REVIEW ARTICLE

Binding and filtering. Edited by FRANK HENY. London: Croom Helm; Cambridge, MA: MIT Press, 1981. Pp. x, 337. £16.95; $27.50.

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This collection opens with a 44-page introduction by Heny, which gives not only an analytical history of how the binding framework developed, but also a more or less thorough discussion of Chomsky's 'On binding' (the first article in the collection) and a rapid, but usually comprehensive discussion of the other seven articles in the book. Heny's introduction thus offers an integrated overview of the material contained here, and renders superfluous part of my job as a reviewer. In order not to duplicate Heny's efforts, I will concentrate on certain particulars of the articles—after first noting who might best benefit from reading them, and how.

Although 'On binding' first appeared in 1980, the other papers cannot be found elsewhere, so far as I know. These include one article each on Turkish, Italian, Dutch, French, Welsh, Icelandic, and Quechua. One (that on Quechua) deals primarily with morphology rather than syntax; but all propose filters and/or binding conditions to solve the problems they address. Given Heny's elaborate introduction, the result is a volume which presents a very helpful and insightful picture of a somewhat transitional phase in recent generative theory (these articles were written from approximately 1977 to 1979, and thus are 'caught' between Chomsky & Lasnik 1977 and Chomsky 1980), and of the types of problems and solutions that are debated within that theory. A major point of Heny's is that binding represents a significant change in syntactic theory: while it indeed offers a generative syntax, the 'transformations' in this theory differ in important ways from classical transformations. In this theory, transformations are NOT defined as rules which can be written with the string formalism (with, perhaps, the addition of Boolean conditions on analysability); linear relationships are NOT the only relevant ones; and factors concerning other types of relationships (like domination information and major category boundaries) are NOT built in by way of general principles (governing which analysis must be taken to satisfy a structural description, such as A/A; or governing the domain of a rule, such as the cycle). Instead, the so-called transformations of the binding theory consist of simple statements of change, such as 'Move NP' (or any other category, symbolized by α); which NP is to be moved is entirely free, and the position to which it can move (left or right, up or down the tree) is any empty NP position. In such a theory, the interesting questions are not how a given change occurred (i.e. not which transformation was involved), but rather how we can interpret the output of whatever rules apply in terms of capturing the original role played by an NP (e.g. which argument of which verb), as well as where that NP has 'traveled'. Accordingly, one major contribution of the binding theory is that it increases our understanding of anaphoric relationships—whether those of control, where an empty subject-

NP node receives an interpretation through association with some other NP node, or those of binding traces and anaphors (like reflexive pronouns or *each other*)—and of semantic interpretation in general.

Although the introduction by Heny is detailed and careful, some of the articles in this volume are difficult to follow, either because their explication leaves something to be desired, or because the data are not fully enough explained to allow all readers to perceive their relevance. I will comment specifically on these points as I discuss each article below. My point here is that this book requires a good deal of work from the reader. For those already familiar with the background against which Chomsky's article was written, the book is useful in that it handles a variety of problems in a wide range of languages, allowing such readers to gain some facility in using binding theory (even though the articles themselves are not always consistent with one another—a point which Heny mentions in his introduction). For those readers who are not so up-to-date on Chomsky's work of the 70's and later, this book might be a bewildering place to start. The choice of issues debated might appear rather arbitrary, and the arguments unnecessarily technical and formal, unless one understood the developments that eventually led to binding theory (e.g. Chomsky 1973, 1976, Chap. III of 1975, 1977, and Chomsky & Lasnik 1977, in that order). For those who want to see how the seeds of destruction of classical TGG first germinated, cf. Chomsky 1971, 1970, in that order. With this preparation, I think any reader can find the present book understandable, and often brilliant. But on the whole, I feel that the collection hasn't quite 'arrived'.

I will now discuss the articles (in the reverse order in which they appear in the volume, as it turns out—not by design); I make no attempt to give equal time.

1. Pieter Muysken's 'Quechua word structure' (279–327) starts out deceptively tedious, but winds up as perhaps the most useful article (besides Chomsky's) in the volume. Quechua has a rich verbal morphology, where a phonological word may express the subject, verb, objects, aspectual information, adverbial information, and more. As such, it exemplifies the kind of language which many scholars (including Pike 1949, cited by Muysken) have pointed out as showing that a morphology–syntax dichotomy is not always useful in the description of a language. Muysken, instead, makes a very interesting case (though perhaps not so strong, as I note below) for maintaining a morphology–syntax dichotomy in Quechua. He argues that word formation and phrase structure have distinct properties which can be used as diagnostics for whether a grammatical process belongs to the realm of word-formation rules (morphology) or of phrase-structure rules (syntax). It is this basic point, plus the ambitious listing of the two sets of properties, that makes this article so original—and, whether right or wrong in details, a significant step forward in our understanding of the distinctions involved. Muysken's article made me think of Wasow's 1977 classic work (which he has since, regrettably, renounced), which aimed at setting up diagnostics for determining what should be explained by a lexical redundancy rule and what by a transformation.

The most obvious potential weakness of Muysken's article is that not all his diagnostics of word-formation phenomena may be correct; and since they are the crucial assumptions upon which his arguments are built, they are the key to the value of his hypotheses. Not being a morphologist, I can say only that these assumptions seem reasonable to me.

