem to misunderstand the main point of the target article, because they criticize me for falling short of "demonstrating that adaptation is the major force behind language change". My point was the opposite, in a sense: that language change is the major force behind adaptation. Still, most of what they say is relevant in one way or another to what I said.

2 For example, Kager (1999: 11) requires a functional justification of newly posited constraints: "Phonological markedness constraints should be phonetically grounded in some property of articulation or perception."

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The least interesting rhetorical point that both **Haider** and **Müller** make is that functional explanations appeal to common sense. Yes, sometimes they are compatible with it, but it so happens that common sense sometimes coincides with the truth. More to the point is **Haider's** negative argument that "there is no independent theory of cognitive processing which would tell us whether a particular expression E is cognitively more easy or more difficult to process", but this is too easy a dismissal of decades of sophisticated phonetic and psycholinguistic research. **Müller's** strategy is far more effective: He actually cites relevant psycholinguistic research, and if this research were representative, then I might indeed be persuaded to change my mind. What linguistics needs is precisely this: A confrontation of posited grammatical patterns with all kinds of system-external evidence, rather than the simple assumption that the most elegant formal principles are innate.

Haider also criticizes the notion of adaptation as "serving the user's needs", but I know nobody who has proposed such a definition of adaptation. Rather, a pattern is adaptive if it shows clear evidence of good design. A stone can serve the need of a user who wants to drive a nail into a wall, but only a hammer is adapted to this need and shows good design. Languages resemble hammers, not stones. **Müller** rightly notes that functional explanations run the risk of being post hoc and arbitrary, but I cannot see why this should be a greater problem for functionalist analyses than for, say, generative analyses.

Newmeyer is worried that the functionalist position is undermined if only 30 % of all language changes are functionally motivated. He cites little more than impressionistic observations in support of such a possibility, but **Croft's** suggestion that functionally adaptive changes are characteristic only of large, high-contact, loose-knit societies is extremely interesting and highly relevant. If this were true, then the role of functional adaptation in explaining grammar would indeed be smaller than I envisaged them. At present, this seems unlikely to me, but of course this is an empirical issue that functionalist research needs to address urgently.

2. Assumptions of Optimality Theory

Several authors criticize some of the basic tenets of OT, and I should stress here that I am in no way advocating this particular framework. I merely note that OT has made a rapprochement between generative and functional linguistics easier by directing attention to the functionally motivated aspects of grammatical structure (this is also stressed by **Minkova**). I fully agree with Haider's criticism that OT has no constraints on constraints (although I think that such constraints are at least implicitly observed by good OT analysts). I agree with **Dahl** that the identification of the input for OT evaluations is a huge problem,[3] and that it is

questionable whether all synchronic patterns can be described insightfully by constraint ranking. **Newmeyer** highlights the fact that we sometimes seem to get different construction-specific rankings of constraints within a single language.[4] Traugott reminds us that many patterns look like "junk" from a synchronic perspective and seem to be due to some kind of inertia, i.e. speakers' preference for conservatism. It is hard to see how they could be captured by universal constraints. Other well-known problems of OT are that it has no straightforward way of dealing with optionality and absolute ungrammaticality.

So I basically agree with **Newmeyer** that we still need the parochial rules of earlier generative grammar, or the analogous constructions and schemas of usage-based models of grammar. In my view, the task of description should not be confused with that of explanation: Our descriptions should incorporate generalizations only where we have positive evidence that speakers actually make these generalizations. That grammatical structures as described in this way are not arbitrary is then captured by system-external functional explanations. What OT is trying to do (in line with a long generative tradition) is to build these functional explanations directly into the grammatical description. While this strategy often yields more elegant descriptions, I have serious doubts that it can be a general solution. The fact that OT has lost the ability to do some things that were easily handled in earlier frameworks (optionality, absolute ungrammaticality) should be a warning. Merely redesigning the descriptive machinery often entails as many losses as gains. True understanding requires reduction ("good reduction", as **Minkova** points out) to some independent domain, for instance language use.

3. Variation and selection

If grammatical patterns indeed show a non-accidental resemblance to the patterns of language use, then it is difficult to see how this could be explained without invoking some kind of evolutionary or invisible-hand process. It is true,

3 The theory of diachronic adaptation and OT would be the most easily comparable if the input in constraint tableaux always represented a real earlier stage of the language. The input for *cats* [kæts] in Tableaux 1 would then be [kætəz] or [kætəs]. I could then have avoided the somewhat unnatural example of a change from [kætz] to [kæts]. (However, I would still say that [kætz] is not impossible, contra **Minkova** and **Dresher & Idsardi**, merely very much dispreferred.)

