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REVIEWS

Herbert H. Clark, *Using language*. Cambridge: Cambridge University Press, 1996. Pp. xi + 432.

Reviewed by ROBYN CARSTON, University College London

We are told at the beginning of this book that using language is like dancing a waltz, playing a piano duet or making love, in that they are all kinds of joint action. The key word in this book is, without doubt, 'joint'; it occurs in at least the following phrases, all of which have an important role to play in the account: joint activities, joint actions, joint acts, joint events, joint closure, joint construals, joint projects, joint effort, joint commitments, joint focus of attention, joint perceptual experience, joint salience, joint knowledge, joint management, joint purpose, joint pretense, joint solutions.

This central thesis that 'language use is a form of joint action' might strike many linguists as simply wrong. For instance, some might think that language is used in thinking, which is surely not a JOINT action, in fact not usefully thought of as an ACTION of any sort; then there is talking aloud when alone, which one may do for a variety of reasons: to rehearse a speech, to see whether something one has written really does express the intended ideas, to divert oneself from tormenting thoughts, to enjoy the sounds of a poem, for the sheer delight of belting out the words of 'Oh come all ye faithful' while in the shower, etc. For those who construe 'language use' as encompassing these possibilities, the title of the book is misleading: 'language' here does not entail something that has such linguistic properties as phonology, morphology and syntax, because it includes non-linguistic gestures such as pointing, nodding, eye-gaze, and certain types of smiling, frowning and other facial and bodily movements, and the 'use' at issue is entirely communicative. Face-to-face conversation is taken to be the basic type (so it is the primary focus of the book), and all other communicative uses of linguistic entities (like letter-writing, story-telling, interviewing, organised discussion, etc.) are taken to be secondary and derivative. What the book is really about is what Clark calls 'signaling' (and others call 'ostensive communication'); that does indeed include many non-linguistic behaviours and exclude many (non-communicative) employments of linguistic forms.

This volume is the culmination of over a decade of work by Clark and his colleagues, in which the view of language use as a joint action embedded within broader social activities has become more and more the central focus. To establish this, Clark deploys an interesting conjunction of scholars and disciplines, welding the philosophical views of Lewis and Austin to the empirical inductive observations of conversation analysts, such as Sacks,

Schegloff and Jefferson, and to the work of J. B. Bavelas (probably unfamiliar to most linguists) on ostensive gestures, or what she calls 'nonverbal linguistic acts', a concept that is clearly reflected in Clark's account of 'language' use. Ideas from the field of pragmatics, a concept that for many is instantly activated by talk of 'language use', has a very meagre presence: the work of Sperber & Wilson and other relevance theorists, of Bach & Harnish, Larry Horn, and Georgia Green is essentially passed over, and Levinson enters the scene only in his most social and least linguistic manifestations. Inadequate and misguided approaches to language use are characterised by Clark as having most or all of the following properties: they concentrate on the PRODUCTS of language use rather than on what people do with language, they have grown out of the generative grammar tradition, they ignore the central role played by non-linguistic elements of signals, they give insufficient attention and weight to the crucial concept of context, the speaker's meaning (communicative intention) is given an inappropriate primacy over, and autonomy from, the hearer's recognition of it and the collaborative processes of speaker and hearer. Whether or not this characterisation does pick out an approach to language use (Clark names no names), the listed deficiencies provide a useful way of highlighting the properties of his own account, which are in clear contrast to them.

The view of language and communication adopted in the book is pure David Lewis; the key concepts are 'coordination problem', 'convention' and 'common ground'. The goal of language use (= signaling) is an increase in the interlocutors' common ground, but to achieve this they have to solve a participant coordination problem (that is, reach a joint construal of the signal), and one of the most important devices for achieving the required coordination is that of convention. As well as conventions of use (e.g. different types of greeting in different cultures) and conventions of perspective (e.g. the difference between 'first floor' as used in Britain and in North America), the lexical entries and grammatical rules that make up a linguistic system are conventions. The following gives the flavour: 'As Lewis argued, the phonological, lexical, morphological, syntactic, and semantic rules of a language – its grammar – constitute a conventional signaling system. They describe regularities of behavior – what English speakers regularly do, and expect others to do, to achieve part of what they intend to do in using sounds, words, constructions, and sentences for communication' (77).

