

On the Relationship of Typology to Theoretical Syntax

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In these remarks on the relationship between typology and theoretical (morpho)syntax, we would like to briefly touch on three issues: what is their relationship in practice now, what relationship should one in principle expect given the founding goals of each enterprise, and what kind of research could help connect the two fields in a more productive way in the future.

In current practice, we see signs that work in theoretical syntax is being influenced more and more by the methodology and results of typological work. The Greenberg Universals of word order (Greenberg, 1963) have been incorporated into the generative theory of phrase structure for a long time, at least since Stowell's (1981) reduction of explicit phrase structure rules to more general principles. Perhaps the most discussed formal research of the late 1990's was Guglielmo Cinque's (1999) study of clause structure, which made use of the typological results of Bybee (1985) and related work; he also performed his own survey of more than 500 languages. Julien 2002 is another important example of this emerging genre. In a similar spirit, Baker (1996) attempted to incorporate and explain some of Johanna Nichols' (1986, 1992) typological observations about head marking languages into his theory of polysynthesis, and Baker's (2003) theory of lexical categories is deeply informed by typological studies like Stassen (1997) and Wetzer (1996). More generally, we sampled four recent issues of *Linguistic Inquiry* and four of *Natural Language and Linguistic Theory* (two leading venues for formal syntactic work). Of the 27 articles on syntax in these issues, the mean number of

languages discussed per article was 3.37 (ranging from 1 to 8 languages). We suspect that this is much higher than it would have been 20 years ago.

But there are also signs that the integration of typology and formal theory is progressing more slowly and more sporadically than one might hope. Cinque's research has been widely admired, and widely cited, but not widely imitated. Moreover, one gets a different impression of the 27 recent research articles if one counts how many language *families* are discussed in each article. Sixteen of the 27 focus exclusively on languages from a single family (in every case but one, Indo-European). Another 9 discuss languages from two families—comparing a single East Asian language (Chinese, Japanese, or Korean) with English, for instance, or comparing a Semitic language with various branches of Indo-European. One paper considered languages from three families, and one single paper considered languages from five families. A journal article with strict page limits is not of course the ideal genre for presenting a large scale typology. Still, we get the strong impression that, while crosslinguistic comparison is clearly on the rise in formal work, true typology is not. Conspicuously absent in most of this work is the typologist's vision of controlling for genetic and areal factors by sampling unrelated languages and language families. Rather, the methodology of comparing closely related languages, pioneered by Richard Kayne, has been much more influential in formal generative circles—partly because of its intellectual merits, but also partly (we suspect) because it is easier to do.

This leads us to consider the logical relationship between the goals of formal linguistic theory and those of typology, to evaluate whether this situation is desirable or not. A standard goal of typological research since Greenberg has been to discover (and

explain) “universals” of human language by sampling widely among all the attested natural languages. The universals that have been proposed are of three main types:

- Truly universal claims, of the form “no language/every language has feature X.”
- Statistical regularities, of the form “Languages with feature X are very (un)common.”
- Implicational universals, of the form “If a language has X then it will also have Y.”

Two classes of implicational universal can also be distinguished---absolute implicational universals (“If X, then always Y”) and statistical implicational universals (“If X, then usually Y”).

These different kinds of empirical discovery differ in their implications for the program of formal theoretical syntax. This program typically aims to provide an understanding of what it is to be a “possible human language”, or “attainable state of the language faculty”. For this project, universals of the first type (to the extent that they are not attributable to external contingencies) are extremely relevant. They must either be built into the design of the theory, or the theory must be developed in such a way that it guarantees their truth. In contrast, the relevance of universals of the second type is much less clear. Obviously languages with even the rarest of rare features—say Hixkaryana, with its OVS word order (Derbyshire, 1985)—are *possible* human languages (represent attainable states of the language faculty); otherwise they could not be *actual* human languages. Nor is there is any evidence that we are aware of that such languages are more difficult to acquire or to use than languages that have the more common features. It follows then that nothing in the theory of what counts as an attainable human language

should rule out the existence of these rare languages, so the relevance of statistical universals to the theoretical tasks is obscure at best.

