

A few lessons from typology

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Typology has a low profile in much of American linguistics, especially outside of phonology (Nichols 2007, Hyman 2007, Van Valin 2007). Yet, as I will suggest, the study of the results and methods of modern typology has important lessons for us as the field of linguistics undergoes a paradigm shift. Typologists study a wide range of language types, but I will show that even when one does theoretical work on a single, well-studied standardized national language like English, one can (and should) benefit from an awareness of typological findings.

With the explosive growth of language technologies, it is increasingly recognized that the traditional ways of collecting linguistic data are deeply flawed. Although grammaticality judgments are considered an extremely rich source of data, it has long been evident that introspections about decontextualized, constructed examples – especially in syntactic and semantic domains – are unreliable and inconsistent, as pointed out by sociolinguists and dialectologists (Labov 1975, 1996, Cornips & Poletto 2004). Improvements in experimental judgment elicitation techniques have been suggested (Schütze 1996, Cowart 1997, Bard et al. 1996), but the constructed sentences used in many controlled psycholinguistic experiments are themselves highly artificial, lacking discourse cohesion, and subject to assumptions about default referents (Roland & Jurafsky 2002). Moreover, theoretical linguists are usually unaware of the multiple variables that are known to affect linguistic judgments and can hardly control for them (Gries 2005). What is needed are data from language as it is used in ecologically natural settings and better models for understanding it.

Consider as an example the “Affectedness Constraint” on complement preposing with nominalizations (Anderson 1978, Giorgi & Longobardi 1991: 140–145). It states that preposing the complement is grammatical only when the referent of the complement is affected by the denoted event. Thus in the examples in (1), destruction, removal, and defacement affect the possessive arguments, while recollection, perception, and observation do not:

- (1)
- | | | | |
|----|---------------------------------|----|-----------------------------------|
| a. | <i>the city's destruction</i> | d. | <i>*the event's recollection</i> |
| b. | <i>the boy's removal</i> | e. | <i>*the problem's perception</i> |
| c. | <i>the picture's defacement</i> | f. | <i>*the picture's observation</i> |

There is clearly a contrast between these two columns of examples, which experiments, questionnaires, and the like would doubtless confirm.

Nevertheless the Affectedness Constraint is not empirically well founded. Preposed possessor nominals are grammatically possible despite being unaffected, when they are appropriately contextualized to maximize topicality (Taylor 1994, 1996). Examples collected from actual uses on the web illustrate this point:

- (2)
- a. *Certainly, between the presentation of information to the senses and its recollection, various cognitive processes take place.*
 - b. *Lesson 2: Sound Properties and Their Perception.*
 - c. *But the standard idea that an event is inseparable from its observation is just scientific silliness.*

According to Taylor possessor nominals have to be TOPICAL and INFORMATIVE relative to the possessed. In (2) the definite pronouns corefer with the immediately preceding phrases, which are highly topical. In (1) the intuitive contrast between the columns is explained by relative informativeness: entities affected by destruction, removal, and defacement provide more reliable cues to identification of these actions than do objects of recollection, perception, and observation, and according to Taylor this explains why (1d–f) are unused.

Still, can we trust data taken from the web? To what extent does it reflect grammatical (though perhaps infrequent) possibilities rather than sporadic errors? This is one place where awareness of typological patterns can aid the researcher.

Typological studies of nominalization constructions show a pattern with more “highly referential” arguments (pronouns, definites, and/or animates) favoring prenominal genitive positions, and less highly referential arguments (lexical nouns, indefinites, and/or inanimates) generally favoring postnominal positions (Koptjevskaja-Tamm 1993: 73–76, 172–176, 201–203). This type of split occurs more or less variably in Bulgarian, Russian, Czech, French, Samoan, and varieties of modern Greek and Hebrew, among other languages, and the English web data in (1) indicate that it overrides the dispreference for preposed possessors in “unaffected” nominalizations seen in (2). This evidence from usage and typology converges with experimental and corpus evidence concerning English possessives more generally, showing that the topicality of the possessor is a strong determinant of construction choice, stronger in fact than the semantic relation expressed by the possessive construction (Rosenbach 2002, 2003, 2005).

In short, the examples from the web illustrated in (2) are unlikely to be sporadic errors, because they are instances of typologically valid patterns reflecting cognitive principles which are systematically manifested elsewhere in English grammar. They show that the “Affectedness Constraint”, discovered by means of the traditional methods of generative syntax, wrongly locates the boundary of what is grammatical in English.

The Affectedness Constraint is not an isolated case. Consider the English dative alternation. Following the pioneering work of Green (1974) and Oehrle (1976), many linguists have sought to explain the choice of construction in terms of lexical or constructional semantics, the double object construction being associated with broadly “possessive” semantics and the prepositional construction with broadly “allative” semantics. For thirty years this explanation has been supported by the contrasts in (3) and (4) or very similar ones:

- (3) a. *That movie gave me the creeps.*
 b. **That movie gave the creeps to me.*
- (4) a. *The lighting gives me a headache.*
 b. **The lighting gives a headache to me.*

Very roughly, the idea is that in giving the creeps or a headache, there is no movement to a goal, but only the coming into existence of a physical or psychological state of the dative referent, and so the “allative” prepositional phrase is not permitted. This semantic contrast has been the basis for important theories in syntax, semantics, and language learnability (see Bresnan & Nikitina 2003 for further discussion).

