CONTRERAS, HELES, and JÜRGEN KLAUSENBURGER, eds. *Proceedings of the Tenth Annniversary Symposium on Romance Linguistics = Papers in Romance*, Suppl. II, Vol. 3 (Dec. 1981). Seattle: Univ. of Washington. Pp. 290

This volume includes 25 papers given at the Tenth Annual Linguistic Symposium on Romance Languages (LSRL), which was held at the University of Washington, Seattle, on March 27–29, 1980. Of these papers nine (if we include Malkiel's keynote article) concern phonology and/or morphology, and the remaining bear on syntax and/or semantics. While the number of languages examined is impressive (Latin, French, Italian, Portuguese, Romansch, Spanish, as well as German, Russian, and Greek), a full eleven of the articles are exclusively on French and six exclusively on Spanish. Portuguese, Romansch, and Latin each claim only one. Italian is covered exclusively in one and in contrast to Spanish in another. And the three remaining pieces involve several languages each.

The volume is divided into two sections. Let me begin with the PHONOLOGY/MORPHOLOGY section first, which makes up one third of the entire text. This section opens with an article by **Jürgen Klausenburger**, one of the two coördinators of the conference, offering an overview of Romance phonological studies in the '70's, i.e., in the decade in which LSRL became a tradition. This article gives a nice, brief history of phonological theory in that decade followed by a discussion of problems that have occupied Romance scholars in LSRL volumes as well as elsewhere. K. does not hesitate to supply his own critiques of the writings he outlines, and I, as someone who rarely works on phonology/morphology, found these critiques helpful. In his discussion of work on Romance languages other than French and Spanish, he leaves out some interesting work on Italian, such as Nespor and Vogel 1979. But on the whole, this article is comprehensive and lucid.

The items on problems in phonology/morphology that follow are difficult for me to evaluate fairly, so I will for the most part merely describe them briefly or simply remark on some aspect of them that seems particularly interesting.

**William Cressey**, a reliable contributor to LSRL since its early years, offers a proposal for the lexical component of a generative grammar which contains

Here is a brief explanation of the abbreviations used in this paper in the order in which they appear in the text: P = preposition; GEND = gender; PL = plural; III = third person; II = second person; DET = determiner; NP = noun phrase; N = noun; AUX = auxiliary; VP = verb phrase; BLS = Berkeley Linguistics Society; CLS = Chicago Linguistics Society; V = verb; S = sentence; Obj-to-Subj-Raising = Object to Subject Raising; PRO = a phonetically null NP (with certain syntactic characteristics irrelevant here); AP = adjective or adverb phrase; PP = prepositional phrase; QP = quantifier phrase; COMP = complementizer; Equi = Equivalent NP Deletion; I.O. = indirect object; Q.M = quantifier movement; CC = clitic climbing; D.O. = direct object; $\bar{S}$ = the first node dominating an unconjoined S node; $\bar{V}$ = the first node dominating an unconjoined V (i.e., the node that dominates V and its complements); $\bar{\bar{V}}$ = the first node dominating an unconjoined $\bar{V}$ (i.e., the node that dominates $\bar{V}$ and its specifier).

two units: a lexicon and a set of lexical rules which state how the lexical items combine with each other. C.'s major interest is in the structure of the lexicon, which he says consists of a main entry (morpheme or root) and several subentries (stems). The hierarchical organization he outlines is intended to allow some aspects of the meaning of a composite form to be derived from the meanings of its parts and others to be associated directly with the form itself. The data presented are from Spanish. This article reads like a grant proposal; i.e., it reminds one of a discussion of work about to begin rather than a report on a study completed. I wonder if C. has since further developed this approach to the lexicon or not.