Some details of this article confound, and others delight. On p. 290, ex. 18b uses a word translated as 'already', an adverb, as an example of a minor category word. Some explanation is needed here, perhaps a distinction between syntactic distributions (and hence categories) in the Quechua word and in its English gloss.

On p. 293, a potential problem for Muysken is shown to be not a morphologically conditioned process, but rather a process determined solely by stress patterns—hence phonological, and not problematic after all. This is an elegant example of avoiding a well-known pitfall: failure to determine the proper conditioning factors for a process can obfuscate the very nature of the process, and lead to unnecessary complications of some component of the grammar.

Sections 3–4 of the article confuse me. In §3, Muysken argues that word-formation rules and output filters are constrained by subjacency. He insists upon this point, in the face of some apparently intractable problems, because (a) subjacency does seem to constrain most word-formation processes, (b) a subjacency condition nicely limits the power of the word-formation component. But in §4, Muysken argues that morphological readjustment rules are not constrained by subjacency, but only by the notion of c-command. While he claims that readjustment rules follow word-formation rules, since they are both part of the morphology, his argument that subjacency applies in word-formation rules loses much of its force. If one component of the morphology is not bound by subjacency, why insist that another component be so bound—especially when considerable problems arise for that position? The answer (and I hope I'm not being unfair) may lie in Muysken's goal. He wishes to show that morphological processes use the same set of general principles that govern syntactic processes—although he has shown that the way in which word structure is created differs from the way in which phrase structure is created, and hence that these general principles apply in different ways. Among these general principles are the notions of c-command, the cycle, and of course subjacency. If Muysken did not argue that word-formation rules were bound by subjacency, he would have to omit subjacency from the set of general principles shared by morphology and syntax—a regrettable weakening of his proposal. A final point of interest in this article is Muysken's argument that no formal differences exist between inflection and derivation in Quechua.

2. Another gem is Joan Maling and Annie Zaenen's 'Germanic word order and the format of surface filters' (255–78). This tightly-argued paper proposes that Icelandic declarative sentences have the tensed verb in second position; but direct questions, i.e. requests for information (and commands, which are not analysed), have the tensed verb in first position. In this analysis, a WH word or phrase is Chomsky-adjoined to S′ so that it precedes COMP (which is also outside of S, under the original S′); but a topicalized word or phrase is Chomsky-adjoined to the left of S, so that TOPIC is immediately dominated by S (not S′). In both types of sentences, the tensed verb APPEARS to come in second position; but actually, only in the topicalization is the verb in second position in the S. M&Z propose that all declarative tensed clauses in Icelandic are constrained by a surface filter which requires the tensed verb to be the second constituent of S. This analysis, by placing word-order constraints on S and not on S′, allows us to use the same Subject–Verb Inversion rule in both WH and *yes/no* questions. One result is that the difference in word order in English between WH questions and Topicalized sentences (where the first has the tensed verb following the initial constituent, and the second does not) can neatly be attributed to the fact that English has a Subject–AUX Inversion rule, but no verb-second constraint.

A complication arises with embedded questions—which, being declarative, should be constrained by the verb-second filter. The facts here are that, when a subject is extracted by WH-Movement, the tensed verb appears in initial position; otherwise, it is in second position. M&Z resolve this problem by allowing the traces of extraction rules (but not NP movement rules) to 'count' for the filter. Thus, even when a subject is extracted, the tensed verb (which follows the subject trace) is in second position.

This article makes several interesting points of broad implication. For one, the analysis indicates that Left Dislocation sentences and Topicalized sentences have different derivations, neither of which involves WH Movement (contrary to Chomsky 1977). For another, it shows that surface filters can be positive, and can deal with properties other than those of the complementizer system—contrary to Chomsky & Lasnik. In fact, M&Z point out that, so long as *unless* conditions are allowed on negative filters, any positive filter can be stated as a negative filter with an *unless* condition (which would be essentially the same as the positive filter). Chomsky & Lasnik indeed claim that filters can be viewed as a type of transformation, where the change is simply 'asterisk' (i.e., mark as ungrammatical). But if positive filters are allowed, we must admit filters into the grammar as a mechanism distinct from transformations.

A third result of M&Z's article is the suggestion (in §2.2) that all insertion rules follow movement rules; this is worth exploration.

One part of M&Z's article which I found less than convincing is their fn. 7 (p. 275): here they point out what look like cases of Topicalization past a WH word in English—which should be impossible, if their analysis of Topicalization extends to English; but they then claim these are really Left Dislocation sentences in which the resumptive pronoun has been deleted. This proposal is disturbing in that I can see no theoretical way to refute it. That is, any time an example arose of Topicalization past a WH word, M&Z could claim it was Left Dislocation with a deleted resumptive pronoun. Furthermore, the deletion of the resumptive pronoun would have to be obligatory (at least in my speech). M&Z claim that these problems arise only when the Left Dislocated element (according to their analysis) is a time adverbial or a locative phrase. They claim further that Left Dislocated elements may be NP's or other phrasal categories, so long as they are pronominalizable by a one-word pro-form. Thus temporal and locative phrases would be possible Left Dislocated elements because the one-word pro-forms *then* and *there* exist. This requirement on Left Dislocation seems highly suspect theoretically, and I think it is empirically wrong. If ex. 1, below, is taken as an example of Left Dislocation with a deleted resumptive pronoun, why shouldn't 2 be analysed in the same way?