But many examples of the kinds claimed to be ungrammatical can be found in current use on the web, including (5) from Bresnan & Nikitina (2003):

- (5) a. *This life-sized prop will give the creeps to just about anyone!*
Guess he wasn't quite dead when we buried him!
 b. *... Stories like these must give the creeps to people whose idea of heaven is a world without religion ...*

Again we must ask whether we can trust these examples from the web. Could they simply be unrepresentative anomalies fished up from the vast depths of the internet?

Notice that the frequently cited ungrammatical examples of *give* idioms in (3) and (4) have PPs containing short, pronominal recipient expressions denoting accessible referents, but the great majority of the examples from actual language use have a nonpronominal recipient PP which is greater in length than the theme NP. In these conditions, the alternative constructions would have been disharmonic according to the principle of end weight (Behaghel 1909/1910):

- (6) *Stories like these must give people whose idea of heaven is a world without religion the creeps.*

We conclude that idioms like *give the creeps/a headache*, have a strong bias toward the double object construction, but the principle of end weight can override it. Indeed, converging evidence shows that the same principle is alive in spoken English across all of the broad semantic classes of dative verbs (Bresnan, Cueni, Nikitina, & Baayen 2007) and may be grounded in general principles of sentence perception or production (cf. Hawkins 1994, Arnold et al. 2000, Wasow 2002).

In short, the traditional methods of generative grammar which discovered the ungrammaticality of prepositional datives with the idioms in (3) and (4) (and repeated it for thirty years as a building block of various syntactic theories of English verb phrase structure), have again wrongly located the boundaries of grammaticality.

Georgia Green (1971) showed early on that some of the generalizations that have attained textbook status for ditransitives – such as the claim that certain verbs obligatorily occur in the double object construction –

- (7) a. *Ted denied Kim the opportunity to march.*
 b. **Ted denied the opportunity to march to Kim.*
- (8) a. *The brass refused Tony the promotion.*
 b. **The brass refused the promotion to Tony.*

– are untrue when one looks systematically at the effects of conflicting constraints that prohibit noun-pronoun sequences of objects:

- (9) a. **Ted gave Joey permission to march, but he denied Kim it.*
 b. *Ted gave Joey permission to march, but he denied it to Kim.*
- (10) a. **The brass gave Martin permission to sit, but they denied Tony it.*
 b. *The brass gave Martin permission to sit, but they denied it to Tony.*

Green's insights came from the systematic investigation of her own intuitions about the grammaticality of English examples. But the overriding constraint that she investigated has broad empirical validity in typological findings. English speakers' avoidance of (9a) and (10a) is both gradient and rooted in well-documented typological patterns found elsewhere in English and other languages (Bresnan & Nikitina 2003, Haspelmath 2004). Compare Haspelmath's typological study of ditransitive "R" and "T" (prototypically Recipient and Theme) expressions in descending order of harmony. In Figure 1 the least harmonic combination of expression types for ditransitives is precisely the one

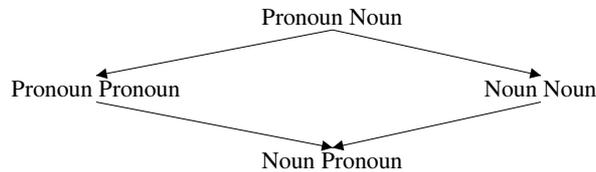


Figure 1. *Relative harmony of ditransitive R-T expressions (represented with R preceding T)*

that is avoided by adopting the otherwise dispreferred construction for verbs like *deny*, *refuse*.

The implicational hierarchy in Figure 1 is supported by evidence from Lummi, Capeverdean Creole, Hausa, Lillooet, English, French, among others. Converging evidence for this and related typological principles that characterize a broad range of English ditransitives is provided by corpus studies (Collins 1995, Bresnan & Nikitina 2003, Bresnan et al. 2007) and by psycholinguistic studies of language production (see Ferreira 1996 and references).

The few cases discussed here could be multiplied. Erroneous generalizations based on linguistic intuitions about isolated, constructed examples occur throughout all parts of the grammar. They often seriously underestimate the space of grammatical possibility (Taylor 1994, 1996, Bresnan & Nikitina 2003, Fellbaum 2005, Lødrup 2006, among others), reflect relative frequency instead of categorical grammaticality (Labov 1996, Lapata 1999, Manning 2003), overlook complex constraint interactions (Green 1971, Gries 2003) and processing effects (Arnon et al. 2005a, b), and fail to address the problems of investigator bias (Labov 1975, Naro 1980, Chambers 2003: 34) and social intervention (Labov 1996, Milroy 2001, Cornips & Poletto 2005).