**Jorge Guitart** next discusses liquid gliding in Cibaeño Domenican Spanish. His description of the environments in which gliding occurs is very intriguing: It appears that gliding, like *liaison* in French and *raddoppiamento* in Italian, can operate across word boundaries (although it is also conditioned word-internally). Let me reproduce G.'s first description of the data (223, with a typographical error corrected and examples omitted):

> Liquid gliding occurs when the liquid is after a vowel other than the high front vowel /i/ and is (a) in word-internal position before a consonant; (b) at the end of an oxytonic word, regardless of what follows; (c) at the end of an unstressed monosyllable if followed by a consonant.

G. gives evidence to conclude that, instead of this first approximation above, the correct environment for liquid gliding is the disjunction "before a consonant or a word boundary" (227) with no reference to stress placement. This conclusion rests squarely on the assumption that articles and prepositions—which would fall under case (c), above—are not "words" but perhaps "clitics" (loc. cit.); thus they will not undergo gliding before a vowel. I find G. quite convincing in his arguments against stress as a factor and in stating that case (c) above is somehow to be viewed as a special instance of case (a). But I wonder if the kind of syntactically-based approach that has proved so insightful for *liaison* and *raddoppiamento* might not help to explain case (c) better than resorting to the claim that articles and prepositions are "not words." If specifiers allow exactly one sister and prepositions do the same, we might try to appeal to this syntactic configuration fact somehow to explain case (c). (Note that in Italian P's may well allow more than one sister; in Spanish I simply do not know the relevant facts.)

**Julia Herschensohn**, in discussing syntactic features and French morphology, argues that her Feature Matrix Hypothesis, which uses ten syntactic features (Gender, Feminine, etc.), is a superior way to account for the lexical choice of pronouns in French to Halle's (1973, cited by H.) Lexical Insertion Hypothesis, which requires lexical insertion of all pronoun-determiners, with appropriate surface filters. She makes her case well; but I have a number of questions about details of her presentation which may or may not be disturbing, depending upon how she might answer them. First, H. talks about words



being "syntactic phrase-final" (229). Then one of her examples is *elle* in *C'est elle que je vois*. How is *elle* syntactic phrase-final? I fail to understand either her unstated definition of syntactic phrase-final, or what syntactic structure she is assigning to this sentence. Does not *elle* form a constituent with the following *que* phrase? Since the notion of syntactic phrase-final is crucial to her hypothesis (230, [7]), an explicit definition would have been helpful. Next, some of H.'s feature matrices confuse me. For example, she gives the matrix (231, [9]),

$$\begin{bmatrix} +\text{PERS} \\ -\text{GEND} \\ -\text{PL} \\ -\text{III} \\ -\text{II} \end{bmatrix}$$

where PERS stands for "Personal," which "can cliticize to verb," and where the other symbols are easily recognizable. Why does she include – GEND? Once we realize the form is first person (i.e., [ – III] and [ – II]), we automatically know it is not morphologically marked for gender. Since H. does not give the full ten features in every feature bundle, she must have some reason for leaving some out. Why not omit those features already uniquely determined by the combination of other features? I also had trouble understanding the relationship of her diagram in (11) with her explanation of the features in (10). Finally, I wonder why she uses symbols like $\alpha$, $\beta$, $\gamma$ on binary features (232, [13]). Do not these symbols indicate a range of more than two values? Despite these questions, the article is persuasive. One particularly interesting point: H. claims that pronouns are simply feature matrices under the DET node in an NP whose head N is empty. This exceptionally insightful proposal is the analogue of the claim that auxiliaries which stand alone are pro-predicates, since AUX is the specifier of the VP (just as DET is the specifier of the NP). Such a claim is independently supported by Schachter 1978, Pullum 1981, and Johnson 1982.

The remaining four articles in this section involve dephonologization in Romansch by JEAN-PIERRE MONTREUIL, Italian *raddoppiamento* by EUGENE PAPA, French *liaison* by BERNARD TRANEL, and the morphological component (particularly word-formation rules), with French and English examples, by WIECHER ZWANENBURG. I group these four together for considerations of space and because I have little to say about each.

