

## Book reviews

Valerie Adams: *Complex Words in English*. Harlow: Person Education Limited, 2001. 173 pp.

They say that you cannot step into the same river twice. Adams's latest book is such an attempt and — in my view — a worthwhile one. Twenty-eight years after her first book on word formation, Adams presents a profound, comprehensive, and mature account of the ways complex words are coined in English. While, as she notes in the Preface, this book “was first planned as a second edition of *An Introduction to Modern English Word-formation* ... nothing of that work has remained.” The advances in the research into word formation have been dramatic since the former publication, and they could not but alter the new book.

While there are many references to the latest theoretical works in Adams's *Complex Words*, it is first of all a practical manual. Brief discussions of theoretical questions in the individual chapters primarily serve more practical ends: a review of the whole variety of ways employed by present-day English speakers in coining new complex words. Moreover, while the essential goal is synchronic description of word-formation trends, their account is systematically set in a broader historical framework, which results in an intriguing synchronic-diachronic overview, perhaps the most comprehensive one since Marchand's *Categories* (1969) and Bauer's *English Word-Formation* (1983). Still, there is one more point that makes Adams's work highly valuable: each word-formation process and type is illustrated by copious examples, frequently contextualized, with many of them of recent origin — all this thanks to the use of the OED on CD ROM.

The book is divided into twelve chapters. While the first chapter introduces some of the essential notions of word formation, the following chapters discuss, in remarkable detail, the individual, more or less productive, word-formation processes in English, including transposition, prefixation, suffixation, formations with particles, and compounds.

Considerable space is devoted to borderline cases like stem formations, phonaesthemes, and reanalysis.

Since the book's main goal is mapping the trends in English word formation and illustrating them with ample examples, theoretical issues, as indicated above, are reduced to the necessary minimum. While this is understandable and justified by the objectives of the book, some theoretical positions are — consequently — presented, as it were, axiomatically. By implication, there are some issues in Adams's account that raise a question of an alternative approach. Therefore, in the following comments, without diminishing the significance of Adams's book, I would like to briefly discuss some of the moot points and offer a different way of treating them.

Adams obviously relates word formation to the existence of semantically parallel adverbs or prepositions (*post-war* : *after war*) for prefixes, and free lexemes (*-ful* : *full*, *-like* : *like*, etc.) for suffixes. This seems to be an awkward justification because affixes as form–meaning complexes have their meaning independently of the corresponding “free morphemes.” Moreover, it should be emphasized, word-formation rules of suffixation in principle combine two signs: a word-formation base with a meaningful affix (an exception to this approach is, for example, Beard's lexeme–morpheme base theory [1995], which basically rids affixes of their meaning; and, on the opposite pole, for example, Halle's [1973] treatment of morpheme as a unit that need not carry any meaning). By implication, each affix has its distinctive meaning, for example, that of agent (*-er*, *-ist*, *-ant*), instrument (*-er*), abstract process/result (*-ment*, *-ation*), action (*-ize*, *-ify*, *-en*), etc. This conclusion also follows, for example, from Lieber's (1981, 1992) treatment of affixes on a par with other entries in the lexicon, and Plag's discussion (1999) in which various affixes are assigned their specific meanings (or, lexical conceptual structure).

Considerable space is reserved for the discussion of the complex word–phrase distinction, one of the hard nuts of English word formation. Among other things, Adams maintains that while the hyphenated strings like *not-in-my-backyardism*, *morning-afterish*, *what-have-we-got-to-lose-ness*, etc., behave like words, they are not complex words, because “[a]ccording to the no-phrase constraint ..., complex words are not formed from phrases” (p. 3). This view seems to be questionable. These formations are, no doubt, products of a combination of a phrase and a suffix. The suffix determines their word-class category and thus functions as “determinatum” (head). Given numerous examples of this type of formation, the no-phrase constraint appears to be too strong.

Rather surprisingly, in spite of the existence of abundant counter-evidence to Williams's *right-hand head rule* provided by a number of

morphologists (Lieber 1981, 1992; Selkirk 1982; Anderson 1992; Bauer 1988, 1990; Williams in Di Sciullo and Williams [1987] himself modified his original approach by the notion of relativized head), Adams heavily relies on the right-hand head identification principle in distinguishing between complex words and phrases. Thus, in her view, “[a]ny expression which we can see as not right-headed will be distinctive or untypical in some way, or will have the character of a phrase.” *Outswim*, for example, is used by Adams to exemplify the change of an intransitive verb to transitive by prefixation. Apparently, the transitive characteristic is percolated to the complex word from the prefix. In Adams’s view, however, *swim* is the head because it is conjugated irregularly as *swim* itself. Here, one might ask whether the category of conjugation is more significant than that of transitivity, that is, whether there is any hierarchy of morphosyntactic characteristics with the topmost one determining the headedness.

It is also not quite clear why words like *anti-bacterial* are considered to be parasynthetic formations (p. 4), that is, words coined by joint operation of prefix and suffix. This runs counter to the general binary-structure-based approach in generative word formation: (*anti-(bacterium<sub>N</sub> + al<sub>A</sub>)<sub>A</sub>*)<sub>A</sub>. Moreover, the claim that, for example, *on-line* in *on-line editing* is a phrase (unlike *inter-city* in *inter-city train*) because the latter contains a “preposition-like prefix” does not appear to be well justified. No doubt, *on-line* and similar expressions are common technical terms that have a firm place in dictionaries, and as such they are words rather than phrases. Phrases do not function as technical terms. *To change over* is classified as a phrase but *change-over<sub>N</sub>* as an exocentric compound (pp. 4–5). It may be proposed, however, that the noun member of this pair was converted from the corresponding verb, which implies that the nominal member — as opposed to the verbal member — is not a compound.

Derivation is divided by Adams into transpositional and nontranspositional, that is, class-changing and class-maintaining; at the same time, she admits that this division is not quite accurate because some prefixes, such as *de-*, *dis-*, *un-*, etc., form verbs with both noun bases and verb bases; adjectival *-able* and nominal *-ee* have nominal as well as verbal bases. Based on this approach, suffixes — rather than separately — are treated in transpositional groups. Moreover, conversion and suffixation are treated under one heading, with the focus of discussion being laid on similarities and differences. This approach reflects an important feature of Adams’s book, demonstrating that setting strict boundaries between various word-formation phenomena is incorrect. Thus, apart from the vague boundary between compounds and phrases, “[t]he boundary

between derivation and compounding is crossed at many points" (p. 16), and there are also numerous ambiguous cases for which there are no firm criteria, for example, some adjective + noun, adverb + noun, and adjective + participial adjective formations (p. 81).

Within the chapter on transposition, some postulates are disputable. Thus, for example, *the innocent*, *the wealthy* are not treated as cases of word formation "since any adjective can function like a noun in a definite noun phrase denoting a class of people, or a quality (*the sublime*)" (p. 20). This, however, seems to give support to the opposite claim and witnesses a high productivity of this word-formation rule. The same applies to "attributively used nouns": they are not treated as converted adjectives "since any noun can be used in this way" (p. 20). Adams's reasons for this classification do not seem to be well founded. Conversion of noun to adjective can be viewed as one of the most productive WF rules in English. High productivity should not become the reason for eliminating this process from the field of word formation. The background of this claim is understandable, though, and concerns the deep-rooted prejudice of Chomskian linguistics against word-formation rules that are considered much less productive and regular than syntactic and inflectional rules. For an opposite view, see for example, Anderson (1982), DiSciullo and Williams (1987), and Štekauer (1998).

Conversion is treated as "transposition without affix," and in Adams's view it "is not essentially different from other transpositional patterns"; "zero" is, in her view, just a "convenient shorthand term for 'without affix'" (p. 20). This claim is arguable because zero morpheme is generally considered to be an affix, that is, a form-meaning complex (see, for example, Marchand's [1969] or Kastovsky's [1968, 1969, 1982] theories of zero derivation, or Haas's [1957] discussion of zero in linguistics), the meaning of which corresponds to that of the respective overt affix.

Adams demonstrates that while complex words with particles are generally classified as compounds, initial particles are closely related to prefixes. Both of them can produce verbs from verbal bases (*precook* : *overlook*), from adjectival stems (*interleaved* : *undermentioned*), nouns from verb-related nominal stems (*subcontractor* : *onlooker*); both can modify monemes (*super-volcano* : *outfield*), etc. (p. 71). Interestingly, however, verb + particle formations are treated as phrases and similar conclusions apply to deverbal noun + particle formations like *shoot-out*, *kick-off*, etc. (p. 76). In the latter case, however, a conversion-based account seems to be more appropriate.