(1) In your better moments, where did you like to travel?
Cf. Where did you, in your better moments, like to travel?

(2) As a man-about-town, where did you like to eat?
Cf. Where did you, as a man-about-town, like to eat?

In your better moments has the pro-form *then*; but what is the pro-form corresponding to *as a man-about-town*? And what do we do with sentences like this?

(3) With trout, which kind of wine do you prefer?
(*Cf.* *Happily why does she sing?)

This can certainly be given a Left Dislocation type of reading, rather than an AP Preposed reading (cf. the ungrammatical counterpart). But on the whole, M&Z's article is most convincing.

3. Let me turn now to a group of articles which seem closely interrelated in the problems which they address. In Chomsky 1973, 1976, a tensed S was identified as an Island: nothing could escape from it except by way of movement into COMP (i.e. by WH Movement). Furthermore, nothing could be extracted from a cyclic node with a specified subject. Clearly, then, both these conditions would block the extraction of, say, an object NP from a tensed clause with a specified subject. In 'On binding', Chomsky eliminates this redundancy by the Nominative Island Condition (NIC), which requires that no nominative anaphor can be free in S′, and the Opacity Condition, which requires that no anaphor in the domain of a minimal subject of a node B can be free in B. Chomsky claims that tense governs the nominative case; i.e., nominative case is assigned to the subject of a tensed clause. No fewer than five articles in this volume address questions concerning these two conditions in terms of their proper formulation—particularly, in terms of whether tense is really the crucial factor.

Stephen Harlow, 'Government and relativization in Celtic' (213–54), examines relative clauses and other constructions in literary Welsh. He notes two kinds of relative constructions: those introduced by *a* with a gap, and those introduced by *y*/*yr* with a resumptive pronoun (and no gap). *A* is used when the position of relativization is a subject, or a direct object of a finite clause. *Y*/*yr* is used when the position of relativization is in a possessive NP, or is the object of a preposition. Harlow argues that this distribution of *a* and *y*/*yr* is best understood if we look at resulting surface structures not in terms of whether nominatives are properly bound, but in terms of whether empty NP nodes (whether empty because of deletion, or by movement in the relative clause) are governed, regardless of what role these NP's play in their clauses. Here Harlow is using Chomsky's 1981 Empty Category Principle (ECP)—which, he states, simply provides that all empty NP nodes must be properly governed. Harlow gives approximately the following definition: an NP is properly governed (a) if it is co-indexed with a minimally c-commanding node, or (b) if it is minimally c-commanded by N, V, ADJ, or P. Case (b) is called 'lexical government'. (A node A minimally c-commands a node B if A c-commands B; and no node C exists such that A c-commands C, and C c-commands B, and C does not c-command A.) Harlow takes the position, following Chomsky 1981, that Agreement (AG) may be indexed. Then he goes on to show that Subject–Verb AG in finite clauses allows empty subject NP's (since AG is co-indexed); empty object NP's are allowed only in the presence of a co-indexed clitic (which Harlow claims governs the empty object—apparently assuming the position which Kayne outlines in fn. 2, p. 205, although Harlow does not mention this complication). Empty objects of prepositions are allowed only when the preposition shows an agreement marker (and only some prepositions can do this); and empty possessor NP's which follow the head in a possessive NP are allowed only when the determiner position of the outer NP has an agreeing clitic. Then turning to relative clauses, Harlow claims that Relative Deletion (RD) applies, and is constrained by the ECP. Thus he must analyse relative *a* as a pronoun (which governs the empty NP), but relative *y*/*yr* as a complementizer.

The intricacies of Harlow's complex argument may be intellectually appealing, but the exposition is barely comprehensible, since it is written for readers already completely familiar with the theoretical framework. Thus, given Harlow's definition of government (217), all objects of prepositions and all objects of verbs will always be lexically governed, so they should be deletable in relative clauses, without need for a co-indexed governor. (This does not apply to possessors in possessive NP's, because of Harlow's fn. 9; but this comes a little too late to save the reader from thinking that these possessives are also a problem.) Since objects are NOT always so governed, Harlow is clearly assuming something about lexical government that he isn't telling the reader.

To make matters worse, he uses what turn out to be confusing examples from English, contrasting the following (his exx. 16–17):

(4) There$_i$ seems $_S[\ _{NP_i}[e]$ to be a problem]

(5) *There$_i$ wants $_{S'}[\ _S[\ _{NP_i}[e]$ to be a problem]]

Harlow claims that the empty NP of 4 is lexically governed by *seems,* since S′ is deleted here (following a proposal of Chomsky 1981); but that the empty NP of 5 is not lexically governed (and hence not governed at all) by *wants,* since S′ is not deleted in 5. The problem is that Harlow has not told us that a maximal category is an absolute barrier to government. So the reader is left with many (irrelevant) questions: Is government somehow restricted by a distance factor (perhaps subjacency, in which case both S and S′ would have to be taken as cyclic nodes)? Or is S′ taken to branch to an empty COMP, which thereby c-commands the empty NP in 5 and prevents the matrix verb from minimally c-commanding that NP? It is Harlow's job to tell us.

