Chapter 22

Conclusion
to
Government & Binding
and to this
Discussion

1. *Introduction*

Theorists of Government & Binding (or TGG, formerly) often incorporate chapters such as 'Goals of Linguistics' into their works.[1] The self-reflective discussion of theory describes an orientation that requires an independent syntax; and from the 1980's onward, there is a historical orientation, according to which GB has a policy of reducing language specific restrictions on the syntax. That is, it is better to simplify the transformational statement(s) and to push the complexity, which such a strategy removes, from the transformational relationship(s) into the expression of generalized constraints on s-structure, or to push the complexity into the more elaborate d-structures. As in any theory, it will remain to be seen whether the result is enlightening. But enlightenment (aka 'truth') is always relative.

2. *The path of GB*

It is easy to see that certain the 'discoveries' of Government & Binding arise **not** from grasping the intricacies of language, but from the intricacies of following out the prescribed theoretical program. The result is that a given theoretical concept is frequently added to Government & Binding because the theory requires it at that point, and **not** because language requires it. It is more a matter of saving the theory than of understanding language. Advancements in the theory do not come so much from interactions with some data, but from a continued projection of the pre-theoretic orientation. Concepts arise within

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[1] 'On the Goals of Linguistic Theory' in *Syntactic Structures* (Chomsky (1957); 'Goals of Linguistic Theory' in *Current Issues in Linguistic Theory* (Chomsky 1964); "Methodological Preliminaries' in *Aspects of the Theory of Syntax* (1965); and 'Assumptions and Goals' in *Topics in the Theory of Generative Grammar* (Chomsky 1966). Texts generally follow the same model. Cf. Chapter 1 'Goals' in *Transformational Syntax: A student's guide to Chomsky's Extended Standard Theory* (Radford 1981).

the theory, and then justification is sought for them elsewhere ... sometimes in the data. Two illustrations of these chains of theoretical implication are:

(1) Structure —> Movement —> Comp —> Empty Nodes —> Traces
(2) Structure —> Case —> Govern —> Barrier —> Filter

The citations will come principally from Radford (1981).

The idea of *structure* seems to be unproblematic. We have inherited a familiarity with it from earlier linguistic work in the 20th century, and the notion seems so second nature that it must certainly be true, or if not exactly true, at least not harmful.[2] Radford provides common arguments for the acceptance of syntactic structure within a theory of syntax. 'Intuition' plays a role here [All emphases mine, PWD]:

"... we all share the **intuition** that ..." Radford 1981.35)
"... it seems **intuitively** obvious that ..." (Radford 1981.36)
"... we **intuitively** know that ..." (Radford 1981.36)
"... even now we haven't exhausted our **intuitive** knowledge of ..." (Radford 1981.36)
"... we feel **intuitively** ..." (Radford 1981.36)
"... we'd **intuitively** recognize that ..." (Radford 1981.36)
"... we'd **intuitively** recognize that ..." (Radford 1981.37)

Now "The diagram ... [Figure 1] provides a schematic representation of our **intuitions** about the internal syntactic relations between the various words and phrases in the sentence ... [*This boy will speak to that girl*]" (Radford 1981.36):

[2] Everyone appears to have acquiesced to structure. Even such renown functionalists as T. Givón (1995) argue for the acceptance of structure. Givón identifies those who might reject structure (although no such advocates are named) as exhibiting the "grammar denial syndrome" and as practicing "naive functionalism". There are some who **do** accept naive functionalism and who **do** exhibit the grammar denial syndrome. Cf. Davis & Saunders (1997), Davis, Baker, Spitz & Baek (1998), and Davis (Ms.).