In understanding an utterance, these (and other, nonlinguistic) conventional devices interact with nonconventional coordination devices, which include perceptual salience (of objects in the physical context, for instance), explicit agreement (on how a term is to be used, for instance), precedence (in referring to something in a certain way, for instance), the ultimate criterion for their use being joint salience. The factors which necessitate this interaction of the conventional and the nonconventional include ambiguity, indexicality, novel uses of words and structures, and what Clark calls layering, which

arises in various 'nonserious' uses of language, including fictions, tropes like hyperbole and irony, teasing and ostensible communicative acts (e.g. pretend invitations). In short, it is those factors which are generally considered to comprise the domain of pragmatic theory. There is, however, no pragmatic criterion at work in this picture of comprehension; Grice's maxims and other communicative principles, characterised as reductions of Grice's system, such as Sperber & Wilson's Principle of Relevance, are dismissed as misguided. Elements of Grice's Cooperative Principle are taken up and recast in terms of the joint purpose of conversationalists, and the various joint actions they engage in in arriving at a joint construal of a given signal. Among the various coordination devices involved in achieving this, a crucial role is played by a range of metacommunicative acts, in what can be thought of as track 2, parallel with the communicative acts in track 1. The function of these metacommunicative acts, which include acknowledgments like "uh huh", "yeah" or a nod, is to establish the mutual belief that signals have been understood well enough for current purposes (that is, to bring about joint closure on the joint actions in the communicative track).

Signaling is an act by which one person means something for another. While signaling encompasses the Gricean concept of speaker meaning as involving a complex (reflexive) intention on the part of the speaker, one of Clark's central contentions is that it should not be viewed in the listener-free sort of way that Grice, Searle and others adopt. Rather, it is to be seen as typically a joint act located, together with other joint acts, within a broader joint activity such as two people hanging curtains together, planning a holiday with a friend, a buying-selling transaction, a car-driving/road-navigating interaction, or such intrinsically communicative activities as discussing the latest news, having a gossip or making conversation at a party. Signals (including linguistic ones) are devices for coordinating actions at various stages of these activities; they provide a shared basis for adding to common ground some information which will further the current goals of the participants. On this construal, signaling involves both the speaker's meaning intention and the 'discharge' (or fulfilment) of that intention through the hearer's recognition of it. In fact, any signaling act involves a ladder of four levels of (cotemporal) joint action, which are in an upwards causal relation: (a) the joint action of the articulatory/gesticulatory behaviour of the signaler paired with its perception by the addressee; (b) the joint action of presentation of a particular signal (e.g. a linguistic expression and/or a bodily gesture) and its identification by the addressee; (c) the signaling that p and the addressee's recognition that p is what is meant (an action which must ultimately issue in a joint construal of the signal's meaning); (d) the proposal of a joint project and the addressee's consideration of the proposal (followed, in most instances, by uptake). Minimal joint projects (manifest in Schegloffian 'adjacency pairs') include the following: greeting reciprocation, the transfer of information (manifest in an answer to a question), instigating

a behaviour (manifest in, for instance, an addressee sitting down when requested/advised/ordered/invited to do so), settling on a view (manifest in an addressee assenting to a speaker's assertion). This fourth level, which goes beyond the concerns of most pragmatic theories (which focus on level 3), is crucial on Clark's view, because it provides the rationale for signaling, which is to further the joint activities people are engaged in at the time and to advance the common ground of the social group, thereby increasing the possibilities for subsequent joint activities.

Most signals are the result of a fusion of three methods of signaling: 'describing-as', 'indicating' and 'demonstrating', which respectively employ the symbolic (linguistic expressions being the paradigm case), the indexical (e.g. pointing) and the iconic (e.g. manual, facial and/or vocal gestures, including intonation). Here's a simple example: 'George sees Helen and says "Hello." He uses his voice and eye gaze to *indicate* himself as speaker, Helen as addressee, and now as the time of greeting. He uses his smile, open eyes, and magnified intonation to *demonstrate* his enthusiasm. Helen, in turn, not only interprets each of these methods, but integrates them to understand him as meaning, roughly, "I, George, now greet you, Helen, enthusiastically". The point is this: "Hello" is treated not as three PARALLEL signals with separate interpretations, but as a SINGLE signal with a unified interpretation' (185).