Compounds are discussed in three chapters: noun, adjective, and verb compounds. Unlike her 1973 classification of noun compounds (11 groups), which was rather inhomogeneous, combining different,

incompatible criteria (syntactic, semantic, morphological), her new classification distinguishes five patterns (noun + deverbal noun; noun + noun; noun genitive *s* + noun; adjective + noun; exocentric). The claim that syntactic compounds of the first group, for example, *bicycle-repairing* “are phrase-like in that they are as readily formed and as semantically predictable as transitive verb + object collocations” (p. 79) is of the same sort as that concerning the deadjectival nouns and denominal adjectives (see above). In fact, all productively coined complex words can be labelled as “readily formed”; semantically predictable are all compounds whose actional semantic component is formally expressed. For Adams, *-er*-suffixed synthetic compounds are unpredictable because of their capacity to refer to both things and persons (*coffee-maker*). It might be objected that, from the word-FORMATION point of view (genetic aspect), the difference is captured by postulation of two different WFRs employing two homonymous suffixes with their respective meanings of agent and instrument. This semantic difference is an integral part of the intuition or linguistic competence of a speaker, and therefore, also from the speech-level aspect, the predictability in context appears to be guaranteed.

Exocentric compounds are subdivided according to three patterns: verb + complement (*pickpocket*); adjective + noun (*highbrow*); and noun + noun (*spoonbill*). The claim that many exocentric compounds function as modifiers within noun phrases (*free-lance* writer, *long-nose* pliers, *stop-gap* measures) gives indirect support to Štekauer’s (1998) proposal according to which exocentric compounds are regularly generated in the word-formation component as endocentric compounds (*sabertooth* + *tiger*, *redskin* + *person*, *free-lance* + *writer*) and are formally reduced in the lexicon with the categorial and other features being inherited from the (deleted) head noun.

Segmenting out “role-denoting” coordinative N + N formations like *author-illustrator*, *producer-director*, etc., from the group of compounds by referring to them as phrases — as opposed to *absentee landlord*, *demon barber*, *killer virus*, etc., seems forced (see, for example, Hansen et al. 1982).

Adams properly points out the problems one faces when trying to provide a homogeneous semantic classification of compounds and emphasizes the important role of context in their interpretation.

The position of verb compounds seems to engender a number of problems for morphologists, perhaps because, according to general belief, verb compounding is not a productive process in English. As suggested by Adams, “there will almost always be an intermediary nominal or adjectival expression from which an English compound verb is derived”

(p. 100). Then, however, one may ask whether the term “compound verb” is an appropriate one. If one analyzes *to literary-edit* as back-formed and *to cannonball* as converted from the corresponding N + N compound, then one might propose avoiding the term “compounding” for these kinds of words. For back-formed verbs like *literary-edit*, however, it is possible to suggest a synchronic compounding account (see Štekauer 1998) similar to that proposed by Adams in her “reanalysis” of *literary editor* as (A or N + V) + suffix. In that case, the productivity of verbal compounding necessarily increases.

Another problem accompanying the notion of verbal compounds discussed by Adams is the uncertainty of their inflection if the right-hand constituent is an irregular verb. As shown in the book, compound verbs “transposed without affix from compound nouns often have the regular past tense ending” (p. 102): *joyrided*, *moonlighted*, *potshotted*, etc. Such words entail the problem of head identification, which — in my view — can be best answered by admitting the conversion-based account. On the other hand, Adams distinguishes regularly conjugated transposed compound verbs (see the examples above), irregularly conjugated back-formed verb compounds (*book-keep*, *hand-write*), and those corresponding to noun compounds with a “zero” derived deverbal head (*deep-freeze*, *free-fall*). The latter type has irregular past tense “because *freeze* and *fall* are far more common as verbs than as nouns or heads of noun compounds” (p. 103), thus appearing to be the only genuine verbal compounding type in Adams’s analysis. Generally speaking, if a verbal compound includes a converted verb, its inflection appears to be regular, while simple verbs (unconverted) as compound constituents transfer their irregular conjugation to a compound as a whole.

Adams’s focus, however, is not only on classification. She looks for the reasons for limited use of particular patterns. In the case of verbal compounds, one of the reasons — apart from the already discussed uncertainty about the past tense — may be an instance of inappropriately defocused information conveyed by the modifying element, which makes some compound forms rather awkward (“I was cabinet-making in the garage”). On the other hand, there are also factors contributing to wider use of verb compounds: for example, the possibility of focusing the right-hand constituent (p. 106). Given these considerations, Adams’s conclusion appears to me rather surprising and too strong: “... genuine verb compounding is not likely to develop in modern English” (p. 109).

Stem-based formation is another issue discussed in *Complex Words*; it is considered to function as an alternative system in English, alongside the major, word-based system, on the ground that “some affixes productively combine with incomplete word bases” (p. 114). This is a

disputable claim from the point of view of those conceptions of word formation that maintain that both complex-word constituents should be meaningful units (bilateral signs). This is what does not seem to be the case with stem-based formations, although Adams believes that “the stems that appear in modern coinages have meanings for some speakers at least.” Reference to meaning plays an important role here in terms of the combinability of formative elements, more important than the specification of syntactic category of the WE base (p. 115).

This chapter brings a reader to an even more peripheral phenomenon of word formation, the phonaesthemes, the position of which within the system of word formation is disputable if one postulates that word formation deals with productive patterns of coining new complex words. This is recognized by Adams herself when she maintains that phonaesthemes can hardly be compared with the complex words discussed in the other chapters of her book: they are characterized “by elusiveness of meaning, and subjectivity in judgements about it, by proliferation and volatility of form” (p. 132). The reasons underlying the detailed and careful treatment of this group of words follow for Adams from their use and “creation” by speakers, as well as from their frequency and analyzability.

Under the heading of “Reanalysis,” Adams discusses some affixes, back-formation, blending, and shortening. Back-formation, as indicated above, is accounted for as a reanalysis of a “longer” form as a complex base + suffix, with the subsequent subtraction of the suffix (for example, [*guest edit*] + *or*). Certainly, one might ask a question concerning the justification of such a step from the synchronic point of view and propose an independent derivation of the “shorter” form (*guest* + *edit* vs. *guest* + *edit* + *or*), which appears to me a more straightforward account.

Whether or not shortening should be included in the field of complex words is a matter of discussion, because — in my view — they are not “complex” and do not represent new words: clippings have no new meaning, independent of that in their corresponding full forms. On the other hand, they represent new word-formation bases that can serve for coining new complex words, as demonstrated by Adams.

The final chapter gives a useful overview of the manifold factors conditioning the identification of patterns on which new complex words are formed and of factors determining the productivity of such patterns.

The structure of this review may seem unbalanced in view of my initial remarks: despite their clearly positive content, the major part of the review has been devoted to some of Adams’s problematic positions in treating theoretical issues. This is, however, understandable. While these moot points concern details, the positive features are of a conceptual

nature and pertain to the book as a whole. Hence, the latter can be summarized very briefly as follows:

It is good that Valerie Adams has returned to her former love and decided to take up again the intricate issues of word formation. Her main contribution lies in her lucid, systematic, and in-depth account, extraordinary sense of detail, ability to capture the subtleties and intricacies of the individual word-formation processes and types, and ability to give a vivid picture of the latest tendencies in modern English word formation against the historical background, this being supported by a number of examples illustrating both the regularities and idiosyncrasies in context. Therefore, this book is sure to become an indispensable manual for everyone who is interested in this exciting field of linguistics.

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James Higginbotham, Fabio Pianesi, and Achille C. Varzi: *Speaking of Events*. Oxford: Oxford University Press, 2000. 295 pp.

A number of books have been published in 1999 and 2000, perhaps to welcome the new century, but only a small proportion of them really qualify as excellent books. *Speaking of Events* is, in my opinion, one of the finest books ever written in the area of linguistics. In the following I will justify my opinion. This book deals with semantics in terms of the Davidsonian program, which is considered critically and further extended. Parsons attempts to extend the program to states. Higginbotham discusses the relationship between telicity and homogeneity and furthermore considers adverbs such as *intentionally* and *quickly*, and negative events; Eckardt analyzes the idea of causation and then moves on to distinguish between causal and pseudo-causal statements in terms of event semantics; Asher distinguishes between propositions, facts, and events and, furthermore, investigates the interesting issue of evolutive anaphora; ter Meulen investigates the sequential semantics of sentences expressing activities and progressives; Verkuyl investigates the relation between the internal and external argument of the verb and aspectuality; Delfitto and Bertinetto and Lenci and Bertinetto investigate the distinction between habituality and perfectivity in languages like English and Italian.

Both Higginbotham and Pianesi and Varzi make clear that recourse to the Davidsonian event semantics makes simplification in natural-language semantics feasible. Higginbotham offers various examples of how positing a hidden event place in the argument structure of a predicate simplifies semantics — albeit he adds that this simplification is achieved at the cost of positing the hidden event place. There is in Higginbotham’s article a concealed reference to Occam’s Razor — positing an additional linguistic element is justified by the kind of work it can do. The arguments

in favor of positing the hidden event argument in the argument structure of the verb are well known: adverbial modification, quantificational adverbs, nominalization, causal contexts, tense semantics, etc. Higginbotham explicitly states on p. 51 that

Semantic values for nominals must for Montague include higher types, and the combinatorial semantic principles, projecting meaning from parts to wholes, will (even for the simple sentences discussed above) include the application of functions whose arguments are of a higher type. But higher types do not appear in connection with the E-position, and as Davidson noted in part, and as I explain more fully below, with the E-position hypothesis a certain simplification and principled restriction of semantic combination becomes possible — at the cost, of course, of positing the E-position itself.