The article by **Papa** is rather difficult to follow because of both the writing style and the organization. He reports data different from those reported by Vogel 1977, 1978, and mentions that Camilli 1965 and "other handbooks" (255) fail to include data like Vogel's. But in defense of Vogel let me point out that whether or not these other handbooks contain data like Vogel's does not call into question the existence of such data. Many of the well-respected handbooks report only Tuscan data, and then only those of certain geographical areas in Tuscany and of certain social classes. As someone who has worked extensively on *raddoppiamento*, I find the fact that Vogel's data would fall outside those stated by Camilli, e.g., no surprise. Furthermore, some of *P.*'s comments on the relevant data need elaboration so as not to mislead. Thus, one reads: "phrase-initial and isolated words never undergo raddoppiamento" (249). While I believe this is true, there is an emphatic initial consonant lengthening in Italian (as described in Napoli and Nespor 1979) which can apply to phrase-initial and isolated words in at least some varieties of Italian and whose phonological effect is identical to that of *raddoppiamento*. The novice to *raddoppiamento* studies needs this fact pointed out to be able to distinguish true *raddoppiamento* cases from emphatic lengthening cases. Notice also that P. reports *raddoppiamento* after the nom. pron. *tu*. Of all the nominative pronouns, *tu* is the only one ending in a stressed vowel; thus it is the only one endowed with the proper phonological environment to trigger *raddoppiamento* on the following word. Reporting *tu* in isolation rather than talking about subjects in general (which do, in fact, trigger *raddoppiamento*) can leave the naïve reader with the false impression that *tu* is somehow unique among subjects. Clearly, these are problems P. did not consider, since he concentrates on the phonology of the phenomenon alone. However, the potential for confusion is great.

**Tranel**'s paper is interesting because it makes a case for an insertion treatment rather than the heavily-favored consonant-deletion treatment for *liaison*. One of T.'s concluding arguments is that a grammar which would include the deletion treatment is not learnable. While the argument is elaborate, I feel ill-equipped to evaluate it. Still, the comparison to Maori which he supplies is thought-provoking.

The SYNTAX/SEMANTICS section of the volume covers a wide range of topics. **Heles Contreras**, the other coördinator of the conference, opens this section with an overview of Romance syntax/semantics studies in the decade preceding the conference. C. seems far more timid—or perhaps polite—than K. in his preceding overview of Romance phonology/morphology; regrettably so. He limits his discussion to papers which actually appeared in the LSRL proceedings and merely describes them without engaging in criticism. An arresting point of C.'s is that LSRL started out with a strong interest in generative grammar which waned briefly in the mid '70's but returned ever stronger in recent years. Despite this fact, most papers given in the series refrain from challenging existing theories—and thus, we read, from contributing to advancement in theory. I think C.'s point is well taken. In general Romance linguists eager to challenge theories have turned to the journals to do this. The reason seems obvious: In the wide readership of journals a paper with theoretical relevance stands a better chance of being noticed by others working on advancement in theory and, hence, of actually contributing to that advancement. Unfortunately, conference proceedings have nowhere near the wide audience of journals, and, thus, symposia like LSRL, BLS, and even CLS (see Newmeyer's 1980 figures on submissions to CLS) cannot compete for such theoretically-oriented papers. A strategy future LSRL organizers might weigh is contacting a major journal before a conference and arranging for the journal to publish the five to ten best papers of the conference in one issue. With such a guarantee, I would expect Romance linguists of all bents to submit abstracts with the end result of a more exciting conference.



Let me now pass on to brief comments on the articles themselves. **Joëlle Bailard** furnishes French data to propose a system of redundancy rules using passive and the causative construction as examples. While the proposal is interesting, it fails to address some problems that come to mind immediately. For one, no mention is made of how to resolve the subcategorization problems encountered in a base analysis of causatives. Why can an intransitive verb take an NP following it only when it is preceded by a causative, for example? Do we need a system of "subcategorization redundancy rules"? And how can the placement of reflexive clitics in French be accounted for with a base-generated analysis of causatives? These problems, hardly insurmountable, seem significant enough to warrant attention before we can take B.'s proposal and run with it.