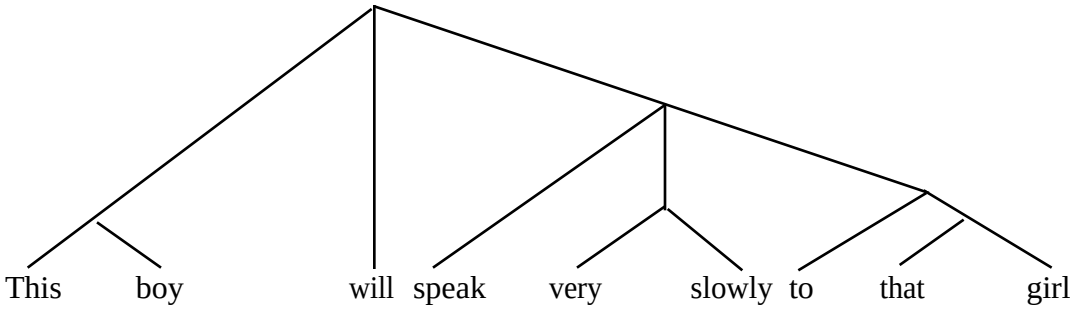

Figure 1: *A "schematic representation of our intuitions".*

If such patterns of syntactic categories are not recognized, we “will miss important generalizations” (Radford 1981.46), and it is “such an assumption [that] makes it possible to provide a principled account of certain types of **ambiguity**” (Radford 1981.55). The ‘intuition’ which is invoked is that of the linguist. Having established the ‘plausibility’ of categories and their distribution in structure(s), “constituents and categories have the status of **theoretical constructs** — i.e., elements without which it is not obvious how we could provide a principled explanation of linguistic phenomena such as coordination” (Radford 1981.60). Now, having established structure as necessary in a theory of language (syntax), we find that we are somehow committed to accept the fact of transformational **movement** (Radford 1981.154):

> In a fairly obvious sense, then, it seems as if the sentence-initial *wh*-phrase in sentences like ... [*Which car will your father put in the garage?*,**Which car will your father go in the garage?*, and **Which car will your father put the bike in the garage?*] behaves for subcategorization purposes *as if it had actually occurred after the verb*, rather than at the beginning of the sentence. But how can we capture the **intuition**? Suppose that we postulate that the *wh*-phrase in each case does actually originate (in a metaphorical [?] sense) after the verb, and only subsequently **gets moved** [Emphasis mine, PWD]...

Whose ‘intuition’? Surely the linguist’s. But this second step into the theory is required ***only*** because we have taken the first step into structure. And in quick succession, after accepting the notion of movement, we have additional problems to solve. Where is the *wh*-phrase going? It requires a landing site/ target, which must now be present as COMP (Radford 1981.168, 171):

> ... it seems plausible to suggest that somehow *wh*-phrases are ‘attracted’ to the initial COMP position ...i.e., we posit that main clauses have an abstract COMP

> (either interrogative or noninterrogative) introducing them, and that *wh*-phrases are adjoined to this COMP ... Thus, within the framework within which we are working, **all clauses** are assumed to be introduced by an (abstract or concrete) COMP-node which marks the clause as interrogative or noninterrogative.[3]

And since movement attracts wh-phrases into the COMP, we may have satisfactorily described the 'intuition' that *wh*-phrases originate in some other position in the structure, but in doing this we create still ***one more*** problem (Radford 1981.191):

> ... what happens to the ... NP-position when *John* moves out of the subordinate subject position into the main clause subject position? Clearly, the lexical material *John* moves, but does the NP-node immediately dominating *John* remain, or is it somehow obliterated? Chomsky's answer is that the NP-node does indeed remain behind, but it is of course **empty** of any lexical material, since the material it used to contain has been moved elsewhere in the sentence.

We have now been lead from structure to movement to COMP to empty nodes (empty COMP and empty nodes vacated), and finally in this series to *traces* (Radford 1981.195-96):

> ... **all** movement rules (including WH-MOVEMENT) leave behind a trace coindexed empty category.

Why? Because we have a difficulty caused by the empty nodes (in turn caused by the movement, in turn caused by the structure) (Radford 1981.194):

> But here we meet a snag: how do we know what the empty node refers to [for the purpose of semantic interpretation, PWD]? ... How can we solve this problem? Chomsky's solution is as follows. Suppose we posit a convention whereby whenever a constituent is moved, the moved category and the empty category it leaves behind are coindexed — that is, assigned some unique subscript numeral which they share.

