Generative Syntax in the Twenty-First Century: The Road Ahead

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1. Strengths and Weaknesses

1.1. A

First of all, I think the field is generally in a good shape, better than it has ever been before. There has been substiantial progress in all relevent domains: More data from many more languages have been investigated, and there have been spectacular theoretical developments over the last few decades, mostly triggered by the move to come up with minimalist accounts. In addition, I take it to be fairly obvious that there is simply no viable alternative to generative grammar (where the concept is understood in a broad sense, as a formal approach that systematically predicts the wellformedness or illformedness of linguistic expressions and is prepared to envisage abstract concepts in doing so); it would seem to me to be the case, for instance, that any potential challenge from pure usage-based construction grammar approaches has by now all but disappeared, due to an absence of well-defined theoretical concepts (e.g., no ontology of theoretical primitives) and an almost complete lack of interesting results.

That said, in my view two basic problems with the generative approach to syntax (as opposed to phonology and morphology) can be identified nevertheless.

The first problem concerns the *algorithms vs. representations* dichotomy. From a representational perspective, one can ask what the structure of a given linguistic expressions looks like; from an algorithmic perspective, one asks how it is generated (or licensed). In the history of the field, one can identify several changes in emphasis on algorithmic vs. representational aspects of syntactic theory, and I think that in and of itself, this is a good thing: After a phase of mainly representational research that has led to better, typically more articulated structural analyses of linguistic expressions in syntax, a subsequent phase of research that is predominantly algorithmic can make use of the new structures and thereby improve on the core concepts employed in the theory; this then triggers a new phase of representational research; and so on.[1]

Against this background, the problem I see is that the field has perhaps put a bit too much emphasis on representational research, and not enough on algorithmic research that should follow it at some point: The cartography enterprise has led to the postulation of many fine-grained structural distinctions, but with very few exceptions (some papers by Luigi Rizzi spring to mind, also the Koopman/Szabolcsi book on verb clusters), people arguing for cartographic structures tend not to spend a lot of time showing how these structures are generated (licensed) in the first place, and what repercussions these structures have for well-established concepts of the algorithmic part. Arguably, similar conclusions hold for much work in nanosyntax (again, with exceptions, e.g. Pavel Caha's work, which also has a focus on algorithmic aspects). And it seems to me that an almost exclusive emphasis on representational aspects also permeates a lot of the work that has been carried out on the syntax/semantics interface in recent years: To put it bluntly, here I sometimes get the impression that people just draw syntactic trees that best seem to correspond to the semantic analysis they want to argue for (given compositionality), and if this requires the postulation of otherwise unmotivated syntactic categories and structures, then so be it. Not to be misunderstood: I believe that all this research on representations is necessary, and has led to a better understanding of the structure of linguistic expressions, but I take it that from the point of view of grammatical theory (more generally, of research on what constitutes the faculty of language), the core question is ultimately not so much what the structures of sentences look like, but rather, what the building blocks (rules, constraints, elementary operations) look like that generate (license) these structures, and how they interact. If one does not address these questions, syntactic analysis will necessarily remain on a descriptive level, much like the approach to syntax in terms of fine-grained topological fields that plays such a big role in traditional research on Germanic languages.

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[1] A simplified example illustrating this is the theory of movement: First there was a predominantly algorithmic approach based on constraints on movement (in the 60s); then there were more articulated represetations containing traces (in the early 70s); this led to a better version of movement theory (algorithmic progress); then there was the introduction of copy theory (representational progress), which in turn gave rise to many interesting new suggestions in the theory of movement (e.g., multidominance: algorithmic progress); etc.

The second potential problem is of a very different nature. A complaint that would seem to be widespread among senior researches working in the Chomskyan tradition is that there is a certain lack of coherence of the field nowadays, and that this is inherently a bad thing. The current situation is then often unfavourably compared with the period when Government and Binding (GB) theory provided a unified, reasonably homogeneous frame of reference in which syntacticians share a huge number of basic assumptions. I do not agree with this assessment. On the contrary, I think that it is great that generative syntax right now presents itself as an open, multi-coloured, diverse paradigm where few things are set in stone (but, of course, *some* are), where scholars may pursue different, and also unorthodox, paths of analysis and collaborate with researchers from different theoretical backgrounds, and where the field is not characterized by an us-vs.-them mentality. As a matter of fact, I would surmise that progress was actually impeded rather than facilitated by GB theory's fairly strict core of hypotheses and principles because non-standard analyses within the Chomskyan paradigm, as well as original analyses in minimally different generative approaches (GPSG, LFG, later OT) were generally neglected even where they could (and perhaps should) have informed orthodox GB-style research.

