REVIEW ARTICLE

Nostalgic views from Building 20

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Kenneth Hale & Samuel Keyser (eds.), *The view from Building 20: essays in linguistics in honor of Sylvain Bromberger.* (Current Studies in Linguistics **24**.) Cambridge, MA: MIT Press, 1993. Pp. xiv+273.

In 1993 a collection of essays was dedicated to MIT philosopher Sylvain Bromberger by his colleagues on the linguistics side of the Department of Linguistics and Philosophy in the famous Building 20 at MIT.[1] Its title, *The view from Building 20*, suggests it provides a representative sample of the department's recent work in linguistics. In a way, it does; but what it reveals may not be what was intended. The predominant feature shared by the views found in Noam Chomsky's department seems to be a high degree of nostalgia. This is work that looks backward, to ideas of a quarter of a century ago. What is generally missing, though, is any direct acknowledgment of that work; if the text of the articles tend to instantiate nostalgia, the bibliographies tend more toward amnesia.

Chomsky's own contribution, 'A minimalist program for linguistic theory', illustrates the point as well as any in the book. Already influential, it will no doubt be the article this book is chiefly remembered for. In it, Chomsky tears down most of the substantive structure of the *government and binding* (GB) framework, thus contriving to reclaim the role of the lone

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revolutionary while, paradoxically, also dominating and defining the status quo to an extent that could not even be dreamed of in most fields. But this revolution is a conservative one, even reactionary.

Chomsky proposes that there may well be only two significant levels of representation in linguistics (levels which he insists are a 'virtual conceptual necessity', an unexplained locution with a 'back to basics' ring to it that has been much quoted by others). Those two levels are (i) phonetic representation and (ii) some sort of semantic or conceptual structure.[2] Taking this view means abandoning the cherished level of deep structure (known as 'd-structure' in the last two decades). This is exactly what Postal (1972) suggested was 'the best theory'. But the names of linguists like Postal, Ross and McCawley, who in the late 1960s tried to *argue* for the elimination of deep structure, are completely absent from Chomsky's bibliography.[3] There is no belated nod in the direction of the literature he resolutely resisted for 25 years (from 1967 to 1992; see Newmeyer (1986: 107ff., and references cited there)) but whose central thesis he now adopts. Nor is there any real effort to supply intelligible reasons and arguments for his abrupt conversion to the tenets of generative semantics. A few remarks are aimed at making the retrogression seem like a natural and desirable development that avoids certain minor technical difficulties of GB theory, but this rapid and unmotivated shift of view is basically not supported by evidence or argument. The implied epistemology is one of miraculous revelation.

For example, no evidence is cited for making the same analytical decision as 1970 generative semantics on the universality of underlying constituent order. Chomsky simply assumes (31) that languages with differing superficial constituent order have the same underlying order. To the suggestion that English is underlying VSO (McCawley 1970), or that all languages are (Bach 1971), Chomsky's response is not the modern (post-1974 relational grammar) position that this is a pseudo-issue thrown up by the error of postulating linear order in nonsurface representations, but instead, a new version of the 'universal base hypothesis': Japanese as an (underlying) SVO language. But while McCawley and Bach gave evidence purporting to support the VSO

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[2] Chomsky still adheres to the idea of a 'conceptual-intentional' level of representation. This is not in line with most thinking in linguistic semantics or philosophy of mind today.

[3] The standard history by Newmeyer (1986: 135–136), displaying a sympathy for the position Chomsky formerly defended, ridicules Postal for having 'constructed a more homogeneous theory only by ceasing to make concrete claims about language', with his 'transparently aprioristic' proposal that syntax should be a unitary mapping between phonetics and semantics; but Chomsky now adopts this position without warning, leaving loyal followers to eat his words as well as their own. (Notice, incidentally, that to save space in this article I have decided not to supply full bibliographical references to the work of the generative semantics era that I mention; I have cited some central items, but the work is all extremely well known; and those who need more pointers than I have given can readily find them in Newmeyer's book, which is useful despite its bias.)

underlying constituent order that they favored, Chomsky does not assume this burden.