It is through this discussion of the composite nature of signals that Clark is led to his revision of the concept of 'language' as *language*$_u$ (that is, language in use), to be distinguished from *language*$_s$ (that is, language structure), language$_s$ providing but one of the entwined strands of a signal. Language$_u$ encompasses all manifestations of communicative (and meta-communicative) intent, including, for instance, the displacement of tokens on a board by the players in a game of chess. Two questions arise for me, the first one rhetorical: (1) Why use the word 'language' here at all, when it is signaling which is the subject? (2) Is it clear that to study the use of language$_s$ is as deeply mistaken an endeavour as Clark would have it? To do so would, of course, be to abstract away from the full complexity of communicative performances at a different point from Clark, carving out a narrower domain, but one which might enable a deeper dig, looking at (some of) the mechanisms that underlie and enable signaling behaviour. Interestingly, he himself points out that his three signaling methods, of which the linguistic is one, involve different cognitive resources: a mental lexicon and grammatical rules for describing-as, a mental representation of current spatio-temporal surroundings for indicating, and memory for appearances for demonstrating (184). This is, however, the only explicit mention of cognitive capacities and systems in the book.

The reservations I've indicated notwithstanding, the meticulous and inexorable way in which Clark builds his account, brick by brick, is very impressive and the final, tightly interlocking structure coheres in a satisfying

way. Each of his many 'principles' is supported by a wealth of detailed discussion and illustrative examples; the examples are authentic, taken from, among others, the London-Lund corpus, and annotated to indicate such features of speech as tone unit, pauses and overlapping utterance. He uses apt literary quotes and amusing anecdotes to engage the reader. Several of the chapters in which he presents existing views are interesting and useful independent of their role in furthering his project. For instance, the chapter on that tricky notion of 'common ground', a notion that seems inevitably to arise in some form or other in any serious model of human communication, sets out three different conceptions of common ground: Lewis's original 'shared basis' view (which he adopts), the reflexive definition and the iterated propositions representation that emerges from it. Clark's discussion of the psychological plausibility (possibility even) of these conceptions is judicious. Similarly, his summaries of Austin (whom he considers a forebear of his 'language as social (hence joint) action' view), of Grice and of Searle (of whom he is more critical – for what he sees as their autonomous speaker orientation) and of Peirce on symbols, indices and icons, are excellent, clear and succinct introductions to these people's ideas.

In reading this book I participated in a joint action with Herb Clark in which I struggled to coordinate with him; our initial common ground was somewhat sparse, apparently not even containing a meaning for the word *language*; I am not sure that we have reached a joint construal, though we have expended a deal of joint effort. I am sure, however, that I gained a good range of cognitive effects, and that all other readers will too.

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Jaap van der Does & Jan van Eijck (eds.), *Quantifiers, logic, and language* (CSLI Lecture Notes **54**). Stanford: CSLI Publications, 1996. Pp. vii + 424.

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The volume under review emerged from a workshop on quantification held in the early nineties at the Institute for Logic, Language and Computation (ILLC) at the University of Amsterdam. It contains an introductory essay and sixteen articles ranging from pure logic to natural language semantics, with a significant number attempting to address both. The idea implicit in the volume is that it is enlightening to look at the topic of quantification from all of these angles at once. It thus represents a snapshot of the major line of research within semantics opened up by Barwise & Cooper (1981). I believe

that the most useful way to evaluate the volume is in light of this point of view, i.e. in terms of whether it indeed supports the idea that a community of scholars comprised of logicians and linguists of this sort can move the field forward in a way that neither group could alone. For reasons that will appear below, my final evaluation of the volume in these terms is mixed, as some papers are much more successful than others, though on the whole the volume does a successful job of implicitly building a lattice of interconnections which is thought-provoking, though not yet near a final, clear pattern.