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[3] In semi-justification for COMP, Radford (1981.173) makes these observations:

> Postulating an interrogative main clause complementizer introducing direct questions is — we might argue — merely an unnecessary fiction,and unwarranted abstraction. But is it really such a strange abstraction: after all, the spelling system makes us of just such an abstract device for marking certain types of structure as interrogative ... namely by use of the *question mark*.

This cannot be a serious justification for COMP.

Radford appears to recognize that the development of such a theory in which an answer begets a problem (recursively begetting an answer, begetting a problem, etc.) will ultimately begin to stretch the limits of credulity. We are moving in the direction of increasingly 'abstract' notions, each justified internally within the theory by a problem created from the acceptance of another notion justified ... (Radford 1981.191):

> ... we may begin to feel that the whole theory is getting so abstract and elaborate that it passes the comprehension of ordinary mortals (i.e., nonlinguists).[4]

The second series of problems and solutions in (2) can be seen to begin again with structure, but to move in a distinct series of developments. It is not necessarily incorrect if a theory adopts/uses a notion which is originally suggested by the application of another within the theory. The task, after all, is to increase our grasp of what/how language is.[5] And elaboration is, as well, not inherently a negative quality. The uncomfortableness comes from the seemingly endless path to this growth. It would feel better if the notions began to turn upon themselves such that one of them informed us of the other(s) and to weave a web of understanding in place of stretching out linearly in an unconstrained way. There seems to be no closure to the ideas and little interrelationship among them.[6]

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4 Do you know how to spell 'hubris'?

5 The linguist's grasp first and then other's, as we explain it to them.

6 Cf. the modularity of GB. Chomsky (1981.135) identifies these:

(1)	(i)	Lexicon
	(ii)	Syntax
		(a) categorial component
		(b) transformational component
	(iii)	PF-component
	(iv)	LF-component
(2)	(i)	bounding theory
	(ii)	government theory
	(iii)	θ-theory
	(iv)	binding theory
	(v)	Case theory
	(vi)	control theory

and remarks that "the system that is emerging is highly modular, in the sense that the full complexity of observed phenomena is traced to the interaction of partially independent subtheories, **each with its own abstract structure** [Emphasis mine, PWD]".

3. *Conclusion to everything*[7]

Where are we now in our conceptualization of language? Collectively, the field of linguistics is not in a good place. But there are prospects for improvement.

3.1 *How we are not in a good place*

Linguistics is not in an advantageous position in two important ways. **First**, as an aspect of human nature, language (whatever its nature may turn out to be) is conceived as isolated from other aspects of what make us human. Language is frequently spoken of as depending upon a 'language faculty' for its existence, and one sense of this is that "the child is, so to speak, 'born knowing' certain facts about universal constraints on possible human languages" (Fodor 1983.4). This body of 'body of information' takes on propositional form, e.g. "x knows (/believes,/cognizes) that P" (Fodor 1983.5) and is a 'knowledge'. Other faculties may include music and number. Such *faculties* appear to be static (Fodor 1983.9):

> ... there must be more to it since, after all, knowledge doesn't eventuate in behavior in virtue of its propositional content alone. It seems obvious that you need some mechanisms to put what you know into action...

And here we have "cognitive processes exhibit the interaction of such faculties as e.g., memory, imagination, attention, sensibility, perception, and so forth, and the character of each such process is determined by the particular mix of faculties that it recruits" (11). Chomsky (1980.39) asserts that:

> We may usefully think of the language faculty, the number faculty, and others, as 'mental organs'; analogous to the heart or the visual system of motor coordination and planning ...

The 'static' quality which Fodor alludes to above is an accompaniment to the assumption that language is a 'thing', and a 'thing' which exhibits the character of a structure. 'Structure' is used here in a broader sense than that of Radford above. It encompasses the 'structure' of Saussure, Bloomfield, and American Structuralism. All take language to be an 'object', somehow the product of articulated components. Some like Saussure emphasize the articulation (a system of pure values); others place more emphasis on the

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7 'Let me make this perfectly clear'. What follows is, more definitely than anything else in this book, my opinion.

components (phonemes and morphemes). But all share the belief that language is a structure. This is the **second** way in which linguistics is in a disadvantageous position.