By 1973, a number of generative semanticists were maintaining that there is no valid notion of syntactic well-formedness at all. Surprisingly, Chomsky now also adopts that position; but in Orwellian style, even as he abandons the concept of grammaticalness, he asserts that no such abandonment is taking place. I am not exaggerating. Readers acquainted with early work in generative grammar may recall Chomsky (1966: 31–33) railing against an earlier foe of grammaticality, R. M. W. Dixon. In the course of his (rightly critical) discussion of a rather strange book of Dixon's (1963), Chomsky heaped scorn on 'Dixon's rejection of the notion of "grammaticalness" on which all grammatical description, traditional, structuralist, or generative, is based.' Chomsky warned then that 'all known grammatical descriptions are based on an assumed delimitation of grammatical and ungrammatical sentences' because 'if distinctions of grammaticalness are not assumed, there is nothing for a grammar to describe...' (1966: 32, fn. 7). Yet in the work under review (by a strange coincidence, again in a (foot)note numbered 7, on p. 44), Chomsky now makes this remark about the class of 'well-formed (grammatical) expressions' of a language:

> The class so defined has no significance. The concepts 'well-formed' and 'grammatical' remain without characterization or known empirical justification; they played virtually no role in early work on generative grammar except in informal exposition, or since.

The concept of grammaticality not only played a role in early generative grammar, but the role it played was that of being the only data considered relevant in linguistics. What is the motivation for Chomsky's direct falsehood? It may be part of a strategy for undercutting the importance of the extended Montague tradition and post-1980 computational linguistics, where grammaticality distinctions have been taken fairly seriously and work has pursued a trajectory leading away from Chomsky's ideas.[4] If a rhetorical undercutting of modern work on formal grammar is not the motivation, I confess myself baffled; but the claim that the concept of grammaticality played no role in early generative grammar is certainly an untruth.

Examples of Chomsky's late conversion to some of the cardinal tenets of generative semantics abound. He accepts that tense elements are not minor affixal members of clauses but the verb-like heads of full projections that form properly containing higher constituents, as famous works by Ross and McCawley argued. Lakoff, McCawley and others have been expressing skepticism about referential indices for more than a quarter of a century, and

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[4] Chomsky waves away computational concerns later on p. 33 when he notes that his principles of economy of derivation may lead to 'high-order computational complexity'; he waffles briefly about whether this is a problem, and passes on.

Chomsky now concurs without acknowledgment (on p. 49, n. 52). And so on.

I stress the many failures of appropriate citation not because of the ethical point that generative semanticists are being cheated of credit that is their due (though they are); my concern is to avoid wasteful and repetitive patterns of scientific exploration. If someone is going to propose a reversion to a decades-old position, it should be done explicitly. If we know that a position is being revived from years ago, we can save time by referring back to contemporaneous arguments for or against it. If the antecedents are denied or the reference to earlier literature suppressed, linguists may waste a lot of time, not just reinventing the wheel but reiterating attempts to square the circle.

The new theoretical ideas of this deep-structureless 'minimalist program' are sketched so hesitantly as to be hard to describe, but in rough outline they are as follows. Chomsky takes syntactic descriptions to be derivations anarchically constructed by plugging together (in extended Montague style) random subtrees of the form $[_{X''}(Z'')[_{X'}X(Y'')]]$. Base rules are claimed to have been eliminated, but this is not true; the base rules, though now universal, are still there implicitly, defining the X-bar theory that Chomsky assumes. In fact, one can give an exact definition of Chomsky's set of base rules. Given the set $\mathbb{C}$ of zero-bar-level categories, the full set of base rules, which has cardinality $2(|\mathbb{C}|^2 + |\mathbb{C}|)$, is:

$$\{X'' \to Z''X' \,|\, X, Z \in \mathbb{C}\} \cup \{X'' \to X' \,|\, X \in \mathbb{C}\} \cup \{X' \to XY'' \,|\, X, Y \in \mathbb{C}\}$$
$$\cup \{X' \to X \,|\, X \cup \mathbb{C}\}$$