Pianesi and Varzi, in the introduction, point out some notorious problems of event-based semantics. It might be wise to discuss some of these in the context of this review. Examples such as (1) have been considered thorny for the theory:

(1) Jones filled the tank halfway

The problem that is allegedly imputed to sentences such as (1) is lack of isomorphism with sentences such as (2):

(2) John filled the tank quickly.

One of the niceties of the event approach is that it explains (so Parsons argues) matters such as entailments. Thus if (2) is analyzed as “There is an event of filling in which John and the tanks are involved as, respectively, agent and theme, the event is quick and the event occurs at  $t$ ,  $< t_u$ ,” it is clear that (2) must entail *John filled the tank*. The problem for (1) is that the sentence cannot be treated in the same way as (2) and, in particular, the adverb cannot be dropped without losing some important entailment of the sentence. This kind of problem has been considered as evidence in favor of recourse to functions of higher types. In other words, adverbial modification, at least for these cases, would have to be dealt with by applying the denotation of the adverb to that of the verb.

Now I must admit that I am perplexed, not so much by the alleged problem, but by the analysis of sentences such as (1), which shows a misunderstanding of the semantics of the verb. To say that the denotation of *halfway* could be applied to the denotation of *filling* is, in my way of seeing things, utterly wrong, as according to respectable dictionaries such as Longman, *fill* is defined in the following way (the other senses are not relevant to our discussion):

(i) Fill = vt 1a to put as much as can be held or conveniently contained.

Now, if this is the semantic elucidation for the meaning of *fill*, there is hardly any room for the view that *halfway* is applied to *fill* to yield *fill halfway*. There might be similar problems for *partially filling*. There are similar problems for the analysis of *I partially removed the dust*. We will not want to say that the removing was partial (I do not know what a partial removing is) but that the dust removed was (only) part of the dust (that had to be removed). These adverbs are quantificational not in the sense that they quantify over the flow of events denoted by the verb but in the sense that they quantify over the object. Thus I would think that (1) can be analyzed as *Jones filled half of the tank*. Now this interpretation is yielded by a Davidsonian analysis too, as it can be rendered as “There is an event of filling in which Jones and half of the tank are involved as agent and theme and the event occurs at  $t < t_u$ .” Now it follows straightforwardly that (1) cannot entail *Jones filled the tank*. The lack of isomorphism between *Jones filled the tank quickly* and *Jones filled the tank halfway* derives from the fact that the former is a manner adverb whereas the latter is a quantificational adverb (strictly speaking, both adverbs are quantificational, but the domains over which they quantify are distinct). Is recourse to functions of higher types indispensable? I propose that we should analyze (1) as

- (ii) [S [NP Jones] [VP [V' t [V' filled [NP t [NP the [N' halfway<sup>1</sup> [N tank]]]]]]]]]]

By moving down at LF the modifier ends up as a modifier of the N *tank* and the desired meaning is arrived at. No recourse to functions of higher types is required. Maximal projections, potentially barriers to movement, are circumvented by Chomsky adjunction (see May 1985).

Verkuyl points out another (alleged) problem for the Davidsonian analysis. While Davidson illustrated the kind of work the hidden event position could do in anaphora (*Jones filled the tank. He did it in the garage*), Verkuyl thinks that sentences such as *Three students filled the tank. They did it in the garage* are potentially problematic in so far as they involve sloppy identity and need to be dealt with by lambda abstraction. Specifically Verkuyl considers sentence (3):

- (3) Three girls buttered their toast in the bathroom. They did it in the bathroom.

Leaving aside the extravagance of the content of the example, Verkuyl presumably argues that there is a joint-event reading (the girls share the toast), and another separate-events reading (the preferred reading), which requires recourse to lambda abstraction. But now her argument presupposes that, at least in one reading, the Davidsonian analysis is

indispensable, which is all we need to show that events have work to do in semantics.

The authors of the articles in the book address the following questions: what are events? Can there be negative events? What is the distinction between events, facts, and propositions? Are states things distinct from events? These questions are addressed in the book here and there and sometimes they receive different treatments. For example, Pianesi and Varzi, aware of the difficulty involved in defining events, claim on p. 4 that

It is possible to work out an event-based semantics of tense and aspect without explicitly committing oneself to any specific metaphysics of events, and it is possible to work out a metaphysical theory without drawing out all its implications for, and applications to, natural language semantics.

While I may agree that a philosopher who has a metaphysics for events need not be interested in working out all its implications for semantics, I do not understand the claim that it is possible to do semantics without committing oneself to a specific metaphysics. I agree that it is possible in the sense that one could do it, but I wonder whether one should be allowed to do it. Furthermore a metaphysics for events might have consequences for linguistic semantics, as I hope to show when I turn to Asher's paper.

Higginbotham does not dwell on a definition of events but he briefly states that, for him, events are concrete objects. Now, while it is not obvious that this might be disputed, I wonder whether this static picture might be really fruitful. Pianesi and Varzi mention one aspect of the Davidsonian analysis of events, which was ultimately abandoned by the philosopher, causality. I think that a metaphysics of events must make reference to some causal notion. One further feature to be noted is that an event is a transition from a state to a subsequent state. Asher's conception of events makes explicit mention of the pre-state and the post-state of the event. As he makes use of this metaphysics in the analysis of evolutive anaphora, Pianesi and Varzi's already cited claim must be tempered. Is there any distinction to be drawn between states and events? Here views can differ. Higginbotham assimilates states to events. Now, I think this is logically incompatible with Asher's view of events, which makes reference to the pre-state and the post-state of the event and, thus, implicitly endorses the distinction between states and events.

I found the article by Parsons entitled "Underlying states and time travel" of great interest. The question Parsons addresses is whether we should consider states as linguistic entities available for the analysis of sentences on a par with events. Parsons takes for granted that having

events in the analysis of sentences accounts for modifier pro-drop and modifier nonconjunction.

Thus, consider the following sentences:

- (4) a. John walked slowly  
b. John walked

It is clear that one who accepts that John walked slowly has to accept that he walked.

Now consider the following:

- (5) a. John killed Brutus violently  
b. John killed Brutus with a knife  
c. John killed Brutus violently with a knife

Parsons argues that the first two sentences do not (jointly) entail the third, because the first two sentences may refer to two distinct events and the event where the knife was involved (as instrument) need not have been violent.

Parsons is struck by the fact that modifier nonconjunction does not follow apparently from an analysis of sentences in terms of states.

Thus consider the following:

- (6) a. John is under the table  
b. John is on the floor  
c. John is on the floor under the table

This appears *prima facie* to be a valid inference. Parsons resorts to a complex time-travel story to explain why it cannot be the case that in the case of states modifier nonconjunction cannot obtain, on the assumption that states act syntactically as events or at least deserve the same kind of analysis.

To start with there are a number of things that puzzle me. Surely modifier nonconjunction is arrived at by the hypothesis that the events that take part in the alleged inferential step are distinct. But surely identity is required in any demonstrative inference where semantic underspecification is involved, as shown by examples in Levinson (1983).

Thus:

- (7) I am the father of the child  
The child is happy  
∴  
I am the father of a happy child

This is a valid inference only if we assume that referents are fixed (pragmatically or by any other correct method) and are assumed to be identical

(the “I” of the first premise must be identical in reference with the “I” of the conclusion) (see Levinson 1983). Now, if events are assimilated somehow to objects, identity must be a prerequisite for demonstrative inferences of the kind Parsons considers. Whether this is decisive or not, it ought to be said. The other thing that puzzles me is that Parsons is looking for evidence that might serve to justify states so as to be able to answer the charge of postulating superfluous entities (so he tries to adhere to Occam’s Razor). I have always been puzzled by Occam’s Razor as well — not so much by the Razor but by the use of the Razor by linguists.

I believe that states, as entities of linguistic analysis, do some work in adding to the conceptual clarity of sentences. To have a more articulated analysis of a sentence amounts to having a better understanding of the meaning of the sentence. Is not this enough to argue in favor of the benevolence of entities such as states? The obsession with Occam’s Razor is surely exaggerated (but see Jaszczolt 1999 and Capone forthcoming c).

I do not understand why Parsons should be puzzled by the fact that states behave unlike events with respect to syntax or deductive inferences. Surely, if he is really preoccupied by Occam’s Razor, he should be able to differentiate states from events — the differences that emerge are not pernicious in this sense — unless his purpose is to assimilate states to events.

From a theoretical point of view, Parsons does not say anything about the difference between states and events. I would believe that the kind of link there is between the two is one of presupposition. States do not presuppose events, while events presuppose states.