3.2 *Why we are not in a good place*

We come to this position because linguistics is practiced (as Hockett [1983] as shown us) in the context of intellectual history and it is naturally a continuation of what has gone before. It is easy to see now that there is an unbroken line of thought from Saussure, through Bloomfield, the American Structuralists, and through Harris in particular, through the early Chomsky, Chomsky of the sixties, and the Chomsky of the 1970's and 1980's and Government and Binding. As Harris (1993.37) claims, there ***was*** a revolution:

> ... in the early sixties, it was clear that the old would have to be scrapped for the new. These last developments — accompanied for the most part with concerted beatings of one or more of the Bloomfieldians' sacred cows - caught most of the old-line linguists somewhat unawares. They reacted with confusion, bitterness,and ineffective rage. Rapidly, the whole kit and kaboodle of Chomsky's [sic] ideas swept the field. The entrenched Bloomfieldians were not looking for a messiah, but apparently, many of their students were. There was a revolution.

Yes, all that happened; one party may have replaced another, ***but*** they are still occupying the same palace.

There is no point in rejecting a century's worth of linguistics if there is no reason to, if there is no crisis in Kuhn's sense. But there is a crisis. I take the central issue of the phenomenon of language to be that language means,[8] but what it means to say language means has never been squarely faced. The light of meaning is too bright, and it is easier to associate some darker form with meaning, and then looking away from the light, examine the form while claiming to examine meaning. Even when one looks more directly at meaning, the shield of form is held up. The 'form' of Saussure evolved into the 'structure' of Bloomfield and the American Structuralists, and then into the 'rules' of Generative Grammar. Along the way, the shape of the form merges with that of formal logic (Harris 1993.116):

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[8] I agree with the following statement by Benjamin Lee Whorf (1956.73) written 60+ years ago in 1936:

> What needs to be clearly seen by anthropologists, who to a large extent may have gotten the idea that linguistics is merely a highly specialized and tediously technical pigeonhole in a far corner of the anthropological workshop, is that linguistics is essentially the quest for meaning.

> Logic is, in almost every way, to linguistics as geometry is to physics. Both are formal sciences which play in and around their respective empirical relatives — the sciences of understanding language and understanding matter — often diverging for long solipsistic periods, but always returning,and frequently bearing new fruit.

When meaning is spoken of directly, it is some version of logic that is pressed into service.

3.3 *Why there should be prospects for improvement.*

In the same passage as the last one above from Harris, it is pointed out that not all are enamored of logic (Harris 1993.116):

> Geometry and logic are especially compelling, as one might expect, for the more formally inclined scientists, for the theoreticians and model builders rather than the field workers and experimentalists ...

One aspect of the revolution of the sixties was that most of those who accepted it had never engaged in field work on a non-Indo-European language: Chomsky, Ray S. Jackendoff, Jerrold Katz, Paul M. Postal, George Lakoff, James D. McCawley, and John Robert Ross.[9] The North American tradition of doing empirical studies of American Indian languages, and more generally a tradition of collecting information first hand about languages has continued in a healthy way. To select some names arbitrarily, Ronald

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[9] Chomsky's 1951 M.A. thesis at the University of Pennsylvania was on Hebrew morphophonemics: *Morphophonemics of Modern Hebrew*. But that almost certainly did not entail field work. Chomsky's 1955 Ph.D. dissertation at University of Pennsylvania was entitled *Transformational Analysis*.

Ray Jackendoff's 1969 Ph.D. dissertation at M.I.T. was *Some Rules of Semantic Interpretation for English.*

Jerrold J. Katz' 1961 Ph.D. dissertation at Princeton was *The Problem of Induction and Its Dissolution.*

Postal's 1962 Ph.D. dissertation at Yale under Rulon S. Wells was on Mohawk: *Some Syntactic Rules in Mohawk*. Cf. Postal (1968), which uses Mohawk data on phonology to argue for the 'irregularity' of sound change. The Mohawk material has never to my knowledge appeared in grammatical work by Postal.