(For those who would like to count finite rule schemata as single rules, rather than multiplying out by the number of categories there are, the number is $2(1^2 + 1) = 4$, but when criticizing phrase structure theories for having too many rules, MIT linguists have always expanded out and multiplied; a fair comparison demands a consistent counting practice. What is important, though, is that regardless of how one counts them, there are axioms of Chomsky's theory stating what phrase structure configurations are permitted, just as there are in GPSG and HPSG.) The 'substitution' operation that plugs the base-generated subtrees together is named ('GT' for 'generalized transformation'), but not stated. What is it like? A link to Aravind Joshi's tree adjoining grammars is implied on p. 21, where Joshi's name is mentioned in connexion with 'devices like' generalized transformations; but there is no indication that Chomsky has grasped the import of Joshi's fecund and wide-ranging research program, or even read the work. Those who have studied the work of Joshi and his collaborators will know how much in a syntactic theory can depend on subtle differences in the exact statement of the operations for plugging subtrees together. Chomsky does not make clear how the operation he has in mind compares with Joshi's 'substitution' and 'adjunction' operations. The reference to Joshi's work

may be highly misleading, but Chomsky is far too inexplicit for there to be any way to tell.

In the trees that are composed (somehow) by GT, chains are randomly formed by an operation called 'Form Chain', which replaces movement. We are given one example of what 'Form chain' does, on p. 15, but again, no definition of the operation. Phonological material paired with inflexional features is randomly inserted under lexical nodes at any point by an operation called 'Spell Out'. Once again, there is no definition of the operation. A successful derivation is one that succeeds in linking an interpretable PF (phonetic) representation to an interpretable LF (logical) representation. (Interpretable by whom or what? It is not clear.) There is a 'requirement that substitution operations always extend their target', which Chomsky claims 'yields a version of the strict cycle'; but the version is not characterized (note 25 hints that successive cyclic rules are a problem and note 28 suggests cyclicity does not hold for adjunctions), and the alleged theorem deriving it from the (unstated) definition of GT is, naturally, not proved.

The paper is replete with this kind of vagueness. An egregious example concerns the principle 'Procrastinate'. This name implies an exaggerated recourse to temporal metaphors about derivations. 'Procrastinate' says that movements should be left till 'later'. It is described as 'a natural economy condition: LF movement is "cheaper" than overt movement' (30). As if sensing that this care-quoted allusion to economics is insufficient explication, Chomsky elaborates: 'LF operations are a kind of "wired-in" reflex, operating mechanically beyond any observable effects.' This quadruple mix of metaphors ('wired-in for electricians, 'reflex' for physiologists, 'mechanically' for engineers, 'observable' for astronomers) does not improve things, of course; so Chomsky moves to a transportation systems analogy: 'The system tries to reach PF "as fast as possible", minimizing overt syntax'. (As fast as possible, or as cheaply as possible? These are not usually the same in transport economics.) The level of explication, in short, is risible.

The 'more intricate' principle of 'Last Resort' (32) says 'a step in a derivation is legitimate only if it is necessary for convergence – had the step not been taken, the derivation would not have converged.' One thing that is clear about this is that it is a TRANSDERIVATIONAL constraint: it quantifies over derivations. David Perlmutter, Paul Postal and George Lakoff suggested such constraints in the early 1970s (see Newmeyer (1986: 121)). Hankamer (1973) explored the idea that universal grammar incorporated a trans-derivational constraint preventing ellipsis from introducing ambiguity. None of this work is referenced. No explanation of Chomsky's startling reversion to 1970-vintage generative-semantics ideas emerges, other than the epistemology of miraculous revelation. And once again the history of Chomsky's fierce opposition to these ideas is suppressed. (It should also be noted that hardly anything was ever determined about whether grammars

using transderivational organizing principles can actually work; Jacobson (1974) studied the topic, and ended up pessimistic on the grounds that mutual-dependency paradoxes seemed to arise.)