4 Similarly, Raffelsiefen (1996) assumes different constraint rankings for different English affixes. Actually, if one grants affixes independent existence in the lexicon, it is not too problematic to assume that they should be associated with their own constraint rankings. Similarly, in a construction-based model where constructions are listed as separate entities, each of them could be associated with different constraint rankings. I doubt, however, that currently anyone would be prepared to defend this position.

however, that it is not obvious to what extent this process is analogous to biological evolution with its interplay of random genetic variation and environmental selection. So I agree with those commentators who point to open questions in this area. As **Traugott** notes, we have no conclusive answers to some basic questions regarding language change, such as what motivates innovation, and who originates change and who passes it on. Many linguists still think that most innovation originates in language acquisition (as **Dresher & Idsardi** seem to do), a view that I do not share. Needless to say, I do not have new answers to all these big questions, but I feel that work on diachronic change would profit if it addressed more directly the question of whether "language change is language improvement" (Vennemann 1993). **Dresher & Idsardi's** remark concerning final devoicing illustrates the pitfalls of ignoring functional explanations: These authors seem to say that devoicing and revoicing in coda position are mere random oscillations without any adaptive significance. But the phonetic preference for final devoicing is well-understood, and as expected, there are no phonological changes involving voicing in coda position. The loss of final devoicing in Yiddish is clearly morphologically adaptive, because it occurred only when it led to greater morpheme uniformity ("paradigm leveling").[5]

Dahl remarks that the explanatory role of variation is undermined if the source of variation is non-random. **Croft** goes much further by claiming that all functionally adaptive patterns are due to functionally motivated innovations, not to selection. These are important questions to which I have no complete answer. For many changes, it is probably true that the adaptive nature derives entirely from the innovations (e.g. final devoicing), so that the additional mechanism of functional selection could be dispensed with. But I am not sure that this works for all cases. Consider the case of article-possessor complementarity (*the book, my book, *my the book*), which I have argued is explained by economic motivation: Since possessed NPs are very likely to be definite, the definite article is redundant, and this redundancy is exploited by some languages (cf. Haspelmath 1999). In older Germanic, the grammaticalization of the definite article was only in its beginning stages, and the occurrence of the article was highly variable: *book, the book, my book*, and *the my book* were all possible. As the pattern became more fixed, some of the variation was eliminated by some kind of selection process, so that now only *the book* and *my book* are possible.

5 **Dresher & Idsardi** seem to be unaware of the classic functionalist way of dealing with conflicts between phonological and morphological preferences, otherwise they would not mention "increasing opacity" as a problematic case for the functional approach. Increasing opacity indeed creates difficulties and violates morphological preferences, but it is usually well-motivated phonologically. So opacity is good for pronunciation but bad for morphology, while analogical leveling ("restructuring") is good for morphology and sometimes bad for pronunciation. The old generative idea of rule addition to the end of the grammar, as still invoked by **Dresher & Idsardi**, is far less principled. (Why not rule addition to the beginning of the grammar? Why not rule subtraction?)

What kind of functionally-driven innovation might be held responsible for the elimination of the less economical variant *the my book*? It seems to me that in such cases where the source of variation (grammaticalization in its early stages) is not identical to the functional motivation (utterance economy), the concept "functional selection" still has an important role to play.

4. What's in performance, what's in grammar, what's innate?

It is very important to separate at least conceptually the three domains of performance (or language use, or E-language), grammar (or conventional patterns, or competence, or I-language), and innate structures (or "Universal Grammar")[6]. Functionalists sometimes rhetorically reject the performance/grammar distinction, but keeping them separate really seems to be a conceptual necessity. So the question is usually not whether a pattern exists as a conventional grammatical entity, but merely whether it is functionally motivated. When **Dresher & Idsardi** argue that a certain phonological principle "does not emerge from phonetic factors such as articulatory effort", they attack a strawman: Nobody really denies that competence patterns exist, but functionalists claim that linguistic analysis does not stop there. Grammatical patterns do not "emerge directly", but arise in some non-trivial fashion that should be the object of serious linguistic research (and in my view, diachronic adaptation is the key concept here).