Another problem appears on p. 228, where he states: 'Relative Deletion is obligatory, provided that its output does not violate the ECP.' This is a sloppy (and potentially misleading) way of saying one of two things: either RD is obligatory, and any resulting sentences that violate the ECP are starred; or else RD is obligatory, but it can apply only to governed NP's, thus assuring that the empty node after deletion will be governed. (I don't consider here the third alternative of a global condition on RD, since I'm sure Harlow does not intend this; but one could misinterpret him in this way as well.) Which does he intend?

Many small questions come to mind as one reads this article. On pp. 228–9, it would follow that the addition of the pronoun *ef* in each example would lead to ungrammaticality—but Harlow doesn't give us the data. Is this the case? Why isn't *ef* in 59a lexically governed by the verb? Not until fn. 13 (p. 234) does Harlow suggest that V is not a governing category in sentences lacking a VP. But even if we accept this change, the problem of ignoring lexical governance by P remains.

I don't wish to leave the reader with the idea that this article is a complete loss. It may well be right, but it is so confusing that a lot of work is required to judge it fairly.

Another article dealing with the ECP is Richard Kayne's 'Binding, quantifiers, clitics and control' (191–211). Kayne begins by examining data concerning leftward movement of *tout* and *rien* in French (data which have been analysed repeatedly in the literature on Romance syntax during the past ten years) in contrast to data on clitic placement. He notes that the former appear to violate the Opacity Condition (i.e., the trace of a leftward-moved *tout/rien* is not bound within the domain of its subject), while the latter do not (i.e., clitics never move outside of the S′ in which they originate). He proposes that the trace left by *tout/rien* does not count as an anaphor for opacity, whereas the trace left by Clitic Placement does. To defend this proposal against accusations of ad-hocness, Kayne claims that *tout/rien* movement involves a 'quantifier-like' element, just as WH Movement does, and that the trace of WH Movement does not count as an anaphor for opacity (as Chomsky suggests in 'On binding'). Thus the generalization would be that traces of quantifier-like elements would never count as anaphors for opacity. The need to treat the trace of WH Movement differently from that of other types of movement rules is discussed elsewhere in this volume, e.g. by Maling & Zaenen. Kayne suggests a rule of variable insertion in the construction of Logical Form which would remove the trace of WH Movement and *tout/rien* movement from being considered an anaphor by opacity. Unfortunately, such a rule could not help explain the difference in trace types noted by Maling & Zaenen, because that difference must be available to their filter. But filters, being on the left branch of the grammar, do not see Logical Form; cf. Figure 1 (overleaf).

Deep Structure

Transformations

S-Structures
(Case assignment)

(Deletion rules,
Stylistic rules,
Phonology)

(Construal, Quantifier
interpretation, etc.)

Surface Structures
(Filters)

Logical Form
(Binding conditions)

FIGURE 1.

Thus the output of any rule which applies on the right branch is not available to the filters, and the difference between the two types of trace cannot be restricted to the right branch. Whatever difference exists must be present before the grammar branches. For many reasons, we cannot simply say that WH Movement leaves no trace. Thus Kayne (following Chomsky) says that binding conditions do not recognize the trace of WH Movement, but Maling & Zaenen claim that their filter recognizes only the trace of WH Movement (and not of other NP movement rules). To reconcile the two positions, we could propose that two types of traces are left by the two movement rules at the time of movement. This is not so ad-hoc a solution as it might seem: WH Movement leaves a gap (in the sense of Chomsky 1977), and the idea that filters should be sensitive to gaps is not strange. However, the quantifier-like nature of the elements affected by both WH Movement and *tout/rien* movement distinguishes them from other types of anaphors, and it seems reasonable that binding conditions should be sensitive only to non-quantifier-like anaphors. In Chomsky 1981, two types of traces for these two types of movement are indeed proposed.

Kayne goes on to discuss further complications. Not only *tout/rien* can be moved, but a *tous* which quantifies a lexical NP subject can move rightward; and a *tous* which quantifies a clitic object (but not a full lexical NP) can move leftward. The rule which moves both *tous* and *tout/rien* leftward he calls L-*Tous*. But now a new problem arises. A rightward-moved *tous* must be c-commanded by the NP it quantifies—a condition typical of anaphors. But a leftward-moved *tous* is not c-commanded by the clitic it quantifies. Thus a leftward-moved *tous* does not behave like an anaphor. Kayne proposed that every floated *tous* has the status of an anaphor, but that *tous* also has the formal status of a quantifier when it can be construed as binding some element. *Tous* could bind an element only if it c-commands that element, and only if that element can be interpreted as a variable. A rightward-moved *tous*, then, is properly interpreted as an anaphor (c-commanded by the subject, which is its 'antecedent'), and not as a quantifier. And a leftward-moved *tous* must be interpreted as a quantifier, which binds the clitic. This analysis has the advantage of being able to account for the fact that rightward-moved *tous* can be associated with a full lexical NP, but leftward-moved *tous* cannot—since a full lexical NP can never be interpreted as a variable, but a pronoun (here, a clitic) can; this is argued independently by Chomsky 1976 and Williams 1977.