George Lakoff's 1966 Ph.D. dissertation at Indiana University was *On the Nature of Syntactic Irregularity.*

James McCawley's 1965 Ph.D. dissertation at M.I.T. was called *The Accentual System of Standard Japanese.*

John Ross's 1967 Ph.D. M.I.T. dissertation was entitled *Constraints on Variables in Syntax*.

Kenneth L. Hale is an obvious and a notable exception to the trend. He worked on Uto-Aztecan, other American Indian languages, and on Australian languages. His dissertation from 1958 was at Indiana University: *A Papago Grammar*.

Langacker has worked on Uto-Aztecan languages; T. Givón has worked on languages of the Bantu family, S. Ute, and a Papuan language; Sandra Thompson has worked on Mandarin and Wappo (Yukian); Paul Hopper has worked on Malay (Austronesian); Wallace Chafe has worked on Iroquoian languages and on Caddo; William Foley has worked on Austronesian and Papuan languages; Robert D. Van Valin, Jr. has worked on Lakhota (Siouan); John Haiman has worked on Hua (Papuan); Sydney Lamb has worked on Mono (Uto-Aztecan).[10] The contrast is striking. While the opposition is not absolute as their dissertations were being written, it is later precisely among those who began their careers as fieldworkers (or those who took up the tradition) that we find theoretical alternatives to generative grammar.[11]

The tradition of fieldwork is growing. In the same way that such work helped to shape a distinctively North American tradition of linguistics earlier in this century, it *may* be that a continuing and growing interest in fieldwork can shape the character of linguistics a second time, ultimately turning the direction of linguistic thought to more realistic conceptualizations of language. There will be no revolution as there was in the sixties ... for three reasons. Fieldwork and its effect (if any) on the linguist take time. Fieldwork takes more time than reading *Syntactic Structures* and *Aspects of the Theory of Syntax*, or even *Lectures on Government and Binding*. The experience of fieldwork is a personal act of discovery. And it is different for each person. Those linguists who are attracted to fieldwork seem to be independently minded. Unlike those students in the sixties, they are not "looking for a

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[10] Langacker's 1966 University of Illinois dissertation was *A Transformational Syntax of French*.

Givón's 1969 dissertation at UCLA was *Studies in ChiBemba and Bantu Grammar*.

Sandra Thompson received her Ph.D. in 1969 from Ohio State with the dissertation *On Relative Clause Structure in Relation to the Nature of Sentence Complexity*.

Paul J. Hopper in 1967 received his Ph.D. from UTexas/Austin for the dissertation, *The Syntax of the Simple Sentence in Proto-Germanic*.

Wallace L. Chafe received his Ph.D. in 1958 from Yale with the dissertation *Seneca Morphology*.

William A. Foley wrote *Comparative Syntax in Austronesian* and received a 1976 Ph.D. from UCalifornia/Berkeley.

Robert D. VanValin, Jr. received his Ph.D. from UCalifornia/Berkeley in 1977 for his dissertation *Aspects of Lakhota Syntax*.

John Haiman's dissertation was published in 1974 as *Targets and Syntactic Change*.

Sydney M. Lamb's 1958 Ph.D. dissertation at UCalifornia/Berkeley was entitled *Mono Grammar*.

[11] It is perhaps noteworthy that the two generative grammarians who were occupied with Amerindian languages at the time their dissertations were being written were students of American Structuralists. Postal was the student of Rulon S. Wells at Yale, and Hale was the student of Charles Voegelin at Indiana.

messiah". They don't follow leaders. That independence is reflected in the number of theoretical alternatives represented among the fieldworkers cited above: Cognitive Grammar (Langacker), Functional-typological Grammar (Givón), Discourse Grammar (Thompson & Hopper), a mixture of cognitive and discourse grammar (Chafe), Iconicity (Haiman), Role and Reference Grammar (Foley & Van Valin), and Stratificational Grammar (Lamb).[12] The factor of time, the independence of the field linguists, and the diversity of theoretical perspectives all work against there being an immediate (or even approaching) change in the climate of linguistics.