**Susanne Carroll** next argues that French dislocations are parenthetical in nature. One immediate problem is that she lumps together so-called left dislocation (LD) with so-called right dislocation (RD), ignoring the very real functional difference between them (see Sornicola 1981 for a nice discussion of the difference in the Italian constructions, a difference which holds for French as well). The comments she makes may well hold true for RD, but certainly seem to me incorrect for LD, which, instead, appears to be a topic. In her examples (8)–(10), she omits from her data sentences with *quant à Jean* in final position. Are there no such grammatical sentences? If so, she should have told us that. One intriguing point is that, while *as for John* can appear in S-initial and not in S-medial or S-final position in English, *according to John* has the complementary distribution; i.e., parentheticals (medial) pattern with RD, not with LD in English. The French interpretations of *quant à Jean* in various positions support the proposal that LD is distinct from parentheticals, contrary to C. Finally, she makes a comment about the truth value of (6c) which she finds important. Yet the same truth conditions hold for (6a) and (6b), which she apparently contrasts with (6c) on this point. This comment and others like it baffle me.

**Jeffrey Chamberlain** and **Mario Saltarelli**'s diachronic analysis of Romance causatives is hard for me to follow, largely, I think, because their presentation of the analyses they are refuting is so brief that, without prior acquaintance with these analyses, one is hard put to understand. I have some rather detailed questions on their article. How is a perception verb a "three-place verb" (65)? How is case assigned when a V is extracted resulting in the VP pruning (as in 69, [17])? What evidence do they have that S is pruned when its NP subject is extracted? All the evidence I know of points to no pruning of S in Raising structures (*John seems to me to enjoy me/*myself*; *John wants me to like him/*himself*). In fact, they themselves pose problems for this idea in their *n*4. And why do they list Obj-to-Subj-Raising as an example in (17)? Here the subject of no s is being extracted, so the relevance of this rule eludes me. Their proposal (70, bottom) calls to mind Roberts' 1979 similar proposal involving pruning, yet they point out neither their similarities to, nor their differences from, his proposal.

**Benoît de Cornulier**'s article on yes-no and alternative questions in French embodies a convincing if somewhat poorly-written defense of the proposal that yes-no questions are not derived from alternative questions. The proposed method of interpreting alternative questions, however, encounters certain problems. He argues that each tacked on alternative is to be understood as something like "If not the previous proposal, then how about the next proposal?" While this seems to capture the usual alternative question, like *Is John coming or Mary?*, it leaves us with a wrong interpretation of *Is John or Mary coming*? Perhaps de C. would argue that the latter is a yes-no question only; in French it possibly is. However, in English it is most satisfactorily answered by *John is(n't)*, *Mary is(n't)*, or *Both are/(Neither is)*, but not simply by "yes" with no further information (although "no" does seem satisfying as an answer).

**Anne-Marie di Sciullo** follows using French data to argue that optional subcategorization is the unmarked case. She shows that it is impossible to attribute subcategorizing complements to the syntactic properties of verbs alone (88, [18]). Still, I remain confused by her proposal in a few ways. For one she claims that a subcategorization frame such as *eat:* +V[_(NP)] (87, [14]) is equivalent to the proposal of a lexically-governed rule of Object Deletion. How? One says the optionality is in the base; the other finds it in the transformational component. If she means that both proposals are vulnerable to the same criticisms, then she needs to show us how. But if she truly means they are equivalent, I simply do not understand her. A perhaps related problem is the interpretation of her solution. She seems to be saying (89) that whenever an NP object is phonetically absent with a verb like *eat*, we have PRO as the object. But this proposal has no better luck explaining her examples (under 17) than the Object Deletion hypothesis, unless we allow the ad hoc claim that PRO is interpreted differently from all other NP's. Such a claim is, unfortunately, untestable—thus a proposal based on this claim would be equally untestable from a semantic point of view and, consequently, be a nonproposal.