There are a number of negative things which may be said at the outset about the volume as well. If one does not look at the book in the way suggested above, it is difficult to come up with any kind of coherent take on it. The papers included are quite diverse, and in terms of their explicit content most are interesting primarily from the point of view of how they fit into the authors' other work, and not for the most part in how they relate to one another. Thus, most readers are only likely to find a few papers of particular interest to them, unless they can commit to reading the whole thing with the hope of getting a feel for the big picture. Much worse, some of the papers are very poorly edited, and many have at least a few annoying errors. There are missing example numbers and references, even within the text (consider 'But the question can be answered positively by introducing branching of generalized quantifiers, cf.' in Martijn Spaan's paper (282)), and critical mistakes in figures (e.g. Frans Zwarts' Figure 2 (398), where an expositorily crucial illustration is plainly not as described by the text). Problems of this sort make the most afflicted papers quite difficult to read, and undermine the usefulness of the book as a whole as something which can help one understand the fruits of a logical-linguistic investigation of quantification.

Now we should turn to a bit more detail on the actual linguistic content of the volume. Given space constraints, I would like to dismiss immediately (but without prejudice) from consideration a couple of papers which are sufficiently logic-oriented as to be likely of little interest to readers of this journal: the contributions of Kees Doets, Michiel van Lambalgen and Dag Westerståhl. The contributions of William Ladusaw, Henriëtte de Swart and Jacob Hoeksema are the most linguistically oriented of the papers. Because they will be of most interest to readers of this journal, I will discuss them in most detail. Doing so will also allow me to introduce in a gentle way the types of logical concerns which are the focus of most of the authors. Then I will spend the remainder of this review attempting to give a general feel for, and evaluation of, the rest of the volume, with its emphasis on using logical notions to inform linguistic analysis and linguistic facts to bring to light a variety of logically relevant properties of quantifiers.

William Ladusaw's paper 'Configurational expression of negation' makes an interesting, potentially far-reaching proposal about how negation is expressed in sentences. He bases his analysis on data from negative concord

languages, those where a single instance of sentential negation may be expressed in multiple places within the sentence, in particular in the inflectional system and on certain noun phrases and adverbs. For example, consider his (1) (203):

(1) Mario non ha parlato con nessuno di niente. (Italian)
M. NEG has spoken with nobody of nothing
'Mario hasn't spoken with anybody about anything.'

He suggests that the force of negation is actually a feature of the clause itself, and is not directly introduced by any particular morpheme; the morphological realization of negation serves simply to express or license the negativity of the clause. Negative concord languages differ from non-negative concord languages in the rules for how clausal negation is reflected on different types of elements. In particular, a negative concord language will realize negative features (via 'n-words' like Italian *nessuno* 'nobody') on any indefinite NP minimally within the negated clause. Ladusaw outlines this analysis, in a quite sketchy way, after he has made a powerful case against the commonsense view, represented here by the work of Haegeman & Zanuttini (1996) and Rizzi (1982), that clausal negation is an ordinary piece of meaning compositionally introduced into the clause by a negative word. The view assumes that the meaning of a negative phrase comes about from some structure of the form [Neg X], where Neg negates X, though in the face of negative concord data this view is pushed to postulate logical forms which differ substantially from surface structures.

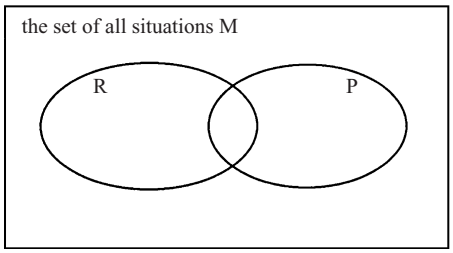
The fundamental insight of Ladusaw's proposal is that the meaning of an element, such as a negative indefinite, may not be entirely self-contained, but rather may come about via configurational relationships within the clause. If this idea is correct, it asks us to rethink many of our assumptions about semantics, including those represented in the other papers in the volume. It suggests that if one is to study the 'meaning' of a quantifier, one needs to carefully distinguish the semantic contribution of the quantifier itself from the contribution of the external relationships in which the quantifier takes part.