In Kayne's by now familiar and inimitable way of tying together a myriad details into a complex, comprehensive whole (and his footnotes in this article are, true to form, the dazzling mass of brilliant asides that we have come to expect of him), he turns back to *tout/rien* and notes that their traces seem sensitive to the NIC. But since he has already argued that these traces should not be sensitive to opacity, he now wishes to say they are not sensitive to the NIC either (i.e., they are not anaphors for binding conditions in general). Instead, the data that look like examples of the NIC are really examples of the ECP. The ECP does not mention anaphors per se, only empty categories; thus it can apply to the trace of L-*tous*, even though that trace is not an anaphor.

Having resolved that issue, Kayne turns to what appears to be a difference between the binding possibilities of WH Movement and those of L-*tous*. He notes that, if *qui* introduces a sentential object in a WH question instead of *que*, the subject of that sentential object can be questioned, even though the ECP blocks the sentence with *que*. But *qui* cannot rescue a violation of the ECP with an L-*tous* sentence. Kayne points out that *qui* allows what looks like a violation of the ECP in still another construction not involving WH Movement. Thus he proposes that *qui* is an indexed form of *que*, and that indexing can be assigned to *qui* only in two configurations: $_{NP}$[NP$_i$ $_{S'}$[$_{COMP}$[*que*] ...]] and $_{COMP}$[NP$_i$ *que*]. The first configuration is that of a head with a relative clause. The second is what would be relevant to WH Movement sentences: a WH word passes from COMP to COMP, leaving a trace each time it moves. That trace can give its index to *que*, resulting in *qui*. But the relevant sentences with L-*tous* have neither of the configurations necessary for indexing assignment; hence *que* cannot become *qui* in these sentences. The point of all this is that an indexed *qui* can now govern the empty subject node in a WH sentence, so the ECP is not violated.

Many questions arise here, mostly dealing with the *que* → *qui* rule. Kayne says that his configurations offer only a necessary factor for the rule, not a sufficient factor; he does not discuss what other factors are needed. But to be convinced by this paper, we must be convinced of the existence of a *que* → *qui* rule; it would have helped to have an extended footnote, giving at least a skeleton of arguments for this rule, plus a formal statement of it (with all the necessary and sufficient conditions).

Let me mention some possible questions concerning *que* → *qui*. First, if *que* can go to *qui* in relative clauses, how do we block the following?

(6) **Voilà l'élève qui j'ai grondé.*
'Here's the student that I scolded.'
Cf. *Voilà l'élève que j'ai grondé.*

Kayne mentions this in his fn. 30, where he refers to Milner 1979 and Taraldsen 1978, neither of which is available to me. Thus the question remains in my mind, at least. Second, and perhaps related, how do we block *que* → *qui* when the head of a relative clause is followed by an S which embeds another S, and the original site of the NP coreferential with the head is found in the subject position of the more deeply embedded S?

(7) **la fille qui je suis sûr qui arrivera la première* 'the girl who I am sure will arrive first'
Cf. ?*la fille que je suis sûr qui arrivera la première.*

(This is from Kayne 1976, fn. 19, which cites Bourciez 1967, p. 743. The question mark indicating marginal acceptability is the judgment reported by Kayne. However, my consultant, Ahmed Fakhri, a native speaker of French, judges this NP to be acceptable.)

Again, how do we block *que* → *qui* in NP's where the N has a sentential complement, such as *la possibilité que S*, but not **la possibilité qui S*? In fn. 24, Kayne says that the exclusion of *qui* here is 'straightforward'; but it isn't to me. Perhaps Kayne is assuming the differing structures offered by Chomsky 1973 (p. 281, fn. 64) for heads with relative clauses and for nouns with sentential complements. If so, a comment to that effect would certainly help the reader.

A final point about Kayne's *que* → *qui* rule is that it must be obligatory when its sufficient conditions (whatever they might be) are met. I would need to see a refutation of a base-generated *qui* analysis before I could feel comfortable working with the *que* → *qui* rule.

Still another question arises for WH sentences: if an indexed *qui* in COMP can govern an empty NP subject, then why can't a trace in COMP, co-indexed with the empty subject, govern that subject? That is, why is the following (K's 23a) bad?

(8) **Qui veux-tu que vienne?*
Qui veux-tu [$_{COMP}$ [t$_i$ *que*] t$_i$ *vienne*]
'Who do you want to come?'

In Kayne's fn. 21, he assumes that the trace in COMP in 8 is 'not a proper antecedent for the subject trace because the non-null *que* prevents it from c-commanding the subject trace.' But why, then, doesn't the presence of the trace in COMP prevent *qui* from c-commanding the subject trace in the following?

(9) *Qui veux-tu qui vienne?*

These questions leave me dissatisfied with an otherwise exceptionally clear article.