This conceptual assessment interacts in turn with an empirical result which seems to emerge clearly from typological research: universals of the first kind are quite rare, and are usually not terribly interesting (e.g. all languages have vowels; all languages have verbs). Most of the striking results in typological research are statistical. There are many of these, and they concern interesting contingent properties of language, but, as a matter of logic, their relevance to a theoretician whose goals are those of generative grammar is not obvious. This then can partially explain why typological concerns and results have had less influence on the practice of generative grammar than one might have hoped. It is easy to get the impression either that there are no truths of the kind the formalist seeks, or that for some reason standard typological methodologies are unable to uncover those truths. In the meantime, those committed to typological methods understandably get very interested in the statistical regularities that they uncover (regardless of what they might reveal about what is a possible human language), and naturally seek to explain them in terms of forces and principles that are completely external to the theory of what it is to be a possible human language. It is Newmeyer's (forthcoming) position that this kind of “externalization” strategy (according to which the task of explaining statistical generalizations is external to the business of constructing a theory of possible grammars) is both logically and empirically justified. On the typological side, this view is also in harmony with the “new typology” reported by Balthazar Bickel, which focuses on areal, historical, and anthropological factors in creating statistical patterns. In this conception, formal linguists are not held responsible for accounting for the statistical distribution of language types and features. Grammatical theory and typology then tend to evolve into

two separate fields, with distinct goals and objects of inquiry, which rightly have little to do with each other.

There are, of course, alternatives to this strict “externalist” mode of explanation. Many strands of current work converge on the view that distributional regularities often reflect the action of functional pressures of one kind or another. One response to this observation is to seek to build those functional pressures directly into individual grammars, as has been done in the functionalist tradition, and more recently in functional OT. Frequent or infrequent grammar-types then reflect the probabilities of different “solutions” to problems of optimization clumping in one or another region of the possibility-space.

A different choice would be to resist building functional pressures directly into grammatical theories, holding instead that functional pressures operate over time to shape the statistical distribution of possible features and systems. The time-span over which such pressures operate could either be historical (shaping the evolution of particular languages) or evolutionary (shaping the evolution of the language faculty). The former would provide an understanding of which systems are (im)probable; the latter would provide an understanding of which systems are (im)possible. One promising path in this direction would be a marriage between John Hawkins' research program and Principles and Parameters/the Minimalist Program—a marriage which seems desirable on other grounds as well (see again Newmeyer forthcoming).

Consider now the third class of universals, those that have the form of implicational statements. To the extent that these are *absolute* implicational universals, they have the same importance for the grammatical theorist that the absolute universals have: they too should be built into the theory or emerge from it as some kind of logical

consequence. Moreover, typological research gives us hope that this kind of universal might be relatively common and involve interesting and substantive linguistic properties. So if there is to be a true partnership between grammatical theory and typology, this is where we might expect it to be centered. Moreover, it might be that some of the known statistical universals will emerge as absolute implicational universals when we uncover additional terms in the if-clause of the conditional. For all we know, statements of the form “If X and Y, then usually W” can be turned into statements of the form “If X and Y and Z, then always W” (where Z itself a relatively common feature of language, for whatever reason).

There are some relatively clear cases of this being so. Consider the Greenbergian discovery that SOV languages are only slightly more common than SVO languages, but that both are much more common than VSO languages. Current formal treatments suggest that VSO word order can arise when a number of factors happen to come together. Heads must come before their complements (as in SVO languages), subjects must remain relatively low (perhaps inside VP or at any rate not moving to the specifier position of InflP), and the verb must move into the Infl node. Suppose that each of these grammatical features has a 50% chance of being set in a particular way (known to be roughly true for the head parameter). Then only about 1 in 8 languages will be VSO languages, because all three factors have to be set in one particular way for a VSO language to emerge. Any other combination of parameter settings will give either an SOV language or a SVO language (see Baker 2001: ch. 5 for details). So this predicts that only about 12.5% of languages of the world will be VSO, as compared to 50% SOV language and 38.5% SVO languages—a prediction which is not far from the observed frequencies. If a substantial proportion of statistical universals emerge as complex

absolute implicational universals in this way, then the prospects for deep and productive interaction between typology and grammatical theory are much rosier.