Now to time travel. The story is like the following. Socrates is talking to Parmenides at *t* sitting outside the city walls. He travels back in time and arrives at a world that is identical with his world but for one feature, the clock has been put back one year. This one-year-younger Socrates lives in this world for several months until *t*. Now according to Parsons we have the same Socrates lying at *t* in the marketplace. According to Parsons the inference

- (8) Socrates is sitting  
       Socrates is in the market place  
       ∴  
       Socrates is sitting in the market place
- is not valid.

Hence he arrives at the conclusion that modifier nonconjunction is valid for states as well and that inferences such as (9),

- (9) Socrates is in the market  
 Socrates is under an awning  
 ∴  
 Socrates is in the market under an awning

are in fact valid enthymemes, inferences with tacit premises, guaranteed by world knowledge, such as “the entities in question are not in two places at the same time.” But in a sense Parsons must be wrong — or the story must be advanced further — as despite the fact that Socrates is in two places at the same time, inferences such as (10),

- (10) Socrates is sitting outside the city walls  
 Socrates is sitting on the floor  
 ∴  
 Socrates is sitting on the floor outside the city walls

are nevertheless valid. Even if we fully believed Parsons’s story, the demonstration does not work because even in this mixed world (a world resulting from a strange mixture between two worlds) modifier non-conjunction is not demonstrated.

Parsons needs to look at evidence from conjunction to see the systematic work done by states, as temporal-sequencing inferences are sensitive to this kind of information. Temporal sequencing occurs with action sentences and events, but not with states or with conjunctions between events and states (see Capone 1997, 2000, forthcoming a, forthcoming b). It is syntactic phenomena of this sort that might justify resorting to states — if justification is really needed. I suggest we should look at this sort of discourse phenomenon to discover the full value of Parsons’s analysis. I think that Parsons goes wrong in the attempt to disinfect a cause of intellectual anxiety, while he simply has to cast away his unjustified anxiety.

We now turn to the delicate question of whether there is a distinction between propositions, facts, and events. Asher argues that there is. For him propositions exist eternally in the sense that they exist independently of whether they are true or not. This might be a good criterion for the distinction. Asher has another argument for the distinction, which is based on the identity principle (p. 127). For Asher sentences such as (11) and (12) are factive contexts in that an expression can be replaced with another expression denoting the same referent:

- (11) Documents A indicate that Cicero was the most highly regarded philosopher of his time.  
 (12) Documents A indicate that Tully was the most highly regarded philosopher of his time.

The verbs *show* and *indicate* take as arguments facts, not propositions. When just a proposition is at stake, referential opacity appears, as in *I believe that Mary is at home*. In this case, I do not believe a fact but a proposition. It remains to be seen whether Asher's considerations are contradicted by data such as *John knows the fact that Mary is in Paris* where one could replace *Mary* with *the Queen of the Scots*, obtaining a fact not known by John. Notice that at least in Italian these sentences are not grammatical (\*Io so il fatto che Maria è a Parigi). Even if the English version were judged to be bad, which I do not take for granted, one must at least admit that *The fact that Mary is licking her fingers is embarrassing* has some opacity, as the sentence might be true or false depending on whether Mary is or is not the queen. Now it appears that, if opacity is considered good grounds for the distinction, one should avoid using the word *fact* in order to express a fact. This conclusion is suspect. What is instead more useful in distinguishing propositions from facts is (p. 128) the consideration that "facts appear to have causal efficacy, whereas propositions do not." Of course, this criterion alone is not sufficient to distinguish between states/events and facts, a distinction that is dear to Asher but, in my opinion, is less important than that between facts and propositions. Asher distinguishes events from facts because we can have negative facts but not negative events, we can have disjunctive facts but not disjunctive events, we can have conditional facts but not conditional events. If we consider Higginbotham's considerations about the existence of negative events the distinction is not so clearcut. Second, facts as well as events seem to have causal efficacy. The arguments about disjunctive or conditional events have some force, but only if one distinguishes between events and states. I am not persuaded by Asher's distinction because it does not cast light on the relation between a fact and an event/state. The relationship ought to be one of presupposition, considering that we make use of sentences such as *The fact that p caused the fact that q*, where *p* and *q* map to events that are presupposed.

Concerning negative events, Higginbotham (p. 75) provides an interesting treatment, which can be summed up below:

$$(13) \quad \forall I (\neg \exists (O(T(e), I) \wedge H(e)) \Rightarrow \exists e' (\text{Compl}^2 H(e') \wedge T(e') = I).$$

My guess is that whether there are negative events or not will still be debated in future discussions. Probably what might serve to define a negative event is reference to a norm that was violated, the violation constituting the negative event — but as this is what Higginbotham denies, I leave the matter as it is, pointing out that here judgments may differ.



We have seen that both events and facts can have causal efficacy. In an interesting article, Eckardt analyzes critically the notion of causation. She exposes Lewis's notion of causation and then criticizes it.

Lewis (1973) on causation:

- D.1 The relation  $O$  is a unary relation on the domain of events.  $O(e)$  is true in a world  $w$  iff  $e$  occurs in  $w$ . An event  $c$  causes an event  $e$ ,  $c$  CAUSE  $e$  iff
- a.  $O(c) \square \Rightarrow O(e)$  and
  - b.  $\neg O(c) \square \Rightarrow \neg O(e)$
- ( $A \square \Rightarrow B$  = if  $A$  were the case,  $B$  would be the case)

On p. 107 Eckardt argues that Lewis's treatment cannot distinguish between causes and necessary preconditions. Her argument is based on a story in which Joe had a bad accident in 1989 ( $e_1$ ). Luckily Dr. Spock gave him first aid ( $e_2$ ). He managed to get Joe's heart beating again ( $e_3$ ) and thus saved his life. One year later, Pat shot at Joe ( $e_4$ ), and Joe died ( $e_5$ ).

The situation exposed by Eckardt, in my opinion, does not really falsify Lewis's treatment. Eckardt writes on p. 107, "If the events  $e$  and  $c$  both occur in  $w$ , then condition (a) is trivially true and (b) roughly says 'If  $c$  had not occurred,  $e$  would not have either'." However, it is not true that in the situation exposed in the example condition (a) is trivially true. Condition (a), following McCawley (1981), in words amounts to "If  $A$  were the case,  $B$  would be the case." Now, if we use Stalnaker's (1999) considerations about conditionals, we establish that in all worlds where  $A$  is true,  $B$  must be true. Now the situation exposed by Eckardt is not such that in all worlds where Dr. Spock gives first aid to Joe, Joe dies. There might be worlds in which Dr. Spock gives him first aid and Joe does not die. Thus, Lewis's definition is by no means falsified by the narration in question.

Let us now see if Eckardt's alternative treatment is at least superior to Lewis (this is a possibility of course). Her treatment boils down to

- D.2  $\forall w (w \models \neg O(c') \Rightarrow \exists w' (w' \models O(c') \wedge \neg O(c) \wedge d(w^\circ, w') \leq d(w^\circ, w)))$

However, it is not clear how D.2 can deal with the notion of co-causation. Suppose that Dr. Spock intentionally provides first aid in such a way that although Joe is going to recover temporarily, should he be involved in another accident he would die (and there is a method for obtaining this effect). Then at a subsequent time, someone shoots Joe. At this point both the shooting and Dr. Spock's first aid are joint causes of the event of Joe's dying. Lewis's treatment of causation allied to Stalnaker's view of condi-

tionals immediately takes care of co-causation. Does Eckardt's treatment do this?

All D.2 tells us is that the distance between the worlds in which the event of giving first aid occurs but the event of Joe's being shot does not occur and the actual world is smaller than the distance between the worlds in which the event of giving first aid does not occur and the event of Joe's being shot (later) occurs and the current world.

Presumably Eckardt's notion of causation is that given  $c, c', c''$  as potential causes of  $e$ ,  $c'''$  is the cause of  $e$  iff the worlds in which  $c'''$  occurs but  $c$  does not occur are more distant from the actual world than the worlds in which  $c$  occurs but  $c'''$  does not occur. But if this is the notion at the basis of her treatment, then this treatment cannot accommodate co-causation. Considering that  $c$  is both a cause and a precondition, a definition of causation in terms of distance from the actual world is no longer possible.  $c$  being a precondition, the worlds that do not contain  $c$  but contain  $c'$  are more distant from the current world than the worlds that contain  $c$  but do not contain  $c'$ . But given the definition by Eckardt, now  $c$  cannot be a cause any more. But indeed  $c$  is a co-cause.

Eckardt in her paper provides a most interesting analysis of pseudo-causal statements such as

(14) The [delayed]<sub>focus</sub> departure caused Bob's rescue.

