

Norbert Hornstein

1. Strengths and Weaknesses:

Intellectually, contemporary Generative Grammar (GG) is unbelievably healthy. Due to the work in the mid to late 20th century, GG has an impressive body of doctrine and mid level laws that are empirically well grounded. We identify these “laws” as “effects,” e.g. binding effects, island effects, control effects S/WCO effects, etc. This is an enviable accomplishment and one that should not ever be forgotten or diminished. Don’t misunderstand: these “laws” are not perfect, but they are very very impressive.

In addition, there continues to be excellent typological work refining these laws and extending their reach. The intensive cross-linguistic study of various languages and their Grammars (G) that began in earnest in the mid 1980s continues even stronger today. I doubt that there has ever been as rich and varied a group of grammatical descriptions as exists today. This is really subtle and excellent work and it has immensely enriched our understanding about the variety of ways that G structure gets realized.

So, as far as the descriptive/typological enterprise goes (the one that explores the variety of Gs), things are better than ever. However, I believe that GG has decided, consciously or not, to narrow its vision and as a result linguistic *theory* has gone into abeyance. What do I mean by this?

It helps to start by asking what the subject matter of linguistics is. There are two related enterprises; descriptions of native speaker’s particular Gs and descriptions of human capacity to acquire Gs. The latter aims, in effect, to describe FL. There have been three strategies for pursuing this inquiry in GG history:

1. Inferring properties of FL from G’s up
2. Inferring properties of FL via Plato’s Problem (PP)
3. Inferring Properties of FL via Darwin’s Problem (DP)

Of these the only currently widely pursued route to FL is via 1, the typological-comparative strategy. Both 2 and 3 are currently, IMO, very weak. Indeed, PP considerations have largely disappeared from the research agenda of syntacticians (we have offloaded some of this to students of real time acquisition, but this is a slightly different enterprise) and DP is at best boilerplate.

Moreover, I don’t believe that using route-1 is sufficient to get a decent account of FL, as there is an inherent limitation to scaling up from Gs to FL. The problem is akin to confusing Chomsky and Greenberg universals. A design feature of FL need not leave overt footprints in every G (e.g. island effects will be absent in Gs without movement) so the idea that one can determine the basic properties of FL by taking the intersection of features present in every G likely is a failing strategy.

This does not mean that route-1 is unimportant for investigating FL (i.e. UG). But it does imply that it won't focus on questions and properties of FL that strategy 2 and 3 will. Thus it is, at best, IMO, incomplete.

Let me illustrate what I mean. As Chomsky has rightly emphasized, there is always a tension between descriptive and explanatory adequacy. The wider the range of descriptive tools at our disposal, the easier it is to make distinctions among various phenomena and the easier it is to cover divergent data points. The secondary place of theory is evident in the reluctance to ever dispose of a G mechanism. Let me give you an example. Early Minimalism distinguished between interpretable and uninterpretable features. For various conceptual reasons it was argued that it would be better to substitute valued and unvalued for interpretable and uninterpretable (I never found these arguments that convincing, but that is not the relevant point here). Ok, so in place of +/-I we get +/-valued. As it happened, the substitution of valued for interpretable had a very short half life and now many (most?) GGers take Gs to exploit BOTH notions. So in place of a two-way distinction, we now have a four-way distinction. Not surprisingly, having two extra ways of cutting up the pie allows for greater empirical suppleness. Have the theoretical concerns of complicating things this way been addressed? Not to my knowledge despite this theoretical inflation's ramifications both for PP and DP.

This is but one example. I think that this is inflationary two step is very common. A suggests principle P. B suggests replacing P with P'. C comes along and argues that both P and P' are required. Rinse and repeat. There is no doubt that such conceptual inflation can have payoffs. However, there is nary a word about the theoretical costs of doing this. Or, to put this more pointedly: the empirical costs of remaining conceptually modest are always tallied while the explanatory costs of inflating the basic theory are rarely considered. This tilts inquiry away from theoretical work.

There are other examples. My favorite hobbyhorse regards the huge overlap between AGREE as a Probe Goal relation and I-merge. But I will save this for the next question.

Let me end with a general point, a practical suggestion and an exhortation.

Nobody denies the importance of category-1 work like that above. However, category 2, and 3 are also important. We should insist that PP and DP questions get asked for all proposals. When an analysis is delivered we should ask how the relevant grammatical operations could be acquired. We should ask concerning the operations how they bear on DP. We should not treat G descriptions as ends in themselves but as way stations to the deeper question of how FL is organized. And even if we cannot deliver answers, should insist that these concerns *not* be shoved aside and forgotten. In the best of all possible worlds, we should even be ready to live with some recalcitrant data points rather than expand the theoretical apparatus in PP or DP unattractive ways.