However, I was somewhat surprised to see **Wurzel** assert "that 'grammatical' optimality is identical to 'user' optimality." This would seem to indicate that Wurzel does not distinguish between grammatical patterns and language use, but I am sure that there is a misunderstanding here. Similarly confusing is Wurzel's quotation from David Stampe, who describes a phonological process as "a mental operation that applies in speech ...". This seems implausible initially if taken literally, but eventually I am confident that Stampe, too, must have an equivalent of the competence/performance distinction.

More difficult is the question of which grammatical structures are innate. Universality is a necessary prerequisite for innateness, but not a sufficient condition, because most of the relevant factors affecting language use are universal as well. Generative grammarians have had a tendency to attribute as much as possible to an innate UG; an instance of this general tendency is **Dresher**

6 By "innate structures" I mean here properties of the mental "hardware" that directly constrain the possible form of grammars. **Wurzel** notes that functional motivations such as articulatory and processing preferences ultimately also have an innate basis, but this "indirect innateness" must of course be separated strictly from the "direct innateness" of "Universal Grammar".

& **Idsardi's** suggestion that what prevents codas from having more than one phonologically distinctive voicing gesture (thus ruling out *[kætz]) is a principle of UG. Since this constraint is eminently reasonable from a usage-based point of view, I find the assumption that UG is not involved much more plausible.

Functionalists, by contrast, have tended to minimize the contribution of innate patterns. **Kirby** focuses on this opposite bias and asks: "Why should be assume that the domain of I-language is a blank slate onto which any useful constraints can be written?" Linguists who are not impressed by the argument from the poverty of the stimulus would probably answer: Because this is the null hypothesis. But Kirby's example from parsing difficulty and word order in ditransitive clauses shows nicely that not all useful performance constraints seem to be possible as grammatical constraints. An even more impressive demonstration that performance factors cannot explain everything can be found in Hayes (1999: 248–53), where it is shown that phonological systems exhibit a tendency toward symmetry that cannot be attributed to functional motivations. But far too little attention has been devoted to these issues, because generative linguists have been much too quick to derive observed universal patterns from the architecture of their grammatical models (thereby attributing them to UG), without even asking whether the patterns might instead be explainable in functional terms.

5. Adaptive and counter-adaptive changes

Traugott and **Dahl** raise the question of those changes that are not adaptive, in particular grammaticalization, which I suggested (in note 10) is "a kind of inflationary counter-adaptive process". Since grammaticalization accounts for so much of diachronic change, Traugott is worried that this undermines my whole enterprise. She then asks: "Under what understanding of adaptation is it reasonable to exclude [grammaticalization] from the kinds of phenomena that should be accounted for?"

In the article I did not express a strong opinion on whether grammaticalization changes are adaptive or not. My reason for describing grammaticalizaation as counter-adaptive is that very often a new grammatical category simply seems to supplant an earlier one, without any change of the design quality. For instance, the French compound past (*j' ai acheté* 'I bought') superseded the older simple past (*j' achet-ai*), the periphrastic comparative forms (*plus fort* 'stronger') replaced the Latin affixal comparatives (*fort-ior*), the *-ment*-adverbs (*forte-ment* 'strongly') replaced the Latin *-iter*-adverbs (*fort-iter*), and so on. Bybee et al. (1994: 298) also prefer a "mechanistic" view of grammaticalization to an adaptive one:

> "Further evidence against the functionalist teleology is the ironical fact that efforts to be more concrete and specific lead to the loss of specific concrete components of meaning in grammaticization ... For instance, if a verb formerly meaning 'want' is now used for intention or prediction, then speakers must choose another verb with a more specific meaning of 'want' if that is what they mean to express."

However, while grammaticalization does not seem to be adaptively motivated, it may well have adaptive consequences, and the way it proceeds may be constrained functionally. Take the example of the English comparative: Here, too, the recently grammaticalized periphrastic comparative is widespread (e.g. *more beautiful*), but many adjectives only allow the affixed comparative (e.g. *warm-er*). The distribution of the periphrastic and the affixed comparative is clearly functionally motivated: Affixal comparatives are allowed only when a prosodically well-designed word results. So the grammaticalization has led to an adaptive pattern, although initially the process was not motivated by adaptation.