(14) is rendered as  $\forall w ([-\exists e (\text{departure}_w(e) \wedge \text{delayed}_w(e) \wedge \exists Q \exists e1 (Q \in [[\text{delayed}_f \text{departure}]]_f \wedge Q_w(e1)))] \Rightarrow -\exists e2 (\text{rescue}_w(e2) \wedge \text{theme}_w(e2, \text{Bob})))$  where  $[[\text{delayed}_f \text{departure}]]_f$  is the set of alternatives to the delayed departure (e.g. a punctual departure, etc.).

So far I have concentrated on the most philosophical part of the book. However, each of the articles also makes a contribution to linguistics, showing the kind of work that events do in the semantics of English or other languages. Higginbotham for example uses events to deal with adverbs and telicity.

Higginbotham considers sentences such as (15):

(15) Mary quickly objected

This sentence is ambiguous between an interpretation according to which the objection was quick and an interpretation according to which Mary was quick to object. The latter interpretation will be represented as:  $\exists e \exists e' (\text{quick}(\text{Mary}, e', e) \wedge \text{object}(\text{Mary}, e'))$ . In this account involving theta identification, Mary is the subject both of *quick* and of *objected*.

Higginbotham addresses the question of the relationship between inhomogeneity and telicity. He redirects arguments by Bach (1986) and claims that inhomogeneity is a consequence of telicity (if I ate an apple,

there was a process and a telos and the achievement of the pair of events  $\langle e, e' \rangle$  cannot be identical with any component of the pair  $\langle e, e' \rangle$ . According to Higginbotham homogeneity of the event described in the predicate results in atelicity. If a verbal projection has the property that the subevents of an event  $e$  that it characterizes are of the same type as  $e$ , then the E-position in that projection cannot be  $\langle e, e' \rangle$ .

Asher (pp. 35–142) considers telic verbs of construction or destruction and uses the event position to show that anaphoric uptake depends on the post-state of the event in question as well as on the sequential placement of the sentence containing the anaphoric pronominal. Consider (16):

(16) The bomb destroyed the car. The police inspected it closely.

According to Asher this sentence ought to be out, as the car no longer exists and, thus, anaphoric uptake is not possible.<sup>3</sup> Asher's important contribution is to have noted how rhetorical relations influence the possibility of anaphoric uptake. Thus, a sentence such as *The bomb destroyed the car. It disappeared in a flash*, where we find the relation *elaboration*, anaphoric uptake is possible. On pp. 138–140 Asher provides an interesting analysis of these facts in terms of a pre-state and post-state analysis.

The important role of having a specific place in a sequence of assertions is emphasized by ter Meulen, who on p. 153 provides a discourse rule such as

- H.2    a. PAST (e1 ACCOMPLISHMENT) + PAST (e2)  
           = e1 precedes e2, all of e1 precedes e2.  
        b. B. PAST (e1 ACTIVITY) + PAST (e2)  
           = e1 includes e2, part of e1 precedes e1.

When examples become more complicated, linguistic judgments vacillate. Consider (17), (18):

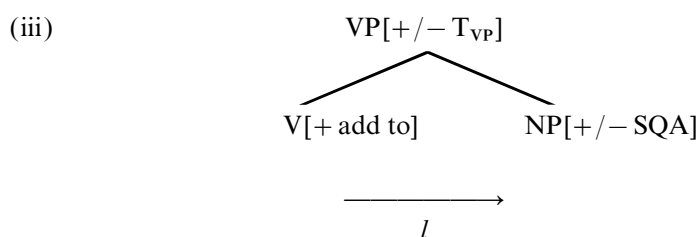
- (17) Jane noticed a car parked in an alley. She patrolled the neighborhood. She was driving along the Rokin.  
 (18) Jane noticed a car parked in an alley. She was patrolling the neighborhood. She was driving along the Rokin.

Concerning these examples ter Meulen says that in (17) the noticing and the patrolling are distinct events in a sequence, whereas in (18) the noticing occurs within the time span in which the patrolling occurs, which in turn is included temporally in the time span in which the driving occurs. In her article, ter Meulen has a plethora of examples that show that the sequential placement of a sentence determines the temporal semantics of the sentence.

The article by Verkuyl is quite interesting because it connects the issue of aspectuality with that of the argument structure of a verb. The basic data are the following:

- (19) Judith ate a sandwich  
 (20) Judith ate sandwiches  
 (21) Nobody ate sandwiches

Verkuyl notes that the inner argument determines whether the VP is durative or terminative. If the internal argument pertains to a specified quantity of A (where A is the denotation of the head noun), the VP is terminative. If the internal argument pertains to a – specified quantity of A, the VP is nonterminative or durative. Thus, it appears from the following diagram that the quantificational features of the NP project to the higher VP node:



$l$  is a path function as defined below:

- (iv)  $l_x : I D_L$  with  $l_x \langle i, p \rangle : [[AT(p)(x)]]_{m,i} = 1$   
 (i being a time interval)

The path function is a function from the position that  $x$  occupies at  $i$  to the value terminative/–terminative. Thus if we know that Mary lifted three tables, we know that at position  $i + 2$  she has lifted a quantity 3 of tables and her action has come to an end. Thus, the path function  $l$  is a function that will map the specified quantity 3 at position  $i + 2$  to the value + terminative of the relevant VP.

It is time to turn to the article by Lenci and Bertinetto on habituality vs. perfectivity, which I will now discuss *dulcis in fundo*. Lenci and Bertinetto discuss the semantics of imperfect with that of perfective sentences. They observe that in Italian imperfect sentences admit quantificational adverbs such as *di solito* ‘usually’ or *generalmente* ‘generally’, while perfective sentences do not. Below are some examples:

- (22) In quel periodo Gianni generalmente/di solito si svegliava alle sei.  
 ‘In that period Gianni generally/usually woke up at six.’

- (23) ??In quel periodo Gianni generalmente/di solito si è svegliato alle sei.

‘In that period Gianni generally/usually woke at six.’

From these data Lenci and Bertinetto deduce that the imperfect expresses habituality and this feature opposes it to the simple past.<sup>4</sup> Lenci and Bertinetto claim that the perfective sentences cannot be turned into habitual sentences by the addition of a quantificational adverb and that imperfect sentences are habitual sentences, whose semantics can be captured by reference to the operator Gn in the logical form. The authors move on to express the notion of habituality through a modal notion, incorporating ideas by Kratzer such as a modal base that is contextually determined, which contains the presuppositions of the conversation, and an ordering source that orders worlds in such a way that those that are closer to the actual world are considered. Now the semantics of habitual/imperfect sentences is given below:

- (v)  $[[\text{Gn } [\varphi(d_1, \dots, d_n)] [\psi(d_1, \dots, d_n)]]]_{w,g} = 1$  iff given a modal base  $B_w$ , for every assignment  $h$  such that  $h = g[d'_1, \dots, d'_n/d_1, \dots, d_n]$  and every  $w_1 \in B_w$  such that  $[\varphi(d_1, \dots, d_n)]_{w_1,h} = 1$  there is a world  $w_2 \in B_w$  such that  $w_2 \leq w_1$  and for every world  $w_3 \leq w_2$  it holds that  $[[\psi(d_1, \dots, d_n)]_{w_3,h} = 1$ .

Summing up, I believe that this is a most instructive book, certainly one worth reading. It addresses many interesting problems and the solutions are generally impressive. For those who are intellectually curious, this is very good nourishment for the mind!

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## Notes

1. We assume that morphology (-ly) is no barrier to movement at logical form.
2. Compl = the complement of.
3. The issue is not so clearcut. I could say things such as *The bomb destroyed the car. It did not move any more* or *The bomb destroyed the car. I looked at it astonished (I looked at what was left of the car)*. We wonder how much the question of anaphoric uptake depends on world knowledge.
4. However, notice that sentences such as *Quell'anno io l' ho sempre visto Mario* ‘That year I always saw Mario’ are perfectly all right contrary to what the authors seem to imply.

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Juan Manuel Sosa: *La entonación del español*. Madrid: Cátedra, 1999. 264 pp.

Sosa's book, a long-awaited study based on his outstanding doctoral dissertation *Fonética y fonología de la entonación del español hispanoamericano* (Sosa 1991), has come as a very agreeable surprise to scholars interested in Spanish prosody. Ever since Navarro-Tomás and Quilis published their seminal research on Peninsular Spanish, the study of intonation has occupied a rather marginal position within the discipline of Hispanic linguistics. The present book is bound to redress this situation by calling attention to this currently burgeoning facet of linguistic theory. The main goal of Sosa's book is to investigate the phonological properties of the intonation contours of several Spanish dialects. Its first positive aspect lies in its descriptive side, for it takes into account a wide pool of data from different Spanish dialects. As the author notes in the first chapter, the basic materials for this project were collected from hours of recording natural conversations from different dialects: "Para este estudio de la

entonación del español hemos utilizado como fuente principal de datos, nuestro extenso corpus constituido de varios centenares de horas de conversaciones, entrevistas y cuestionarios orales grabados de distintas variedades del español, provenientes de la mayor parte de los países hispanoamericanos además de España” (p. 91). The second asset of the present study resides in its strong theoretical basis. Sosa adopts one of the most widely recognized phonological approaches to intonation, namely, the autosegmental-metrical approach — the reader can find an introduction to this model in Pierrehumbert’s (1980) thesis and, more recently, in Ladd’s (1996) book. This study can thus be considered the first full-fledged application of the metrical model to Spanish intonation and a necessary starting-point for subsequent work in this area.