Another article dealing with binding conditions is Eric Reuland's 'Empty subjects, case and agreement, and the grammar of Dutch' (159–90). This shows, on the basis of tenseless independent clauses with nominative subjects in Dutch, that nominative case can occur in the absence of tense, so long as subject–verb agreement occurs. A similar phenomenon exists in Latin (and we can add tenseless embedded clauses with agreement and nominative subjects in Portuguese; see Quicoli 1976, esp. p. 135). Reuland proposes that, if any lexical material is dominated by the abstract node Tense (which dominates any non-null tense), then its subject is assigned nominative case. Then he has a 'Move V' rule which moves both tensed and tenseless verbs into Tense; thus subjects will be assigned nominative case, regardless of tense. The problem is that now the PRO subject in a control structure will be assigned nominative case, and should therefore violate the NIC; but such control sentences are acceptable. To remedy this, Reuland proposes that case be assigned only to lexical NP's; thus PRO (and trace) will not receive nominative case, and will not violate the NIC. Then he adds two surface filters: one to ensure that a nominative NP finds a verbal form with which to agree (i.e., nominative is allowed only when subject–verb agreement applies); and another to ensure that a tensed verbal form has a nominative NP as its subject. These filters allow case assignment to be optional. Then the subject of a tenseless clause in English, such as *him* in *I believe him to be a fool*, can optionally be assigned nominative case. If this is done, the sentence **I believe he to be a fool* will be filtered out for lack of subject–verb agreement. If nominative case is not assigned, then *believe* can assign objective case to *him* (according to Chomsky in 'On binding'). This derivation is successful; but if no case assignment applies, the sentence will fail, since all lexical NP's must be assigned case (according to Reuland).

This approach is complicated, but it has some benefits. For one, it allows us to see the fact that tense requires a nominative subject as separate from the fact that a nominative subject requires subject–verb agreement. This is necessary because imperatives are sensitive to the filter that requires subject–verb agreement for a nominative, but not to the filter by which tense requires a nominative subject. However, Reuland never tells us why he assumes imperatives are tensed (presumably it is because Dutch imperatives differ morphologically from non-tensed forms).

In an especially interesting passage (172–4), Reuland looks at Czech, which is among the languages which allow so-called Subject Pronoun Drop (SPD), and he asks why SPD is allowed—given that filters follow deletion, so that a tensed clause with no nominative should be filtered out. He gives several good arguments (further supported by Napoli 1981) against SPD, then opts for base generation of 'SPD' sentences, with the content of the subject slot being supplied by the form of the agreement-marking on the verb itself. This solution has a number of problems. Reuland is forced to claim that Czech simply lacks the filter which requires tense to have a nominative subject; he gives no explanation for this, and makes no correlation between this fact and other differences between Czech and languages with the filter (e.g. Dutch). Second, Reuland has to devise a subject–verb agreement rule which requires checking of agreement if a nominative subject is present (assuming a base-generated agreement; cf. p. 170), but not otherwise. That is, in a 'SPD' sentence, no subject–verb agreement will apply. This makes these sentences distinct from all other tensed clauses, which, as far as I can tell from this article, call for subject–verb agreement in Czech (and certainly in other SPD languages, e.g. Italian). Reuland either doesn't note this fact or doesn't consider it important; but I find it a considerable drawback to his analysis.

On p. 178, Reuland claims that Eng. *that* in COMP can count as a nominative for one of his filters; and he claims (fn. 13) that this is similar to Kayne's *que* → *qui* rule. But the motivations for posing an index on *qui* in French are lacking for *that* in English. So Reuland really owes us more than this remark: it may be reasonable (cf. Bresnan and Grimshaw 1978), but he must convince us of that.

Reuland's discussion of nominative anaphors (p. 184) seems circular to me. Again, his fn. 8 (p. 188) makes the unhappy claim that one might expect 'ad-hoc conditions' to be 'necessary in the periphery of the grammar', as a defense for an ad-hoc condition on one of his filters. In sum, the article is less convincing that it might have been.

Another article dealing with nominatives in tenseless clauses is Luigi Rizzi's 'Nominative marking in Italian infinitives and the Nominative Island Constraint' (129–57). Rizzi examines certain infinitival clauses which are acceptable if their subjects have been moved away by WH Movement (but not by any other NP movement rule), or if their subjects appear as nominatives inverted with the auxiliary, but which otherwise—when lexical subjects remain in subject position—are unacceptable. (An interesting side point is that the class of verbs to which the AUX Preposing rule applies seems, from Rizzi's fn. 5, to be the same class that allows Restructuring. Perhaps another argument for Restructuring could be extracted from such data.)

Rizzi initially accounts for the ungrammatical infinitivals by a filter which stars any S′ consisting of a lexical NP immediately followed by an infinitival verb phrase. Initially, he also uses the NIC to account for the ungrammaticality of infinitivals in which the subject has been affected by NP movement, since the subject trace would be a free nominative anaphor. By contrast, the subject trace of WH Movement would be bound by the trace in COMP (since WH Movement is COMP-to-COMP); thus the NIC would not be violated, and such sentences would be acceptable. (An alternative account (fn. 8, p. 153) claims that the trace of WH Movement should not count as an anaphor for binding conditions—a claim discussed above with regard to Kayne's article.)