Adding a new layer of murkiness, Chomsky proposes (33) that 'Last Resort' is always 'self-serving', and renames it 'Greed'. It says that phrases only move because they need to have their morphological features checked by certain other constituents that are stipulated as qualified for this job. (In Chomsky's system, fully inflected words are inserted randomly into syntactic structures, and then constituents – nearly all constituents, even in quite short sentences – are moved into positions where their features can be 'checked', and most derivations 'crash' because of features that fail the checking.) It is claimed that a constituent α cannot move 'to enable some different element β to satisfy *its* properties' because 'benefiting other elements is not allowed'. (Benefit? *Cui bono?*)

The upshot is a theory of grammar under which what is wrong with Chomsky's example *Seems to a strange man that it is raining outside* (33) is not simply that it has a finite main clause lacking a subject, but rather the following: the 'Greed' principle refuses to allow the NP *a strange man* to move to the subject position, where the abstract Tense node above *seems* could have assigned Case to it, because the NP can get all its morphological needs met by staying right where it is, which means the derivation 'crashes at LF', since the abstract Tense node has a Case feature to 'discharge' but cannot find anywhere to discharge it. This metaphorical and anthropomorphic talk of phrases moving in a bid to get their needs satisfied, and abstract nodes yearning to discharge their feature burdens, is supposed to be theoretical progress in the characterization of human language. Indeed, it is supposed to be neuroscience (as casually alluded to in his opening paragraph, Chomsky takes grammars to exist physically in the human brain). What it seems to me to represent is a complete collapse in standards of scientific talk about natural language syntax.

Apologists will point out Chomsky's prefatory remarks on p. 5: 'I had hoped to present an exposition in this paper, but that plan proved too ambitious. I will therefore keep to an informal sketch, only indicating some of the problems that must be dealt with.' But Chomsky has been handling out disingenuous promissory notes about the preliminary character of his manuscripts for at least fifteen years (recall the eyebrow-raising starred footnote of 'On binding' (Chomsky 1980), admitting that it is an unrevised first draft published only because of public demand). This article of 52 pages is long enough that, even in a sketch, some of the key theoretical concepts could have been given coherent characterizations if those were available; but apparently they are not.

The second contribution to this view from Building 20, Kenneth Hale & Samuel J. Keyser's 'On argument structure and the lexical expression of syntactic relations', continues a series of these authors' papers on argument

structure. The primary claim is that 'the proper representation of predicate argument structure is itself a syntax' (53). This is a truism: of course predicate-argument structure is syntactic; PREDICATE and ARGUMENT are inherently syntactic terms (to put it in Charles Morris' terms, they concern relations between signs, not about relations between signs and what they denote). What Hale & Keyser mean is that internal to the lexical representation of a word they claim there are phrases, X-bar projections, and movement operations constrained by the usual conditions applicable (under GB assumptions) to the rest of syntax. In other words, Hale & Keyser have revived syntactic lexical decomposition: their intralexical syntax looks a lot like 1968 prelexical syntax (see, for example, McCawley 1968).

Hale's extensive knowledge of Amerindian and Australian languages and Keyser's expertise in the history of English play scarcely any role in this paper; the evidence is virtually all from Modern English syntax. And the analyses look like not-so-modern syntactic theories of English. Hale & Keyser derive *shelve the book* from [V [*the book* [*put on the shelf*]]] (56), *thin the gravy* from [V [*the gravy* V *thin*]] (79), and *get mud on the wall* from [V [*mud* [*get on the wall*]]] (86–87). Their unfilled V nodes, to which embedded verbs move, basically as in McCawley's Predicate Raising, might just as well be shown as abstract causative or inchoative predicates, since that is what (somehow) happens semantically.

Sadly, Hale & Keyser follow Chomsky's citation practice acknowledgment of the generative semantics literature of the late 1960s is missing.[5] And here we encounter a good example of the danger of wheel-reinvention that I mentioned above. ANTI-generative semantics critiques of lexical decomposition in the 1970s, though relevant, are also ignored by Hale & Keyser. Fodor's (1970) arguments against deriving *kill* from *cause to die* are as applicable to Hale & Keyser's structures as to McCawley's. For example, *have mud get on the wall on purpose* has two readings, a sensible one where the causing is done on purpose and a bizarre one where the mud is purposeful (compare *have students sit on the floor on purpose*), but *put mud on the wall on purpose* is univocal (like *sit students on the floor on purpose*). Fodor's rule of thumb that there will be *n* event references that can be adverbially modified only if there are *n* verbs in the structure predicts correctly here, and Hale & Keyser's complex structures appear not to. Perhaps there is a way of defining things so that the right consequences ensue; but then there may have been ways of fixing things within generative semantics, too. The point is that Fodor's reasons for not positing extra abstract verb nodes should be addressed, not ignored.