The lessons I would draw are these:

- (i) Linguistic intuitions of ungrammaticality are a poor guide to the space of grammatical possibility.
- (ii) Usage data reveals generalizations which we are sometimes blind to.
- (iii) Typology helps us to discover and evaluate patterns in the data.

One might object that typology is subject to the same limitations of over-reliance on linguistic intuitions as other parts of our field, perhaps even more so. Don't the authors of the grammars which are consulted by typologists have to rely on the linguistic intuitions of their informants? In fact Koptjevskaja-Tamm's typological study of nominalizations cites the sources of the contrasting examples given in (1) as evidence for her typological classification of English! So if (1) is empirically ill-founded, then isn't any classification based on it also suspect? Likewise, corpus studies rely on subjective judgments in collecting, categorizing, annotating, and counting linguistic observations. Do

these other disciplines then really have anything to teach theoretical linguists about data? Aren't they subject to the same problems as the traditional methods of generative linguistics, only multiplied across many languages which are shallowly accessed through the necessary use of many secondary sources?

These objections are misplaced. All linguistic data depend on classifications of meaning and form, which are, at bottom, subjective. In that sense, it is wrong to imagine that we somehow escape from subjectivity when we count and statistically summarize data. The point is not that we can or should escape subjectivity in this sense. Rather, we must avoid using only intuitions for data, in what has been the traditional methodology of theoretical linguistics. Recall that the intuitive contrast in (1) is very real; what is invalid is the generalization that only affected complements can be preposed. That generalization ("The Affectedness Constraint") was based solely on the introspection of constructed examples like those given in (1), and that generalization is false, as shown by the way nominalizations are actually used (2) and the related evidence given above. The intended lesson is not, then, about the subjectivity of linguistic judgments, which is unavoidable, but about the need to support claimed generalizations with multiple empirical sources of converging evidence, including observations of ecologically natural language use.

Typological discoveries are often supported by convergent research from field work and psycholinguistic experimentation on ecologically natural uses of language. Notable examples include referential density (Bickel 2003) and the framing of motion events (Talmy 1985, Slobin 2004), to add just two to those mentioned above. Typologists also study the spatial and temporal distribution of languages (Dryer 1989, Nichols 1992, Nichols & Peterson 1996) and produce large databases of such information. Far too many theoretical linguists have little exposure to this kind of research, less appreciation of its implications for their own work, and no understanding of the increasingly sophisticated statistical models and methods being used in typology (Bickel 2007, Nichols 2007).

There is not space here to discuss the kinds of new theoretical models that can help us to understand usage data and avoid the oversimplifications working with intuitive data that has been idealized and decontextualized. Bresnan et al. (2007) discuss the role of modern statistical modeling in providing some empirical solutions to problems and controversies of usage data, and Baayen (2004) discusses its role in experimental designs that can incorporate authentic linguistic materials such as sampled rather than constructed data.

One of the empirical results of the Bresnan et al. paper is the demonstration of a quantitative "harmonic alignment" effect on the English dative construction. For example, after adjusting for the effects of relative syntactic complexity, givenness, definiteness, semantic class of the verb, and other properties that are partially correlated with animacy, inanimate recipients are still over

five times as likely to occur in the prepositional phrase position of the *to*-dative as animates. In other languages the harmonic alignment of animates/inanimates with core/non-core syntactic positions is obligatory in the grammar of dative/applicative syntax: examples include Shona and Sesotho (Hawkinson & Hyman 1974, Morolong & Hyman 1977), Spoken Eastern Armenian (Polinsky 1996), and Mayali (Gunwinjguan; Evans 1997). Occurrences of obligatory animacy alignment in the syntax of ditransitives makes a case for the integration of the quantitative data from English into linguistic theory. More generally, the fact that “soft constraints mirror hard constraints” (Bresnan, Dingare, & Manning 2001) is one of the fascinating indications that typology may be as fundamental to theoretical syntax as it is to phonology. A theoretical model for incorporating these kinds of data into linguistic theory has been significantly advanced by a typologist (Maslova in press).

As I hope to have shown, it will no longer do to take the complacent view that each discipline has goals and methods complementary to the other – theorists concerned with possible languages and typologists with probable languages (cf. Newmeyer 2005). It is abundantly clear that the traditional methods of linguistics provide no privileged access to what are possible human languages, and they never did.

What can be done to improve the situation? Encouraging hands-on exposure to typologically diverse languages is of course important (the “middle way” of Baker & McCloskey 2007). My own recommendations are to improve linguistics education by

- (i) training in multiple methods of handling data, including field work, corpus work, experimental paradigms, and typological research using ecologically natural data;
- (ii) teaching a wider range of theoretical models, including probabilistic grammars, exemplar-based models, and evolutionary models;
- (iii) requiring probability and statistics, which are the foundations of the linguistics of the future and the lingua franca that will allow it to join the mainstream of cognitive sciences (Polinsky & Kluender 2007); and
- (iv) hiring a “new typologist” (see Bickel 2007), who does both field work and statistical modelling, in every department.

Of course we wouldn’t expect all students of linguistics to do all of this convergent research by themselves, but they need to know how to find it, evaluate it, and use it in their work.

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