Traugott further observes that grammaticalization often introduces "important functional categories" into a language. But how important these functional categories are may be questioned, because for every functional category there are many languages that get by without it. So **Dahl** has a point when he critically notes that one of my adaptive accounts (the obligatoriness of subject pronouns in languages with rich agreement) seems to assume an adaptive motivation of grammaticalization. But on the other hand, there are no languages that lack functional categories entirely, so it would be surprising if functional categories turned out to be non-adaptive, purely mechanical structures. I acknowledge that this is a difficult problem area that needs much further scrutiny.

6. The biological analogy

Itkonen criticizes me for basing my claims about diachronic adaptation on the analogy with change in biology, and **Wurzel** similarly dismisses the „biological paradigm" as "nothing more than decorative padding". Of course, the comparison with biological adaptation is not crucial to my proposal, but I do think that it helps me make my central points. I fully agree with Itkonen that there are important disanalogies between biological adaptation and linguistic diachronic adaptation. Yes, there is "an application of intelligence in linguistic change which is absent in biological evolution", but I do not agree that "this suffices to make the two domains totally disanalogous". There are still important analogies left, and the real question is whether these analogies are fruitful enough to be worth exploiting. **Croft** goes so far as to advocate a "generalized theory of selection" of which biological and linguistic selection are just special cases, and I think not even Itkonen would disagree that such a generalized theory is at least possible.

For me, the main attraction of biological adaptation as an analogy to linguistics is that I see strong evidence for good design both in biological organisms and in language structure. Biologists have been blessed with a powerful theory for explaining such good design, and it seems to me that linguists can still learn from the biologists' success. Many linguists still find good design in language structure so mysterious that they continue to ignore the accumulating evidence for it,[7] or continue to attribute adapted grammatical patterns directly to speakers' intentions. Biological evolution shows that good design without an intentional designer need not be mysterious.

Moreover, the cross-fertilization between linguistics and biology is by no means a new fashion. Hermann Paul (cited by **Wurzel**) and his neogrammarian contemporaries were profoundly influenced by a zeitgeist that rejected Hegelian idealism and romantic collectivism in favor of an individualism, materialism and uniformitarianism that shared much with (and probably owed much to) Darwin's reconceptualization of biological change. And conversely, Darwin and the early Darwinians often appealed to the analogy with linguistic evolution (cf. Alter 1999 for a recent account).

Itkonen suggests that the biological analogy may also be motivated by the higher prestige of biology. This suspicion is difficult to counter, especially since such factors also work unconsciously. But I would say that biology has simply been more successful than linguistics, so it is natural that linguists should look to biology for enlightenment.[8] Moreover, if prestige were one's main driving force, one should try to be inspired by genetics rather than evolutionary biology, or for that matter, by physics or chaos theory (cf. Lightfoot 1999).

7. On the novelty of my proposals

Some commentators question whether what I have to say is really new. In particular, both **Minkova** and **Wurzel** mention Hayes (1999), an important paper on "phonetically-driven phonology" that gives the OT constraints a basically functionalist interpretation. Hayes explicitly says that he is a functionalist in

7 As a striking example, consider the fact that Hawkins's (1994) sophisticated and detailed work on the performance factors behind word order patterns has been completely ignored by generative syntacticians.

8 Before Darwin developed his theory of descent with modification, linguistics was more successful than biology in that it established the fact of evolution and the details of family trees earlier than biology, so that it was natural that biologists paid more attention to linguistics. (Or was the real reason that linguistics was more prestigious in the 19th century?)

that he believes that "the formal system of grammar characteristically reflects principles of good design" (p. 276). This is an unusual statement for a generativist, but it still seems to me that the more mainstream OT view is the anti-functional stance articulated by **Müller** (although phonologists and syntacticians probably differ significantly in their views). In any event, Minkova's and Wurzel's point is well-taken, and if I had known Hayes's brilliant paper when I wrote the target article, I would have written it differently.

Wurzel stresses that much of what I say is already found in Natural Morphology and Natural Phonology, and indeed in Hermann Paul's writings. I cannot deny this, and it is no secret that I have been profoundly influenced by these approaches. I agree with Wurzel that Paul's insistence on the diachronic perspective cannot be interpreted as betraying a simple lack of interest in synchronic grammatical structures. Paul knew that the true explanatory factors are in diachrony, but he did not, as far as I know, directly address the question of how functional preferences in speech come to be reflected in conventional patterns.