The book is coherently divided into three main chapters. Sosa devotes the first chapter to a very clear and informed introduction to intonation, which may be quite useful for any student of prosody or any reader with a general background in linguistics. The author discusses topics such as the relationship between intonation and phrasing, the factors that influence phrasing decisions in discourse, and the interaction between intonation and stress. Finally, after an overview of some relatively recent contributions to the analysis of Spanish intonation, Sosa proceeds to review the main principles and assumptions underlying Pierrehumbert’s (1980) metrical model of intonation. In a way, the first chapter serves to lay out and motivate the theoretical assumptions adopted in the second chapter, which constitutes the core analytical part of the book. In this chapter, Sosa presents a descriptive analysis of Spanish intonation, taking the metrical model as its main theoretical basis; he also provides a number of well-grounded and insightful observations about the phonological patterning of different Spanish contour types. The third chapter compares a selection of intonation contours of three sentence types (declarative, yes–no questions, and wh questions) produced in several Spanish dialects, concluding that the differences found between them are phonological in nature (rather than phonetic, as was previously claimed). Finally, the *Epilogo* summarizes the main conclusions of the book and its implications for intonation theory.

As far as the methodology is concerned, Sosa acknowledges the importance of taking into account the two complementary aspects of this prosodic feature, namely, its phonological and its phonetic side: “Dada la complicada naturaleza estructural y la multiplicidad de funciones que cumple la entonación, una mera descripción de su sustancia sin referencia a lo lingüístico sería inadecuada e insuficiente, tanto como las referencias funcionales apriorísticas que no incluyen precisiones con respecto a cómo se manifiestan esas funciones en su forma fónica” (p. 247). Hence, the

book is very careful to provide the reader with the  $F_0$  contour of each of the sentences it analyzes, together with a general description of the meaning it conveys.

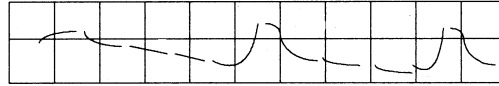
In this review I would like to briefly discuss three of Sosa's proposals regarding the phonological structure of Spanish intonation. I take these proposals to be important criticisms to standard assumptions of the metrical framework, whose resolution may condition the development of the model and its further application to other Romance languages.

Sosa's primary concern in chapter two is to examine the phonological properties of Spanish intonation contours within the metrical model. To accomplish this, he first analyzes acoustically a selection of Spanish tunes and then proceeds to advance an inventory of phonological *pitch accents* and *boundary tones* (the main building blocks of intonation contours) that can account for the tonal variation found in this language. Within the metrical model, phrase-final tones can be of two types: *phrase accents* (associated with the limits of *intermediate phrase boundaries*) and *boundary tones* (associated with the limits of *intonational phrase boundaries*). Moreover, the two tonal events display different association patterns with the segmental material: while boundary tones are always linked to the end of intonation-phrase boundaries, phrase accents describe the tonal trajectory between the nuclear accent and the limits of the prosodic domain (cf. Pierrehumbert 1980: 32). Sosa (1991 and the volume under review) claims that phrase accents could be eliminated entirely from the tonal inventory of languages where the intonation nucleus is always placed at the end of the intonation group. According to Sosa, "es oportuno mencionar que la noción de acento de frase puede ser menos útil, incluso supérflua, en lenguas como el francés, el español y demás lenguas de núcleo fijo. En estas lenguas, el último acento tonal no puede estar muy lejos del borde derecho de la frase, por lo que un eventual acento de frase no podría generar ningún tipo de contraste" (Sosa 1991: 69; Sosa under review p. 87). "En general, no puede haber más de dos sílabas (inacentuadas) después del último acento tonal, y excepcionalmente tres. Por esta circunstancia, el último acento tonal no puede estar muy lejos del tono de juntura, por lo cual un acento de frase, de cualquiera de los dos tipos (H- o L-), no podría generar ningún tipo de contraste" (p. 95).

Sosa argues that the main advantage of dispensing with the phrase-accent category is to help eliminate the potential ambiguity in the analysis of Spanish nuclear configurations in Romance languages. Let us examine the following schematized exclamative tune from Puerto Rican Spanish — which Sosa names "tonema ascendente-descendente" (cf. p. 232ff.). The final pitch pattern consists of a low rise plus a falling movement that links to the posttonic syllable. Within the standard metrical model, such a tune

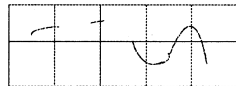


can be either analyzed as  $L^*H - L\%$  or as  $L^* + HL\%$ : the two possibilities cannot be adequately distinguished in Spanish because the last pitch accent is obligatorily attached to the last stressed syllable in the prosodic domain. If we assume, together with Sosa, that no phrase accent exists, then the second option will be the only feasible one: “obligatoriamente debemos recurrir a acentos bitonales para dar cuenta de algunos contornos complejos” (p. 96).



(1) ¡... *con la co ci ne ra de la ca sa ...!*  
 $L^* + HL\%$

In my opinion, the claim of dispensing with the phrase-accent unit is somewhat questionable and the implications of such a proposal will have to be evaluated in detail. Indeed, it is generally true that this proposal helps to eliminate some of the potential analytical ambiguity of nuclear configurations in Romance languages. Yet, on the other hand, Romance languages are also able to produce quite complex final movements that cannot be accounted for with a simple combination of a bitonal accent plus a boundary tone. Let us take a look at the following exhortative contour of Catalan (cf. *¡Escolta!* ‘Listen to me!’), which conveys an additional meaning of insistence and complaint on the part of the speaker. The final tune consists of a rise associated to the tonic syllable followed by a fall–rise–fall movement associated to the posttonic syllable — for more examples of terminal movements of this type, see Prieto (i.p.) for Catalan and Ladd (1996) for Italian. In this case, if we do not allow for pitch accents to have more than one trailing tone (or for boundary tones to be bitonal), then there is a need to refer to the phrase-accent unit — the standard transcription of this case is  $H^* + LH - L\%$ .



(2) ¡ *Es col ta!*

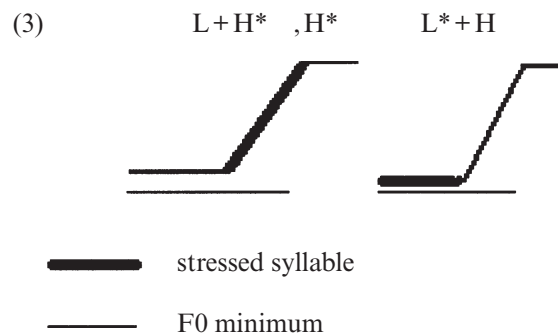
Moreover, dispensing with the phrase-accent category also has implications for the theoretical parallelisms established between this unit and the intermediate phrase-prosodic domain as well as for the labeling of particular final tunes, which call out for reexamination. Let us take the case of the *suspension tone* (“tonema de suspensión”), a level melodic movement (neither rising nor falling), which Pierrehumbert transcribes with the combination  $H - L\%$  and Sosa (pp. 129ff.) reanalyzes as  $H^* + HL\%$  — in a

way, Sosa's labeling accounts for the fact that a final level tone can only appear after a rising pitch accent. Similarly, Sosa reanalyzes the final rise  $H - H\%$  present at the end of interrogatives as  $L^* + HH\%$ . I guess there is no easy answer to the question of what the status of phrase accents should be; the implications of such a proposal will have to be carefully examined in light of some possible evidence about the functional relationship between nuclear pitch accents and boundary tones.

Another potentially controversial aspect of Sosa's study is the analysis of prenuclear pitch accents in unmarked declarative sentences. Even though Sosa's core analysis is framed in the metrical model, he also departs from it in some respects. Following the British tradition (and Navarro-Tomás's notion of *tonema*), Sosa makes a twofold distinction between (a) nuclear or terminal contours (pp. 114ff.) and (b) prenuclear contours (pp. 134ff.). Although the standard metrical view recognizes no internal structure to intonation contours, he argues that making use of such a division is quite useful for Spanish; thus, a finite set of nuclear contours (combinations of nuclear pitch accents plus boundary tones) is presented on page 132. With regards to the analysis of prenuclear declarative accents, Sosa argues that they should be transcribed as  $L^* + H$ : "Nuestra investigación ha arrojado como conclusión la notable regularidad entre todos los dialectos estudiados en lo relativo al pretonema no marcado, integrado por uno o más acentos tonales  $L^* + H$ " (p. 142). The main rationale behind the  $L^* + H$  choice lies in the asymmetry found between the location of the peak in phrase-final ( $H^*$ ) vs. nonfinal ( $L^* + H$ ) pitch accents: "Los picos no finales están situados cerca del final de la sílaba postónica, mientras que los finales se encuentran dentro de los límites de la sílaba acentuada" (p. 143); "Sistemáticamente el pretonema declarativo tiene su primer pico coincidente con la sílaba inacentuada postónica, (...), efecto del acento tonal  $L^* + H$ ." Indeed, several experimental studies have revealed that Spanish prenuclear pitch accents are phonetically realized as a rising movement that starts quite consistently at the onset of the stressed syllable and ends in a more variable position depending upon its right-hand prosodic environment (cf. Prieto et al. 1995 for Spanish; and Silverman and Pierrehumbert 1990 for English).<sup>1</sup> Specifically, these studies defend the position that the H peak is significantly retracted not only in sentence-final position but also in tonal-clash situations, that is, in contexts where the pressure created by an upcoming prosodic event (in particular, a falling boundary tone or a following pitch accent) causes the peak to move leftward.