**Joseph Emonds**' paper on French and English finite verb-formation is difficult to understand unless one is familiar with the other works by Emonds cited here. One arresting point is E.'s proposal about which kinds of rules should be obligatory in linguistic theory. His (16) is appealing, particularly if reflexive clitics can rank as "inflectional morphemes" (although he proposes [16] strictly based on verbal paradigms).

**Paloma García-Bellido** has written on the nominalization of relative clauses in Spanish. She argues that relativized-measure phrases together with their heads form an NP, but their heads are not NP's—instead, they are AP's, VP's, PP's, or QP's. The paper makes a good case, but some sections are confused. Thus, when trying to argue for a structure in which the *lo* introducing the head of these measure phrases forms a constituent with the head (as in [27]) and against one in which both the *lo* and the measure phrase are sisters to the head (as in [26]), the author brings up four types of examples. Her (28a), however, is the only relevant example. (28d) will be precluded by both (26) and (27). And (28b) and (28c) can both be accounted for with either (26) or (27).



**Paul Hirschbühler**'s article on pronominal subject inversion in French suggests that the contrast between direct and indirect questions with regard to inversion should be explained along semantic rather than syntactic lines. In particular, the facts on subject inversion do not lend themselves to a root vs. non-root treatment. The article is very interesting and the data are fascinating, and quite different from English. Thus the idea that English Subject-Auxiliary Inversion is a root phenomenon can be reconciled with acceptance of H.'s totally distinct analysis of the French phenomenon.

**Alfredo Hurtado**'s article on the structure of the complementizer in Spanish is poorly written and for that reason quite hard for me to understand. Thus, when he talks about the "COMP generated by the base" (124), does he mean the COMP filled by lexical insertion (as opposed to transformationally-filled), given that his underlying structure in (2) has all the COMP nodes present in the base? Elsewhere he talks about *que*'s being "interpreted as the complementizer" or not in the constructions he examines (125). But what does he mean by being "interpreted as a complementizer"? The last sentence of the paragraph entitled "The nature of Wh-movement" is incomprehensible (127). Since this sentence is meant to provide his conclusion to that point, the whole argument is lost on me. The final paragraph on p. 129 is equally incomprehensible. H. does make clear some shortcomings of a free-deletion rule in COMP. But I find myself unable to evaluate the details of his own proposal because of flaws in his exposition.

The only article on Portuguese here is concerned with the agreeing and nonagreeing infinitives in part, a phenomenon by now familiar to Romance scholars. **Larry King** points out that inflected infinitives have the same distribution as uninflected infinitives with impersonal *one* subject. What he does not state is that this correlation is exactly as expected. That is, infinitives of any type with uncontrolled subjects have a unitary distribution. K.'s major concern is what motivates the choice in actual usage between inflected infinitives, uninflected infinitives, and embedded tensed verbs where infinitivals are also admissible. He concludes that the tendency is to use a tenseless form whenever the tense of the infinitival is dependent on some other temporal referent in the sentence. Furthermore, there is a tendency to opt for the uninflected infinitive; but K. does not, so far as I can see, attempt to explain why. One problem in the article is that K. gives some examples of Equi with an object controller in (33), taking them for infinitivals with uncontrolled subjects. This makes one question whether he has accurately represented the data to us in general.