3.4 *The last word*

The most frequently shared characteristics of the theories which are alternatives to generative grammar are: (i) some concern with the fact that language means (a 'functionalism'), and (ii) a concern for placing language in its context of usage ('discourse' or 'context'). Yet for all their contrast with generative grammar, these alternatives (and there are more than these), they too, like generative grammar, continue the tradition of the Saussurean sign and the tradition of structure (cf. footnote 1). In my view, even if generative

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[12] **Cognitive Grammar**: Langacker, Ronald A. 1987. *Foundations of Cognitive Grammar, Vol. 1*. Stanford: Stanford University Press.

Functional-typological grammar: Givón, T. 1984. *Syntax: A functional-typological introduction*. Amsterdam & Philadelphia: John Benjamins.

Discourse grammar: Hopper, Paul J. 1979. "Aspect and Foregrounding in Discourse". In *Discourse and Syntax* (= Syntax and Semantics 12), ed. by Talmy Givón. New York: Academic Press.

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_____ & Sandra A. Thompson. 1980. "Transitivity in Grammar and Discourse". *Lg*. 56.251-99.

Thompson, Sandra A. 1983. "Grammar and Discourse: The English detached participial clause". In *Discourse Perspectives on Syntax*, ed. by Flora Klein-Andreu, 43-65. New York: Academic Press.

Cognitive & discourse grammar: Chafe, Wallace L. 1970. *Meaning and the Structure of Language*. Chicago: Chicago University Press.

Iconicity: Haiman, John. 1985a. *Natural Syntax*. Cambridge: Cambridge University Press.

_____, ed. 1985b. *Iconicity in Syntax*. Amsterdam & Philadelphia: John Benjamins.

Role & Reference grammar: Foley, William & Robert D. Van Valin, Jr. 1984. *Functional Syntax and Universal Grammar* (= Cambridge Studies in Linguistics 38). Cambridge: Cambridge University Press.

Van Valin, Robert D., Jr. 2001. *An Introduction to Syntax.* New York: Cambaridge University Press.

Stratificational grammar: Lamb, Sydney M. 1966. *Outline of Stratificational Grammar.* Georgetown: Georgetown University Press.

_____. 1999. *Pathways of the Brain.* Amsterdam and Philadelphia: John Benjamins.

grammar is replaced, assuming that it is, by some version of the theories in footnote 12, it will not be enough. Generative grammar is just one of the excesses in linguistics, and replacing it will only bring us back to some **starting** point.

What may advance us is a conceptualization of language which, **first**, does not take language to be a form. Just as there is a 'substantial' base to the expressive side of language, i.e., something like phonation/audition[13], there is a 'substantial' base to meaning. It is not especially obvious that such a thing should exist and if so what it is like, but I believe that it can be detected. **Second**, the substantial basis is the same for language and for intelligence. **Third**, because of this, language is not (and cannot be) isolated from other human capacities. The ability to make sense of life (become acculturated) and the ability to learn a language exploit/depend on a common human intelligence. **Fourth**, because of the third property, language exists **in** experience. 'Meaning' is in experience from moment to moment to moment ..., and is not separated from it.[14] **Fifth**, because language is in, and suffuses throughout experience, language is not a 'thing'. Imputing structure/form to language (in the broad Saussurean sense) is inappropriate. Nor is language an activity (cf. Davis [1995]). For all its initially odd appearance, such a linguistics can be practiced.[15] This linguistics is productive and I believe it to be closer to the nature of language. But that's just my opinion.

[Version: April 23, 2014]

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[13] Or movement/vision for ASL.

[14] A tradition of science that suggests that we oppose data to theory will then fail here, for the theory must be 'in' the data. The one is the other, and the other is the one.

[15] Its initial difference may make it seem that this position takes language to be something 'abstract', but careful re-reading of the description should convince you that it is just the opposite. In this evaluation, 'language' is necessarily concrete.