Recently, the notion of "starredness" has been subject to discussion within the autosegmental approach. The standard diagnostic for starring a tone has generally been the tone's phonetic alignment with respect to

the metrically strong syllable. As shown by the following schema (after Pierrehumbert 1980), the categorical contrast between  $L + H^*$  (or  $H^*$ , depending on how the L valley is scaled) and  $L^* + H$  in English is manifested phonetically through the relative timing association of the LH gesture: while the former is realized as a rising gesture over the stressed syllable, the latter is realized as a low or a falling tone over the tonic syllable followed by a rising gesture that starts near the end of this syllable. In the first case, the tonic syllable is perceived as carrying a high tone and in the second as carrying a low tone. At first glance, then, it seems that the type of rising gesture involved in Spanish prenuclear accents should correspond to a  $L + H^*$  or  $H^*$  pitch accent.



Recently, Arvaniti et al. (1998, 2000) have reconsidered the notion of starredness in light of the alignment and stability properties of LH gestures in Greek. In particular, the authors highlight a common assumption underlying the way stars are used in bitonal accents, namely, that “unstarred tones are subject to spreading in certain circumstances (...), while starred tones are not expected to spread, precisely because they are associated to a particular syllable” (2000: 120). Following this line of reasoning, Sosa’s use of the accent  $L^* + H$  could be interpreted as a way of signifying both the time stability of L valleys and the more variable timing behavior of H peaks (cf. also Hualde 1999). Yet, let me note that, at least for the Mexican and Peninsular dialects of Spanish, neither of the two assumptions is completely accurate. On the one hand, if we examine the behavior of Spanish prenuclear accents in tonal-clash contexts (cf. Prieto and Shih 1995) we observe that the second L value gets displaced to the right of the onset of its corresponding tonic syllable (hence, the L target is displaced from its anchoring position); on the other hand, recent empirical investigations have revealed that prenuclear H values are aligned systematically with the end of word boundaries; further, their location serves as a clearcut perceptual indicator for the identification of

such a boundary (cf. the perceptual experiments carried out by Juanma Garrido, personal communication). Thus, even though H and L points display different degrees of resistance to spreading, they can exhibit both a high degree of stability or, conversely, a displacement from its canonical position. If we want to keep the alleged one-way relationship between “stability” and “starredness,” should both H and L be labelled as starred? Or as unstarred? Should degree of time resistance be encoded phonologically, or is it rather a phonetic consequence of prosodic and segmental pressure on tonal articulation?

Let me express my doubts about using stability of alignment as a diagnostic for starring tones. The Spanish data at hand has revealed such a complex synchronization behavior of L and H values that there is no principled way of telling which one of the two targets should bear a star. Moreover, representing prenuclear accents as  $L^* + H$  would clearly not be an adequate choice in a language that displays a phonological opposition between  $L^* + H$  and  $L + H^*/H^*$  of the sort shown in (3). If the main argument for choosing the label  $L^* + H$  is based on the stable anchoring of L to the onset of the stressed syllable, how are we going to account for such relative alignment contrasts? Catalan is exactly this type of language, as it exhibits both a prenuclear declarative accent similar to Spanish and a categorical distinction between an unmarked prenuclear accent ( $L + H^*/H^*$ ) and a contrastive prenuclear accent ( $L^* + H$ ). In my view, this fact strongly suggests that the timing stability properties of a given pitch target are completely unrelated to its relative alignment with the text. The crucial difference between pitch accents  $L + H^*$  and  $L^* + H$  lies in the relative alignment properties of the tonal gesture in unmarked conditions rather than in the synchronization strength of both targets.

In light of the previous discussion I believe we must seriously reconsider whether Sosa’s transcription of prenuclear accents in Spanish as  $L^* + H$  constitutes an appropriate choice of form.<sup>2</sup> We believe that the theoretical status of starredness (together with its phonetic implementation properties) deserves to be further investigated, as it constitutes a fundamental issue for a better understanding of the mapping procedure between the phonological form and the melodic *continuum*.

Finally, let me comment on Sosa’s interesting solution regarding the status of accent range differences between interrogative and declarative sentences. As already noted informally by Navarro-Tomás and Quilis, one of the traits that can help the hearer discriminate between Spanish interrogative and declarative intonation contours is the greater pitch excursion displayed by the first pitch accent of interrogative sentences. How is this contrast to be expressed in phonological terms? As is well known, Pierrehumbert’s thesis (together with the standard version of the

autosegmental model) treats variations in pitch range as gradient in nature: that is, pitch range is considered to have an expressive use within the language and to directly reflect the degree of involvement of the speaker in the speech act.<sup>3</sup> Yet, recent studies suggest that this strong phonetic view of the phenomenon cannot accommodate certain cases where variations of prominence seem to trigger categorical distinctions in meaning — see recent work and discussions of this issue by Ladd (1990, 1994, 1996) and experiments by Hirschberg and Ward (1992). While Sosa assumes the gradient nature of pitch range in his analysis of exclamative sentences (which are interpreted as allophonic variants of versions involving less pitch range), in dealing with interrogative sentences he takes a different option. Sosa's solution is the following: "La solución que planteamos para dar cuenta de este efecto ha sido postular un efecto de *upstep* puramente local producido por un tono de juntura inicial opcional H%, restringido a las preguntas absolutas, que elevaría la frecuencia de la primera sílaba acentuada" (p. 152). According to Ladd (1996), the relevance of such an analysis lies in the fact that it constitutes a first step toward phonologization of the pitch range category. The case of Spanish interrogatives thus constitutes an interesting example of the types of linguistic effect pitch range might convey in languages and should be considered in the discussion about its phonological status.

In conclusion, we should welcome this book as an undoubtedly important contribution to the phonological analysis of Spanish intonation and, more generally, to the field of intonational phonology. In addition to its valuable descriptive pool, it tackles several theoretical issues (such as the status that should be given to *phrase accents*, *starredness*, and *pitch range*) that represent important thoughts and considerations likely to be elucidated within the next few years. We are certain that this excellent study will attract a wider readership of scholars and students to the field of intonation and encourage further research in this growing area of linguistics.

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## Notes

1. Judging from close inspection of many of the  $F_0$  contours presented by Sosa, this also seems to be the case in many other Spanish dialects.
2. Sosa himself acknowledges that his inventory of pitch accents can be subject to revision: "nuestra interpretación de lo tonal no es por supuesto la única posible, por lo que no descartamos que se pueda reducir este repertorio por medio de estipulaciones o de reglas de implementación, o incluso por mecanismos de sincronización [timing]. Análisis

alternativos al nuestro podrían seguramente explicar las asimetrías en la distribución de los acentos tonales y colmar los vacíos que encontramos en el inventario de acentos tonales” (p. 133). In the preceding discussion, several arguments were presented against the L\* + H analysis. Basically, we suggested that stability of alignment is probably an independent property of intonation that has nothing to do with relative synchronization with the text.

3. As Pierrehumbert (1980: 17) notes, “English makes considerable use of pitch range, with the result that what is clearly the same basic intonation pattern can be produced in many different pitch ranges. The reader can persuade himself of this by calling out to someone he imagines to be across the room, and then across the street.”

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Sylvain Auroux, E. H. F. Koerner, Hans-Josef Niederehe, and Kees Versteegh, editors: *History of the Language Sciences/Geschichte der Sprachwissenschaften/Histoires des sciences du langage* (HSK 18), vol. 1. Berlin: de Gruyter, 2000. xxiii + 1094 pp. ISBN 311 011 1039.

The weighty volume here reviewed is the first of three; the set is intended to provide a comprehensive survey of the language sciences of all ages and in all cultures, a gigantic project easily measuring up in size and scope with the other handbooks listed in the reference section. The present book is made up of 143 articles (with 71 so far planned for volume 2 and 75 for volume 3).