The suggestion: I think we need to retrain aspiring GGers to understand PP and DP. If my own experience is any indication, many GGers have problems deploying a PoS argument. Thus, the practical consequences of PP and DP reasoning for the practicing syntactician

are far from clear. I suggest making this be part of any discussion. We may decide to put it aside, but foregrounding these concerns may have the beneficial effect of advancing respect for explanatory adequacy. It should also provide theoretical grounds for rejecting formal inflation. This would in itself be a very good thing.

The exhortation: I began by noting the tremendous intellectual accomplishments of GG. To repeat: GG has an impressive set of results. Moreover, these results should serve as starting points for furthering GG's investigation of the fine structure of the Faculty of Language (FL). We now have results we can build on theoretically. So, in the best sense, there has never been a better time for doing good theoretical work that addresses PP and DP concerns, the problems that got many of us (e.g. me) into GG to begin with.

2. Central unresolved theoretical issues

There are a whole bunch. But in the minimalist setting I think that three immediate ones that stand out in my mind.

First, what to do about islands and the “antecedent government” parts of the ECP? Island theory (aka Subjacency and Barriers) used to be the jewel in the crown of linguistic theory. Within minimalism, islands are theoretically poorly understood.¹ The deficiency is four fold:

1. Phases, as currently understood, do not comfortably accommodate islands. Many have noted this, but it doesn't make it less true. There is a straightforward way of *translating* island effects via subjacency theory into phase terms (and the translation is not any worse than the earlier subjacency account) but it is not better either. This means that we have gained no insight into the structure of islands that removes their linguistic particularity (thereby making them DP problems). Here are some questions: are Ds phase heads? If so, how do they differ from v and C? Why can't they have edges that can be used for escaping islands? Why are C, v and D phase heads? These are all well-known questions to which we have offered no very enlightening answers.
2. Though it is possible to translate subjacency effects into phase terms, it is much harder to do the same for ECP effects. First there is the problem of what ECP effects are effects of. The ECP was a trace licensing principle. Traces within GB were considered in need of “help” due to their lexical anemia. But, there are no traces in minimalist theory, only copies/occurrences. Why then are some copies/occurrences invidiously distinguished (e.g. why are arguments being more leniently treated than adjuncts?)? Moreover, there is some evidence (mainly from Lasnik and his students) that whereas argument island effects can be obviated via ellipsis, this seems less true for adjuncts. Why? What's it mean to say that “LF” well formedness conditions apply to adjuncts but not arguments? Moreover, why are the domains relevant to the ECP so like those relevant for

¹ Most curious to my mind is that successive cyclicity, which was in GB very tightly tied to islands, is now largely divorced from such concerns.

- islands? It seems like these should really be treated in the very same way, but it appears that they are not. In other words, ECP effects seem like they should reduce to subadjacency effects but they appear not to. What's up?
3. Why are island effects restricted to movement dependencies? Why doesn't binding obey islands? Are these really PF effects and if so in what way are they such? Do islands only apply to movement chains that terminate in a phonetically null deleted copy and if so why? These are all really the same question. We have had some insights into these questions from the ellipsis literature, but there is lots that we still don't really understand. But what would be really nice to know is what exactly it is about a chain's terminating in a gap that matters to islandhood.
 4. As Luigi Rizzi has pointed out, there appears to be a kind of redundancy between phases and minimality. Do we require both notions? Can we reduce one restriction to the other? How much is long movement via intermediate positions feature driven? And if it is, can we use these features to explain movement limitations via something like relativized minimality?

Second, what to do about AGREE and I-merge. I mentioned this before, but it strikes me that the Probe-goal technology and AGREE based Gs that also include I-merge are massively redundant. How so? Well the "distances" spanned by movement (A-movement in particular) are often the same as those that show up in agreement configurations. This is obvious for the many cases of agreement where the surface configuration is spec-head. But it is even true for the cases of inverse agreement. It is seldom the case that the span of an inverse agreement relation differs from that found in overt movement. This is why, I believe, Movement is often analyzed as AGREE+EPP.