**Monique Monville-Burston** offers a semantic analysis of French tonic dative pronouns. Her analysis might well be correct, but she does not argue it in a tight enough way to convince. Thus, she claims that an I.O. participating minimally in the verbal action appears as a tonic rather than clitic pronoun. In support she supplies examples of verbs that allow clitic I.O's and others that allow tonic I.O's. But the proposal seems little more than one of many hypotheses one could speculate upon given those data in the absence of some kind of test. M.-B. offers none. Yet a test is possible, I think. Consider the sentence *I didn't know he was deaf, so I spoke to him.* Here *him* exhibits the minimal participation that M.-B. leads us to expect a tonic form to show. How would this sentence be rendered in French—with . . . *j'ai parlé à lui* or . . . *je lui ai parlé*? If the former, this is nice evidence for her. If the latter, M.-B. needs to find an explanation, otherwise her hypothesis looks untenable. Beyond the fact that no tests of the above sort are offered, the article suffers from a lack of precision. Take p. 151: Regarding (15)–(16), she simply does not provide enough examples to let us guess whether or not her proposals are mere conjecture. In the last paragraph she relies on a notion of "outside the verbal process" left undefined. The proposal regarding (17) seems interesting until one notes that the act of introducing has the same semantic properties, yet dative clitics are allowed. On the whole this article presents an as yet undeveloped idea.

**Anne Rochette** writes about quantifier movement and infinitival complements in French, arguing that the possibility of QM is syntactically rather than semantically determined (contra Pollock 1978). In particular, she argues that QM is not allowed out of infinitivals introduced by a preposition but is allowed out of those introduced by complementizers. She makes her point very well with nice contrasts like those in (7)–(9), using clefting, pied-piping of P's in question formation, and the type of clitic that corresponds to an infinitival complement (i.e., *y* or *le*) as tests for whether or not a complement is introduced by a P. One major residual question, however, is why PREPOSITIONAL complements disallow QM but others do not. R. makes no attempt to answer this. Thus her proposal is descriptive rather than explanatory. One final point: the data on Italian infinitival complements that allow clitic climbing do not fall into a one-to-one correspondence with the data R. offers on French infinitival complements that allow QM. Thus the analogous claim for Italian (that CC is impossible out of a prepositional infinitival complement, but allowed elsewhere) cannot be made.

**Carmen Silva-Corvalán**'s article on object-verb agreement in Spanish examines the conditions under which an accusative clitic may co-occur with a coreferential D.O. in Chilean and Argentinian Spanish. She considers instances where the D.O. is preverbal and others where it is postverbal, concluding that the degree of "topicality" of the D.O. is the relevant factor. When the D.O. is more topical (i.e., more specific as determined by a combination of the two features of having a determiner and being morphologically definite), the D.O. is more likely to appear with an accusative clitic. The author concludes that this phenomenon is a manifestation of object-verb agreement.

In one of the bolder articles here **Judith Strozer** proposes an analysis of so-called clitic-climbing sentences in Italian and Spanish which do not involve restructuring. She claims that CC structures always involve embedded (orphan) VP's, whereas the phenomenon known as CC never occurs with embedded S complements. S. offers no independent evidence for the claim that the relevant V's embed VP or S complements. She then goes on to attack Rizzi's 1978 arguments for restructuring. She argues that R.'s tests for different constituency of paired S's where one has a climbed clitic and the other does not are faulty. Against his pied-piping test, she contends that the ungrammatical S can be excluded because the trace of the clitic is not properly bound (181); thus no evidence for constituency emerges. However, French subject clitics with the same binding problem are grammatical. Thus, S.'s argument may be not so convincing, since Romance offers evidence that the constraints on proper trace-binding need to be further studied. Second, she argues against R.'s Right Node Raising test, maintaining that the relevant S's could be out because "there is only one trace for two different clitics" (182). S. offers no independent evidence for this claim about proper trace-binding, and I am unaware of such a claim or evidence for it in the literature. It appears to be an ad hoc claim of S.'s alone, a circumstance which certainly does not make it necessarily wrong; only it is in need of support. Third, S. argues against R.'s complex NP shift test saying the relevant S's are out because "some kind of interference that makes it difficult to relate the clitic to its NP position" is "involved" (182). Again, no independent evidence is advanced, nor do I know of any in the literature. These three examples typify the problematic nature of this paper. S. offers us one ad hoc claim after another, expecting us to be convinced that her proposal is superior to an admittedly complicated but at least motivated rule of restructuring. She also raises questions with the implication that answers to them are not readily forthcoming, when, in fact, the answers are sometimes obvious. She asks, e.g., why her (21), with a plural verb, can be interpreted as a reflexive S or an impersonal *si* S, while her (22) is interpretable only as an impersonal *si* S.