How likely is it that this program can be successfully carried out? The question, it seems to us, remains open. The pessimist will note that the parameters proposed in the earliest work on the topic (e.g. the pro-drop parameter or the configurationality parameter, both of which were explicitly implicational) have tended to shatter, on close investigation, into smaller-scale and independent “microparameters”. Relatively few new proposals of the same scale have arisen to take their place and current “parameters” tend to allow or disallow, say, Object Shift or Verb Second patterns but predict few correlations. The optimist will note that there does seem to be something true and significant about the headedness parameter, which underlies the Greenbergian word order universals, and that there is no obvious reason why this should be the only parameter of its kind.

This brings us to our final methodological point. Ultimately, the question of whether statistical universals can be transformed into implicational ones, and hence the proper relationship between grammatical theory and typology, is an empirical one. Moreover, it seems clear to us that the kind of research that could best decide this question is done too rarely under either the generative or the typological banner. Generative linguists, when they compare languages at all, typically compare languages from a single family, or only two languages at a time. The positive side of this is that it enables them to dig deeply into the languages in question. But the obvious problem is that they find many spurious generalizations resulting from their very small sample size, and miss many true generalizations by not looking at enough languages. Why haven't these linguists found more interesting large or medium scale parameters? It could be that

these are common enough, but too few formal linguists have spent enough time looking at a range of non-Indo-European (non-Semitic, non-East-Asian) languages to see them.

Typological linguists are also unable to find complex implicational universals, because they look at so many languages at once. This means that their samples inevitably contain errors, either inherited from the descriptions they draw on, or introduced by their own misreadings of those descriptions. This introduces an additional source of noise into their results, so it is unsurprising that most of their generalizations look to be statistical in nature. Perhaps even more significant is the fact that complex implicational universals are harder to investigate at the relatively shallow level of analysis that these studies demand. There is an issue of combinatorial explosion here: there are *many* complex conditionals to consider once one imagines nesting implications or allowing factors to be conjoined. These cannot all be tested against a typological sample in a bottom-up, data-driven manner. Rather, one must have some kind of deeper conceptual analysis that can tell you what combinations might be worth testing—a kind of analysis that is more typical of the formal-generative tradition. Furthermore, the more factors that go into a complex implicational universal, the less likely it is that the descriptions of individual languages will discuss all of the relevant factors, making it harder to use a large sample. These pressures make it difficult for typological methods on their own to find these sorts of universals if they exist.

What kind of methodology might tell us whether or not language is characterized by complex implicational universals embedded in a formal theory of grammar? We suggest that there is a “Middle Way” which will shed light on this question—research that would look at fewer languages than a typical typological study, but at more languages than a typical generative study. This Middle Way would dig into the internal

workings of each language to an intermediate degree, so as to cull out superficial counter-examples and identify additional factors that could be relevant, while still leaving time to look at more than one or two languages. More concretely, we might expect followers of the Middle Way to base their research on 5-10 languages that are genetically and areally unrelated. That would greatly reduce (although not eliminate) the danger of spurious generalizations that besets formal work, while at the same time reducing (although not eliminating) the danger of errors introduced by superficiality of analysis that besets typological work. The results of Middle Way research would probably make more sense to typologists than most generative results do; the typologists could see how to generalize that work by exploring parts of it in the wider samples that they work with. The results of Middle Way research could also make more sense to generative linguists than most typological results do, because some sources of statistical noise would be reduced, leading to sharper (though less certain) results of a kind that invite theoretical understanding. The Middle Way could be pursued either by one researcher in a multi-year research project, or by a team of researchers, each doing careful work on one or two languages with a commitment to combining their results. A few models of both kinds exist in the field.

If these potentials could be realized, the Middle Way could form a kind of conduit for the best results of typology to flow into formal theory and vice versa, providing an escape from those sterile debates between “functionalist” (typological) and “formal” approaches to language design that have too long hindered dialogue and deepening of understanding.

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