At this point (p. 139), Rizzi turns his attention to the case system of Italian, trying to account for the fact that Italian (unlike English) can have nominatives in tenseless clauses. He suggests two basic possibilities: either all surface subjects are nominative in Italian, regardless of tense; or else the Italian nominative is governed by tense (like the English nominative), but also has a subsidiary case-marking process which marks any postverbal subjects as nominative. He opts for the latter solution, since he can then eliminate the surface filter. Any S′ with a lexical NP subject, not governed by tense and not postverbal, will fail to receive case—and since all lexical NP's must receive case, any sentence containing such an S′ will fail. But now Rizzi cannot use the NIC to account for the ungrammaticality of NP-movement trace subjects in these infinitivals. So he says that traces must also be case-marked (contrast Reuland, as discussed above); now these structures will fail since the trace will have no case.

Finally, Rizzi must account for the grammaticality of WH Movement trace subjects in these infinitivals. He claims that, if the trace remains in subject position, the sentence will fail because the subject trace has no case. But AUX Preposing can apply in any of these sentences. If it does, the postverbal subject will be marked nominative. Then, if it is a trace of WH Movement, it will be bound by a trace in COMP. But if it is a trace of NP movement, it will be free and thus violate the NIC. To the objection that Italian is an SPD language—and that SPD can cancel the un-case-marked traces, and can thus save the day—Rizzi responds that SPD should be restricted to apply only to clauses where the verb is inflected, i.e. to finite clauses; thus SPD could never apply to these infinitivals.

If you feel you were led down a garden path in the above, you're right. Rizzi has a clear expository style; but he employs a method of advancing his argument that results in the kind of exasperation so often felt by anyone reading Chomsky's recent works. Certainly, insight can be gained by going through the history of a scholar's solution to a problem—in seeing all the wrong paths he took, and thus understanding the development of his solution. But this gain can easily be offset by the confusion induced each time a reader comes to the end of a path and realizes it leads nowhere. Still, Rizzi's paper is a model of clarity.

Some questions do remain, however. First, in all the infinitivals which Rizzi gives with postverbal subjects, those subjects are pronominal. (This is not true of the gerunds; but they are not the object of study here.) I have found no speakers who accept these postverbal subjects (as Rizzi notes, the construction is 'stylistically highly marked'); so I do not know if a non-pronominal subject would be possible. If not, this is a point worth investigating; i.e., if the postverbal subject must be pronominal, that knowledge might lead to new insights. (Recall that, in French, a floated *tous* requires a pronominal element to bind, which Kayne analyses as a variable.)

Second, Rizzi requires case assignment to apply after AUX Preposing. But such assignment must apply before the grammar branches, so that case information is available both to filters (on the left branch) and to binding conditions (on the right branch). But that would make AUX Preposing a part of core grammar. However, AUX Preposing is not structure-preserving, nor is it an instance of WH Movement; thus it should be a stylistic rule. Furthermore, if the existence of a 'stylistically marked' structure is relevant for determining whether a rule is stylistic, then surely AUX Preposing is a stylistic rule. But such rules apply on the left branch of the grammar, and thus after case-marking. To avoid this paradox, one might propose that case be assigned before AUX Preposing. Then Rizzi would have to take the position that the system for case in Italian is quite different from that in English; specifically, all subjects would have to be assigned nominative case, regardless of the presence of tense. In that instance, he would have to return to his original (garden) path and assume his original filter, with various violations of the NIC in the examples discussed.

Third, control structures are a problem, no matter which path Rizzi takes (i.e., they should be ruled ungrammatical whether PRO is caseless or nominative). Rizzi assumes Chomsky's position (in 'On binding') that PRO is deleted in the course of the left branch of the grammar, so that it is unavailable for filters but available for binding conditions. But if this is correct, we cannot allow PRO to be marked nominative; if we did, the NIC would rule out all control structures. So we cannot take the position that all subjects are marked nominative. But now we face again the problem of making AUX Preposing apply before case assignment—which, I repeat, should be disallowed.

Another problem is buried in Rizzi's fn. 13 (p. 154). Here he points out that WH Movement sentences with infinitivals are not stylistically marked, whereas sentences with AUX Preposing in these infinitivals ARE so marked. But he has claimed in the text that WH Movement sentences involve AUX Preposing. So now he seems to claim that the WH Movement sentences must have additional sources that don't involve AUX Preposing. (Unfortunately, he does not explain what these sources might be, but refers us to Kayne 1980.) I consider the difference in stylistic markedness to be significant in determining the proper structure of the sentences involved. Furthermore, I have found speakers who accept the infinitivals in WH Movement sentences, but not in AUX Preposing sentences; for such speakers, a derivation of the WH sentences by AUX Preposing is highly problematical. Thus Rizzi's own footnote, plus the variety of speech observable among Italian speakers, undermines the argument in his text.

Another problem is that the second paragraph of fn. 14 puts forth the completely ad-hoc proposal that, in gerundival structures, the NP preceding an AUX which is understood to be the subject of the VP to which the AUX belongs is not in subject position at all, but rather in dislocated position. Such a proposal would predict that dislocated elements occurring before the AUX need not be interpreted as subjects of the VP. But this is not possible in Italian, as far as I know. However, fn. 14 also contains a beautiful argument by Guglielmo Cinque which deserves to be in the main text. Cinque points out that, given Rizzi's analysis, we expect that WH Movement of the subject of an infinitival clause will be possible only when the clause satisfies the conditions in which Subject–AUX Inversion can apply. This prediction turns out to be true.