1.2. B

The obvious solution to the first problem is that (a) it is widely identified as such, and (b) more emphasis is accordingly put on teaching of algorithmic concepts in syntax intro classes at every level.[2]

The solution to the second problem is even simpler, assuming that imhomogeneity of research in generative syntax is not a fault but a virtue: On this view, there is no reason to fondly reminisce about what one takes to be golden years, and the only real problem that is then left in this area is that by complaining too much about the current status of the field, both people outside the field and, even more importantly, young researchers within the field are given the impression that generative syntax is somehow beyond its prime – which it clearly isn't.

3. Syntax in Relation to Other Fields of Linguistic Inquiry

3.1. A: Morphology-Syntax Interface

I think there is very strong evidence that morphology is not simply syntax "all the way down"; the mechanisms that determine the structure of words are at least to some extent "morphomic" (in Mark Aronoff's sense) – e.g., inflection class is a grammatical category that is very important in morphology but plays no role in syntax, and the same arguably goes for decomposed features of the standard (i.e., syntactically motivated) grammatical categories (number, person, case, tense, etc.). The conclusion that there are specific morphological concepts and building blocks (rules, constraints, operations) seems to be shared by all current theories of morphology (Distributed Morphology, Paradigm Function Morphology, Network Morphology, etc.), and closer inspection reveals all these approaches to converge on a surprising number of common ideas (e.g., the role of underspecification and specificity in inflection). However, given that morphology and syntax are both involved in generating complex structures, it is surprising that the analyses given for the two subdomains are so very different in nature. For instance, whereas current minimalst syntax works with extremely simple and elegant operations, all versions of current morphological theory employ baroque operations of exponent realization and feature manipulation (like vocabulary insertion, fusion, fission, dissociation, impoverishment, readjustment etc. in Distributed Morphology; or rule blocks, referral, expanded rule mode, and metarules in Paradigm Function Morphology; and so on) that would not seem out of place in the context of, say, the *Sound Pattern of English*, but simply do not match the beauty and simplicity of contemporary syntactic operations. A unified theory of morphology and syntax that recognizes both the differences and the similarities of the two domains and provides a convincing approach to the morphology-syntax interface is, I believe, an important goal for future research.

[2] As a side remark, I think it's fair to conclude that, whatever their merits otherwise, algorithmic aspects are not a strong point of the more successful GB textbooks by Liliane Haegeman and Andrew Radford, in contrast to less widely used textbooks like those by Henk van Riemsdijk & Edwin Williams, or by Robert Freidin. This would then seem to suggest that choice of textbook may have been an amplifying factor in the development away from algorithmic approaches.

1.3. B: Experimental Subdisciplines

It is my belief that experimental subdisciplines and syntactic theory are, unfortunately, not yet so close that results in one domain can be taken to be directly relevant to the other domain. The underlying rationale is that the levels of abstraction are still radically different. Therefore, I also think there is reason to be skeptical of the work that has come out of *experimental syntax* approaches. These approaches are designed to provide a better empirical basis for syntactic research. However, my impression is that they have managed to draw a lot of engergy from the field (because time spent on working on experiments and their interpretation is time taken away from theoretical analysis), without actually providing either a better empirical basis (because the competence/performance distinction is ignored, and idealization and abstraction become much harder, or even impossible) or new theoretical insights (I am not aware of a single convincing theoretical innovation that was possible only because of experimental syntax). For this reason, even though it may create short-term problems from a political point of view, I think a case can be made that generative syntax should, at this point, focus on its own development, by incorporating more and more data from lesser-studies languages and detecting abstract patterns in the data that lead to empirical generalizations, which in turn lead to plausible candidates for elementary building blocks of the faculty of language.