(1) (= Strozer's [21]) Quelle persone si vorrebbero licenziare su due piedi.
(2) (= Strozer's [22]) Si vorrebbe licenziare su due piedi quelle persone.

But the sg. verb of her (22) excludes a refl. *si* reading, since if it were a reflexive the subject would be pl. (*quelle persone*) and subject-verb agreement would have failed. For reasons such as these, I find S.'s proposal unconvincing. A well worked out base-generated analysis of this phenomenon would be very welcome, but S. has not offered us any.

**Karen Zagona**, in the concluding article of this section, submits a proposal similar to Strozer's for the same phenomenon, claiming that the difference is VP vs. $\bar{S}$ complements (whereas S. offered VP vs. S). Unlike S., Z. treats causative constructions as well. Z. goes into details of her proposal with examples of bracketing. Many points leave me confused, although on the whole I find the proposal stimulating. Here is an illustration. First, Z. claims Spanish impers. *se* S's cannot be embedded under causatives because causatives take VP not $\bar{S}$ complements. Her evidence is furnished in (14)–(15). But the same sentences without *se* are grammatical and with the intended impersonal *se* reading, I think; at least their Italian counterparts are. Thus Z. will have to admit some way of allowing an orphan VP an impers. *se* subject reading. Second, Z. says adverbs and negatives cannot come in the middle of a CC verb string because such a string is a base generated $\bar{V}$, and adverbs and negatives cannot appear in medial position of $\bar{V}$ (p. 188). But she offers no independent evidence for this claim. Certainly, in Italian adverbs can come in medial position in a $\bar{\bar{V}}$: between auxiliary and main verb, and between main verb and following complements (NP, PP, AP). I am not sure why Z. talks of $\bar{V}$ (that is, V and its complements) rather than $\bar{\bar{V}}$, unless it is precisely to get at the string V VP. But, again, she supplies no examples of V VP that are not the focus of her analysis; thus no independent evidence is forthcoming. Finally, she argues that selectional restrictions operate within an S, so selectional restrictions cannot be appealed to when ruling out **Hice ser inteligente a Juan*, unless *hacer* takes a VP rather than an $\bar{S}$ complement. But the restriction here is pragmatic, not grammatical. Witness the fact that the above Spanish sentence becomes acceptable when embedded under a matrix like the English "It is ridiculous to believe that . . .". In sum, while the approach is worthy of investigation, it is not yet clear that it offers obvious advantages over a transformational approach.

The very first article of the entire volume, that by **Yakov Malkiel** on the Romance numerals "one" through "ten," remains to be discussed. M.'s paper ties together the numerals with kinship terms across Romance languages as well as other languages (Greek, Russian, German). His presentation of the previous literature on the matter is comprehensive and speckled with his own critiques. Only quite late in the article does he reveal to us that, in his view, the principal ancestor of *tâtar(a)-* was "a word meaning 'three' or 'third' " (17). The revelation comes as something of a surprise, given his own doubtful remarks earlier in the paper about others' similar speculations. But he makes what to me—an outsider in the domain—seems an interesting case. And his etymologies (particularly of words like It. *barlume*) are fascinating. Most intriguing of all, I must admit, is M.'s eloquence. The entire rest of the volume pales in comparison with M.'s masterful exposition.

On the whole this volume seems very much like past LSRL proceedings. Many seeds of interesting ideas are buried within, but few have yet germinated. Perhaps any conference of papers accepted on abstracts alone must suffer in this way. For we all know that promising a finished piece of research by a given date is an illusion. Yet the volume is thought-provoking. And the Romance data are as alluring as ever.

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