Finally, Rizzi's appeal to analogy, in his appendix, is well done, but ultimately questionable. Here he considers sentences with NP movement of the subject of an infinitival by rules other than WH Movement (all of which we expect to be ungrammatical, given Rizzi's analysis), and he argues that these sentences are good or marginal only because they are similar to other well-formed sentences which have NP followed immediately by a predicative AP (with no verbal form present—as in the Italian equivalent of 'I judge your brother a wretch.') That is, these sentences are accepted by some process of analogy. But there are problems with this. All the speakers I have asked accept at least some of Rizzi's 'analogy' examples (including his 53b,c). Surely, if an analogical process were responsible for the acceptability of these sentences, we would expect at least some speakers to lack that process, and so to reject them. But the judgments I gathered seem to indicate no hesitation whatever with these examples.

The last paper I will discuss is by Leland George and Jaklin Kornfilt, 'Finiteness and boundedness in Turkish' (105–27). This article is less ambitious than the others, and it is difficult to follow if you are unfamiliar with Turkish. Thus ex. 27 (pp. 116–17) took me a long time to figure out; and the comments about specifiers on p. 118 made no sense to me until I read the claim on p. 125 that V (even a main verb) is a specifier in Turkish.

G&K argue that both the Tensed S Condition and the Specified Subject Condition are basically correct, but that they both need modification. Instead of tense, the relevant factor for Turkish is the presence of Agreement; and instead of talking about the subject of a cyclic node, G&K talk about the subject of the maximal projection of X in the $\bar{X}$ (= X′) theory (where X is any major category, I presume). But since their examples involve nothing but the subjects of sentences and NP's, I fail to see why they didn't stick to Chomsky's Specified Subject Condition. The arguments and data are complicated, but the over-all point is straightforward (I've stated it). One final remark: G&K suggest that 'the verbal specifier furthest from the main verb is the key to the definition of "finiteness"' (125)—which seems a very promising suggestion.

The other article in the book is Chomsky's 'On binding' (47–103), about which a monograph could be written. But since Heny discusses it at length, and since so many works (in this volume and elsewhere) discuss it, I will abstain from making further comments here.

In conclusion, the book is a maze: in reading it, I often got tired, exasperated and lost. But it is definitely a serious contribution to the discussion of filters and binding; and it is packed with important arguments and fascinating data. I suggest that all syntacticians buckle down and read it.

REFERENCES

BOURCIEZ, E. E. J. 1967. Éléments de linguistique romane. 5ème ed. Paris: Klincksieck.

BRESNAN, JOAN, and JANE GRIMSHAW. 1978. The syntax of free relatives in English. LI 9.331–91.

CHOMSKY, NOAM. 1970. Remarks on nominalization. Readings in English transformational grammar, ed. by Roderick Jacobs & Peter Rosenbaum, 184–221. Waltham, MA: Ginn.

——. 1971. Deep structure, surface structure, and semantic interpretation. Semantics, ed. by Danny Steinberg & Leon Jakobovits, 183–216. Cambridge: University Press.

——. 1973. Conditions on transformations. A Festschrift for Morris Halle, ed. by Stephen R. Anderson & Paul Kiparsky, 232–86. New York: Holt, Rinehart & Winston.

——. 1975. Reflections on language. New York: Pantheon.

——. 1976. Conditions on rules of grammar. Linguistic Analysis 2.303–51.

——. 1977. On WH Movement. Formal syntax, ed. by Peter Culicover et al., 71–132. New York: Academic Press.

——. 1980. On binding. LI 11.1–46.

——. 1981. Lectures on government and binding. Dordrecht: Foris.

——, and HOWARD LASNIK. 1977. Filters and control. LI 8.425–504.

KAYNE, RICHARD. 1976. French relative *que*. Current studies in Romance linguistics, ed. by Marta Luján & Fritz Hensey, 255–99. Washington, DC: Georgetown University Press.

——. 1980. Extensions of binding and case marking. LI 11.75–96.

MILNER, JEAN-CLAUDE. 1979. La redondance fonctionnelle. Lingvisticae Investigationes 3.87–145.

NAPOLI, DONNA JO. 1981. Subject pronouns: The pronominal system of Italian vs. French. CLS 17.249–76.

PIKE, KENNETH. 1949. A problem in morphology–syntax division. Acta Linguistica Hafniensia 5.125–36.

QUICOLI, CARLOS. 1976. Missing subjects in Portuguese. Current studies in Romance linguistics, ed. by Marta Luján & Fritz Hensey, 100–43. Washington, DC: Georgetown University Press.

TARALDSEN, KNUT T. 1978. On the NIC, vacuous application, and the *That*-Trace Filter. MS (so cited by Kayne in the book under review).

WASOW, THOMAS. 1977. Transformations and the lexicon. Formal syntax, ed. by Peter Culicover et al., 327–60. New York: Academic Press.

WILLIAMS, EDWIN. 1977. Discourse and logical form. LI 8.101–39.

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