This question, called "the problem of linkage" by Kirby (1999 : 19), has very rarely been addressed directly by functionalists, including Natural Morphologists. It may be an exaggeration to speak of "the biggest flaw in the functional approach" (Kirby 1999: 19), because it is more a lacuna than an error, but sometimes functionalists' formulations are indeed misleading. For instance, Dressler (1990: 76) writes about his approach (Natural Phonology/Natural Morphology):

> "It is assumed that both linguistic universals and all language systems have the teleology of overcoming substantial difficulties of language performance (including storage/memorization, retrieval, evaluation) for the purpose of the two basic functions of language: the communicative and the cognitive function."

Bybee (1999: 212) concludes from this statement that Dressler attributes goal-oriented behavior to language systems, and even makes the generalization that this teleological approach is characteristic of European functionalism. I am sure that Bybee misinterprets Dressler, but his formulations invite such a misinterpretation. And Dressler is not an isolated case: Functionalists have not focused sufficiently on the "problem of linkage", and they should be careful to avoid teleological ways of speaking until there is a generally recognized solution for this problem.

8. On how questions and why questions

Müller ends his comments with a call for modesty: "Given the lack of decisive evidence, my conclusion is that, for the time being, the (generative) grammarian is well advised to concentrate on how questions, and postpone why questions until our understanding of cognitive issues has deepened, and these questions can be answered (or even asked) properly." I completely agree that modesty is a virtue, and perhaps linguists have a dangerous tendency to be overambitious. This also applies to generativists, who have advanced their approach as a solution to "Plato's Problem", or have speculated on whether the language faculty is "perfect".

But what I want to question here is whether it is a good strategy to defer why questions and concentrate on how questions in isolation. I believe that the answer to how questions often depends on the answer to why questions, i.e. that description is not independent of explanation.

Consider the issue of whether noun phrases contain a determiner position which can be filled only once and therefore accounts for article-possessor complementarity (**the my book*). Henry Sweet modestly noted this cooccurrence restriction in his 1898 grammar of English, and added a brief speculation about an explanation:

> "Nouns qualified by a genitive do not take the articles ..., evidently because the preceding genitive is felt to define them enough by itself" (Sweet 1898: 64)

Throughout the 20th century, structural and generative syntacticians have defended a very different analysis in terms of a single determiner position, which builds the explanation, so to speak, into the description. Now it turns out that as more languages are considered, the determiner-position analysis is not general enough, and only a functional economy-based explanation makes the right predictions about the distribution of articles and possessors (Haspelmath 1999). The determiner category now seems redundant and indeed questionable. The postulation of a structural determiner position may well have been an overambitious step in the wrong direction that would have been avoided if syntacticians had paid enough attention to the why question. This is a small simple example, but it seems to me that it nicely illustrates the dangers of limiting oneself to how questions.

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Aufsätze

Wortbegriff und Nomen-Verb-Verbindungen*

Peter Gallmann

Abstract

In connection with the new spelling conventions in German, the status of noun-verb combinations has repeatedly been discussed, with the emphasis on whether such combinations are to be written in one or two graphic words, e.g. *maßhalten* or *Maß halten* ('to observe moderation'). Thereby it has not sufficiently been taken into account that in grammatical theory the term "word" is not clearly defined unless a theoretical context is given. In section 2, I will attempt to clarify the different concepts of "word" found in the literature when used in syntactic, phonological, graphemic or lexematic-paradigmatic contexts. In section 3, I will compare the terminology thus obtained with a series of concepts that can be found in the literature on the phenomena of noun-verb combinations such as compounding, incorporation, lexicalization, etc. In my last section – as a result of the previous discussion – I will show that the new regulation of the German orthography corroborates the prevailing usage of writing noun-verb combinations as two graphic words.

1. Zu diesem Aufsatz

Im Deutschen gibt es eine Reihe von Verbindungen des Typs X + Verb, bei denen X auf ein nominales Lexem bezogen werden kann. Rein orthographisch kann man zwei prototypische Fallgruppen unterscheiden, vgl. als Beispiele (1a, b) mit den nominalen Lexemen (2a, b):

(1) a. teilnehmen (ich nehme teil, ich habe teilgenommen)
b. Anteil nehmen (ich nehme Anteil, ich habe Anteil genommen)

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