The third contribution, 'Distributed morphology and the pieces of

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inflection' by Morris Halle and Alec Marantz, defends the view (long familiar within transformational grammar) that morphology, rather than making up a component in its own right, is distributed through other components as an assortment of quasi-syntactic rules, quasi-phonological rules, and readjustment operations. Hall & Marantz defend an odd mélange of 1958-style formatives, 1968-style readjustment rules, 1978-style movement rules, and 1988-style functional heads. Arbitrary transformations on structured strings of abstract 'morphemes' wrestle surface syntactic orders into configurations more suited to deriving phonological and phonetic representations, but in a way that attempts to maintain compatibility with Pollock's phrasal projections founded on such 'functional' elements as tense and agreement affixes. A new layer of incredibility is added to the profligate Pollock-style 'exploded Infl' syntax when Halle & Marantz accept the distinction of T (Tense) from Agr (Agreement) in s-structure but then fuse them together in Morphological Structure. That is, in the surface syntactic structure of *Birds eat* they have *eat* and its tense and agreement suffixes as three different constituents (indeed, three different phrasal projections), but in morphological structure this is not the case. Surely it would be more sensible to maintain the diametrical opposite: that *eat* is morphologically analysable in terms of *EAT*, *Pres*, and *3pl*, but is a syntactically unitary verb constituent.

Halle & Marantz devote a large proportion of their article to an attack on the impressive defense of traditional word-and-paradigm morphology presented in Anderson (1992). The assault seems unmotivatedly aggressive. True, Anderson does, unlike Halle & Marantz, reject the idea of morphs as syntagmatic elements. But he hardly differs enough from Halle & Marantz on the linguistically relevant properties of words, or on what generalizations need to be captured, to motivate the disproportionate vigour of Halle & Marantz's assault. Halle & Marantz argue not like scientists interested in theory improvement but like crusaders defending a faith against a minor heresy. They falsify history, using the term 'the traditional approaches' for the structuralist and transformationalist morpheme-based ideas opposed by Anderson (112), as if the post-1930 American structuralist excursion had the four-thousand-year history of the word-and-paradigm model (on which see Black (1989)). They attribute to Anderson views that he clearly does not hold; for example, having alleged that he 'neither offers alternative analyses nor indicates any intention to revise syntactic theory', they affect to believe that 'he accepts the current view' that inflexional affixes define separate syntactic projections (112), whereas in truth Anderson explicitly assumes morphosyntactic features on phrase nodes, as in GPSG (see his pp. 107ff.). They ignore clear strengths of Anderson's view, like the desirable consequence that syntactic rules cannot affect proper subparts of words. And they expunge more unsavory heretics from history altogether, for example, on pp. 132–133 they attribute to Anderson (1992: 61) a Latin-based

argument against zero morphemes that is not from Anderson, but from another work, which he cites. (The authors of that work are evidently too evil for Halle & Marantz to mention, so I will not risk mentioning them either.)

In their own theory construction, Halle & Marantz avail themselves of an unusual and powerful palette of ill-delineated devices. Among these are morpheme fissions, fusions and mergers (fusions and mergers being distinct: p. 116), diacritic features that make suffixes optional (126), zero morphemes of two types (133), context-sensitive constraints on lexical insertion (136), morphological well-formedness constraints on affixal morphemes capable of forcing insertion of extra verb nodes at a new level of Morphological Structure (137ff.); impoverishment rules that delete morphosyntactic feature specifications under identity with those in other nodes (156); and so on. It is an ornate and over-powered yet at the same time oddly atavistic theory, redolent of the complicated rule systems and derivational histories of the 1960s, utterly unaffected by the dramatic developments that elsewhere have brought declarative formalisms and non-procedural thinking (and default systems that embody non-monotonicity without derivationality) into modern morphological description.