The first 71 chapters are more or less arranged according to language groups and their specific linguistic traditions. Whereas the first section, devoted to the ancient languages of the orient, is untypical because so little of relevance can be said about them (in the case of Ugarit not even the identity of the language is quite clear), the subsequent Chinese chapters are unusual in having so much of the argument restricted to spelling and to philosophy. Chapters 10 to 71, devoted to the traditions of Japanese (chapters 10–16), Sanskrit (chapters 17–26), Arabic (chapters 37–50), Greek (chapters 53–65), and Latin (chapters 66–71), are organized on roughly the same pattern, the full coverage including chapters on the beginnings of linguistic description and early classic authors and works; later developments of national traditions; structural descriptions of the languages and consequences for theory and methods; relations to neighboring disciplines (such as philosophy and literature) and to society; influences on the respective tradition, and its impact on neighboring cultures, often as a consequence of political conquests or the spread of religions. The section on Arabic, which is especially comprehensive and well-done, illustrates how the pattern is filled: chapter 37 is devoted to eighth-century grammars and their origins in the Old Iraqi School; chapter 38 to the classical author Sībawayhi (died 896?) and an eminently clear exposition of his work; chapter 39 to the subsequent development of Arabic linguistics in Basra, Kūfa, and Baghdad; chapter 40 to the innovative approach of Ibn Ğinnī in the tenth century; and chapter 41 to the post-classic period, leading on to chapter 42, a summary of Arabic grammatical theory — for outsiders possibly the most informative chapter. This is in turn related to “logic” in chapter 43, to “revelation” in Islamic

society (chapter 44) — a necessary topic in all societies in which linguistic traditions are bound up with sacred texts — and to rhetoric in chapter 45. Chapter 46 then compares the weight of traditional Arab linguistics and influences of Western thought, followed by three chapters in which the impact of Arab linguistics on other traditions is detailed: on Coptic (chapter 47), Turkic (chapter 48), Persian (chapter 49), and Malay (chapter 50), all clearly consequences of the spread of the Arab people and of Islam. The other sections follow this pattern, with Greek given significantly more space than the discussion of the largely receptive Latin (which is given additional space in the later medieval tradition).

The pattern changes from chapter 72 onward, historical periods of the cultural history of Europe now becoming the organizing principle. Chapters 72–87 summarize the study of Latin and linguistic theories in the Middle Ages, with specific authors singled out, which necessarily results in certain imbalances: for instance, Alcuin (chapter 73) and Ælfric (chapter 85) are the only English authors treated in detail, and there are two chapters specifically devoted to the Low Countries (chapter 78, chapter 81), but not to other individual countries. Since so much has been written on medieval linguistics recently, the section included here is a useful complement (especially in its contrastive portions) rather than remarkable for innovation. Five chapters follow on the functions of Latin and Greek in Renaissance society (chapters 88–92) and on the teaching of the vernacular and classical languages in Western Europe (chapters 93–101). This section, helpfully framed by Schröder's summarizing chapters on commercial interests in, and educational traditions of, the vernaculars, is of course only marginally related to the volume's main concerns, its relevance being restricted to linguistic theories applied in teaching strategies.

Ten fascinating chapters summarize the origins of grammatical traditions of European vernaculars, *viz.* Italian (chapter 102), Portuguese (chapter 104), French (chapter 105), English (chapter 106), German (chapter 107), Dutch (chapter 108), Slavic (chapter 109), Celtic (chapter 110), and Finno-Ugric languages (chapter 111) — of course, more detailed investigations of all these are available elsewhere, and the length of the texts here provided does not permit any new insights for specialists in the languages treated. The section is a natural basis for the next, which is devoted to the period of academies and normative grammars, consolidating the results of the early language-planning activities, in which Italy (chapter 112), France (chapter 115), Spain (chapter 116), and Portugal (chapter 117) are sketched, but the failure of such projects elsewhere, as in Britain and Germany, is only hinted at. Normative tendencies ARE found in other countries, of which the Low Countries (chapter 120),



Russia (chapter 122), Czechia (chapter 122), Poland (chapter 123), Hungary (chapter 124), and Malta (chapter 125) are treated, chapters in which certainly otherwise inaccessible information is summarized. It would have been good to supply a comparative chapter correlating the differences of development with the diverging sociohistorical conditions.

After 1500, the view of European linguists widened to include various “exotic” languages of the Americas, Africa, and Asia. Accordingly, two introductory chapters summarizing the contributions of travellers and missionaries are followed by three chapters on South America (on Nahuatl, Quechua, and Guaraní) and two on North America, and three on Africa and Asia. Finally, theories of grammar and language philosophy between 1600 and 1800 are treated in chapters 137 to 141 — with five chapters not being sufficient to do justice to the discipline in a period that saw a great variety of innovative models. Three chapters deal with ideas on the origin of language(s) and early collections of parallel texts (such as translations of the Lord’s Prayer). Lists of references are appended to the individual chapters, but indexes will only be found in volume 3 to cover the entire set.

A large number of similar surveys have been published in the past few years, illustrating the fact that the historical study of the discipline has seen a somewhat unexpected revival. Is there, then, a need for such a huge enterprise, and what are the distinctive features of the new handbook that explain or justify the undertaking? One reason is of course the series character. The general editors have delayed the treatment of the topic until number 18, previously producing handbooks devoted to 17 other disciplines, and must have felt that one on the history of the language sciences was now in order to complete the survey. Other characteristics immediately come to mind:

1. The size allocated to the project has allowed authors to deal with more marginal topics, and to discuss the more central ones in unprecedented detail.
2. The range of aspects treated in the chapters on Japanese, Sanskrit, Greek, Latin, and Arabic gives the respective sections an almost monographic character.
3. The parallel structure of these sections — and of others devoted to language teaching and the emergence of European standard languages — facilitates, in principle, very welcome comparisons, especially where parallel developments (in Western Europe) or contrasts between unrelated cultures and the independent growth of concepts and theories are concerned.
4. The inclusion of most recent research obviously makes the handbook more reliable on new methods and insights.

A final evaluation, especially a comparison with other handbooks such as those [edited] by Auroux (1990–2000), Hymes (1974), Koerner and Asher (1995), Lepschy (1994–), Schmitter (1987–), and Sebeok (1975) will be possible only when the three volumes of the HSK set are published. However, there are a few doubts as to whether the aims mentioned above have been fully achieved (or are in fact achievable):

1a. The discussion of marginal topics such as Ugarit in chapter 2 facilitates no proper comparison with other chapters; it also leaves open how many more of the smaller languages could possibly have been included but are not.

2a. Similarities or differences and gaps in the parallel sections make interpretations risky, since the chapters were obviously not circulated among authors — which would have made explicit comparisons, cross-references, and evaluative comments possible. As the texts stand now, readers' conclusions may be based on chance omissions or other idiosyncrasies.

3a. Some pan-European developments provide an excellent chance for seeing cross-connections and influences (whether derived from a common Latin source or lateral transmission, such as French or German theories taken over and adapted to national conditions). Some of this may be expected in the two forthcoming volumes, but it is already clear that a few chances have been missed so far. To name two such cases:

Academies were established in various European countries to provide a reference for linguistic standardization and guidance in cases of divided usage. These institutions and their impact have been treated for Italy (chapter 112), France (chapter 115), Spain (chapter 116), and Portugal (chapter 117) — but the failure of such proposals is mentioned only in passing for Germany and Britain, and not even hinted at for other countries (including the US). Second, certain streams of grammatical traditions have affected individual languages, such as early models based on Donatus and Priscian, and on Ramus in the Renaissance. These are taken up, with varying degrees of completeness and depth, in the national chapters, but this mosaic does not add up to a coherent picture. It might have been worthwhile considering whether contrastive summaries could have been supplied — as they have for language teaching, where two chapters by Schröder sum up the European situation.

4a. The editors appear to have done an excellent job in making authors update their contributions, as quite a number of references to books published in 1999 testify. However, the project has had, quite naturally, a long gestation period (I know of a chapter for volume 2 submitted five years ago), and so it might seem fair to authors to state at the end of papers when the text was handed in. The situation is particularly awkward

with several competing projects (see above) being still under way or having been completed only last year.

The editors have also been successful in restricting the length of chapters. In contrast to preceding HSK volumes, there are no book-length contributions (although chapters vary between two and seventeen pages according to the wealth of the data covered). Contributions are again in three languages: 74 in English (= 51.7%), 36 in German (= 25.2%) and 33 in French (= 23.1%); this proportion is to be kept up in the two successor volumes. This decision in favor of trilingualism is sound, since the specialized readers of the book can be expected to master the three languages. It is quite another question whether German texts (possibly the least understood nowadays) must exhibit the extreme syntactic complexity or pseudo-literary allusiveness of chapters 143 and 112, which makes these chapters difficult to comprehend even for native speakers (however much, it could be argued, they exhibit hallmarks of the dated German style of scholarly exposition). The book has been edited and printed with remarkable perfection. A cursory check yielded fewer than twenty typographical errors, which proves the high technical standard of production.

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