I find this very problematic. It seems to me to go against the great (late) minimalist idea that movement and structure building are flip sides of the same coin. It undermines the idea that movement is an expected design feature of Gs. Why? Because, if movement is parasitic on AGREE then there is little reason to expect to see it. There already exists a way of establishing non local dependencies between expressions in a phrase marker, AGREE, and it can apply independent of I-merge so why does I-merge ever apply. Our current answer is the EPP. In other words, it applies *because* it does apply. True, but not enlightening. Let me put this another way: Early minimalism took movement to be a design flaw. Later minimalism took it to be an inevitable by-product of the simplest version of Merge. Current theory takes it to be a design flaw of the perfect theory? Hmm.

What's the alternative? Well, that there is no long distance AGREE operation. All non-local dependencies are mediated via I-merge. This is the "old" minimalist idea that feature checking is in Spec-head configurations. Agree was thus a rather local head to head relation that takes place within a restricted domain. This idea was, IMO, hastily abandoned at the expense of the minimalist conceit that aimed to reduce the theoretical machinery so as to make it more DP tractable. However, given a copy theory of movement and a single cycle theory it is possible to mimic long distance agree with movement and deletion of the higher copy. Given that this is probably required anyhow for other cases, why introduce a novel operation, AGREE, and a novel dependency (probe-goal) as primitive features of Gs?

Responding to these kinds of Darwin-Problem arguments (and eliminating AGREE as a primitive operation) has serious theoretical consequences, which may indicate that this is not a tenable move. For example, here are two:

1. We will need headedness in the syntax (note just at the CI interface for interpretation as some currently assume (e.g. Chomsky)) to make it work. Thus labeling will be a syntactic operation as it is required to allow heads to locally converse. This is not currently the rage, at least not if we go by Chomsky's latest "Problems of Projection" papers on the issue (which, I should confess, I am not moved by at all, but that is a topic for another discussion). To be clear: moving in this direction might complicate the minimalist conceit of treating Merge as the magic sauce that launched FL. If labeling is a basic syntactic operation, then Merge alone does not suffice. I believe that there are ways of navigating these theoretical shoals, but adding labeling is not theoretically innocuous.
2. Anaphoric dependencies are not products of feature agreement. I think that so understanding anaphora is a bad idea on minimalist grounds. Nonetheless, if AGREE does not exist as a grammatical operation then treating binding and anaphora in terms of AGREE is simply a non-starter. As many of you know, this is perhaps the most popular way of analyzing anaphoric dependence.

In sum, I think that the theoretical cost of allowing AGREE as an operation additional to I-merge has theoretical costs that we have not carefully considered, and we should.

There are many more hot questions (e.g. should we expect a minimalist FL to be modular?, What exactly is a syntactic feature and why do they exist?, What role if any does morphology play in the syntax (e.g. does it drive operations or is it a relatively useless by-product?)) but I am happy to submit the above two as my prime candidates for current consideration.

3. Syntax and..

There is lots of low hanging fruit in the combination of syntax with work in psycho-linguistics. There is even an interesting high level question relating them: how transparent is the relation between Gs and the systems that put these Gs to use in real time processes. If this is correct, then it provides another window into the basic primitives of FL. This has been very interesting work done on this question. Syntacticians can do quite a bit to help it along. I know this from personal experience given what we do at UMD. For those interested in this, I have reviewed a bunch of this stuff on Faculty of Language so look there. Colin Phillips, Jeff Lidz, Paul Pietroski have done lots of interesting work showing how to combine good formal linguistics with interesting psycho-linguistics. I even think that there are interesting argument forms out there that may be of interest to syntacticians concerning, for example, the right notions of locality or the right format for the semantics of determiners).

What can syntacticians do to help this along? Well, most importantly, learn how to talk to psycho-types. To do most of the work they are doing it doesn't really matter which

vintage syntactic theory they exploit. If you want to study the processing or acquisition of binding, for example, GB binding theory is almost always sufficient. The newest fangled grammatical devices are not always the most helpful. So, don't feed your psycho-friends the latest minimalist wisdom when a good easy to use GB analyses is all that they really need.

Second, be ready to think of things from the psycho point of view. Here's what I mean. IMO, results in syntax are far better grounded than almost any result in psycho. As a result, syntacticians reasonably find it implausible that results from psycho might have consequences for syntax. However, taking transparency seriously lends itself to the unsettling conclusion that results using psycho techniques might provide arguments for rearranging our syntactic theories. We should be ready to consider this a real option and even help develop such arguments. IMO, they are not quite ready for prime time, but they are getting very interesting and, in some areas (e.g. on determiner meaning) have provided strong reasons for preferring some representations over others.

4. I've answered 4 in various ways above. So I won't repeat myself.