Oddly, when Halle & Marantz come to where their summary and conclusions should have been they feel obliged to present instead an essay on Chomsky's minimalist program. That program is in fact thoroughly incompatible with Halle & Marantz's position, but they mull over it politely for five pages, ending their paper in a state of indecision with a lame suggestion that more research is needed – a nod of respect toward the fleet flagship instead of an intelligible summary of where their own boat has taken them.

The other work in this volume will doubtless be neglected while the spotlight lingers on Chomsky's minimalism and work associated with it. This is a pity. The remaining four papers, making up only one third of the book, are somewhat better than the ones discussed above. One chapter of the book is sharply differentiated from the others in virtue of being focused on pedagogy. Maya Honda and Wayne O'Neil, in 'Triggering science-forming capacity through linguistic inquiry', report some preliminary work testing out the idea that linguistics might be used as a science subject in secondary and even primary schools. Their paper is somewhat programmatic, and the linguistic topics discussed as potential lesson material represent only a tiny sample of familiar topics in the analysis of English (plural suffix allomorphy; the distribution of reflexives; and, this being MIT work, traces and *wanna* contraction), but the idea is praiseworthy and the commitment to improving pre-college science education in America is sorely needed.

In phonology, Michael Kenstowicz presents in 'Evidence for metrical constituency' a survey of the kinds of evidence supporting claims about metrical constituent structure. Showing the clarity and care of his excellent recent textbook (Kenstowicz 1994), it could be quite useful reading for first-

year graduate students. It does suffer, though, from two familiar maladies of linguists. The first is an epistemological affliction: difficulty in telling the difference between (i) data that are well accounted for if construct C is posited and (ii) evidence that C actually exists. Kenstowicz's useful review of reasons for favoring some constituent structure assignments over others when doing metrical analyses should not be confused with a demonstration of the actual existence of metrical constituents.

Kenstowicz's second malady is excessive proneness to temporal metaphors. He talks so procedurally about definitions of stress patterns that it is almost as if he is describing a parsing algorithm for assigning stress to unstressed phonetic strings. But in fact he is just employing a perverse way of talking about what stress patterns occur in languages. This is one reflexion of the pervasive proceduralism that was seldom absent from pre-optimality theory phonology. It highlights the fact that in phonological research, too, MIT's work is starting to look less than progressive. In the last few years, phonological theory in America has not so much embraced optimality theory as turned into it; yet MIT phonology still largely ignores the explosive impact of this framework on the field.

The other phonological paper, 'Integrity of prosodic constituents and the domain of syllabification rules in Spanish and Catalan' by James Harris, offers a detail-rich investigation of the ways in which the integrity of prosodic constituents like the foot help to explain subtle phenomena of syllabification and other aspects of Spanish and Catalan phonology. And the sole semantics paper in the book (the only one, incidentally, to make any meaningful reference to the philosophical work of Sylvain Bromberger) likewise has some serious content: James Higginbotham sets out a specific view on the semantics of interrogatives – associating interrogative sentences with partitions of the possible states of nature into mutually exclusive alternatives and defining answerhood on that basis – in a way that to a considerable extent breaks clear of the standard MIT insularity,[6] being responsive to the literature of philosophy as well as linguistics, and to Montagovian semantics work as well as transformational treatments of *wh*-constructions. It may not be a breakthrough, but it is a mature piece of formally-based work on natural language, a vast distance away from the undisciplined musings with which the book opens.

Although the papers in the last third of the book constitute workmanlike examples of modern linguistics, *The view from Building 20* as a whole does not speak well for MIT linguistics. Far too much of it is sloppy, backward-looking, and partially recycled from unacknowledged earlier sources. And the hallmark *esprit de corps* and collaborative research of the department's linguists is beginning to degenerate into mere half-hearted mutual citation

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[6] As readers of this journal will probably know, Higginbotham subsequently broke clear of MIT altogether, and moved to Oxford University.

(and denial of citation to outsiders), no integrated and progressive research program being apparent. What readers will get from this book, in short, is not so much a view FROM Building 20 but a view OF Building 20. Those who expect MIT to be a source of serious, precisely framed, well-motivated theories of natural language structure will be disappointed, to say the least.

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