Henriëtte de Swart's paper 'Quantification over time' also addresses questions which are primarily of linguistic interest. Its central goal is to argue that quantificational adverbs (Q-adverbs, e.g. *always*, *sometimes*, *most often*) are simply the counterparts of determiner quantifiers (*all*, *some*, *most*) in the temporal domain of 'situations'. That is, *always* is argued to mean "for all situations", in parallel to *all*'s meaning "for all individuals". Such an account is not novel, as de Swart notes, but she does a useful job of placing it into the context of a broader understanding of quantification. For instance, she attempts to define the logically permissible class of Q-adverbs in terms of the constraints Conservativity, Extension, Quality and Plus. The first two are familiar from work on previous Generalized Quantifier Theory,

and can be understood informally in terms of the following example and diagram:

(2) When it rains, it **Q-adverb** pours.

(3)



R = the set of raining situations
P = the set of pouring situations

Conservativity says that, given a domain M, (2) only depends on R and R ∩ P (if Q-adverb is *always*, it says that R = R ∩ P). Extension says that enlarging M won't affect the truth of (2), unless it enlarges R or P. Quality is a little trickier, but essentially says that all that matters for (2) is the number of elements in M, and the temporal relationships among them. And finally, Plus, which is de Swart's main contribution to this point, says roughly that if you thought (2) was true (likewise, false) but you discover a new element x of R, it is still possible that (2) is true (false), so long as x is in the right one of R–P or R ∩ P. So, if you think *When it rains*, *it always pours* is true, and then it rains again, you'll still think it's true so long as this new raining is a pouring (in R ∩ P); or, if you think *When it rains*, *it never pours* is true, and then it rains again, you'll still think it's true so long as this new raining is not a pouring (it's in R–P).

What is interesting about this project is how she attempts to frame the discussion of logical properties in terms of both what's common with quantifier theory in general and what's special to temporal quantifiers. For instance, Quality is a deviation from what we get with nominal quantifiers, since the latter don't care about temporal relations. This said, it must be pointed out that there are significant doubts concerning whether we can indeed consider Q-adverbs to be quantifiers over situations. The well-known problems arise from sentences like (4).

(4) When a bishop meets a man, he always blesses him.

According to the situation analysis, this should mean "every situation where a bishop meets a man is a situation where a bishop blesses a man" or "every situation where a bishop meets a man is a situation where the bishop blesses the man". In a case where the other man is himself a bishop, these interpretations suggest that there need only be one blessing, contrary to intuition (i.e. in reality each bishop must bless the other). Drawing on other

literature on indefinites, de Swart proposes a novel interpretation along the lines of "every situation where a bishop meets a man is a situation where every bishop who meets a man in that situation blesses him in that situation" (317). This correctly yields two blessings in the circumstance mentioned above. Nevertheless, it does not appear to yield the right result in all cases, as with (5).

(5) When a bishop meets a man, he usually blesses him.

This does not have the required reading, as can be seen by considering the College of Cardinals situation, where the vast majority of instances of a bishop meeting another man are cases where this other man is also a bishop. The sentence does not then require that most meetings involve mutual blessing. Rather, it means that most bishops who meet a man bless the man. But this is quantification over bishops, not quantification over situations. Hence, the assumption underlying de Swart's interesting discussion of temporal quantification can be called into question.

Jacob Hoeksema's 'The semantics of exception phrases' is equally empirically oriented, concentrating on sentences like (6) (his (12) (d), (148)).

(6) Everyone smiled, except Felix.

These constructions show many interesting syntactic and semantic properties, and I will not go into the literature they have given rise to. (It's small enough that one can begin with recent works like this paper and Fintel (1993), and cover it easily working backwards.) Of concern in the present context is what the paper has to offer towards a general understanding of the logical and semantic properties of quantification. Though this is not made explicit, it seems to me that the main thing here is the 'substitutional' analysis of quantification which Hoeksema proposes. The idea is that (7) would have an analysis along the lines of (8).

(7) Everyone smiled.

(8) $\cap \{\text{smiled}(x) \mid x \in E\} = \text{True}.$

Here, $\{\text{smiled}(x) \mid x \in E\}$ is the set of propositions of the form *x smiled*, with x an individual, and (8) as a whole is the conjunction ("meet") of these propositions: *a smiled and b smiled and* .... Given this, (6) is then interpreted as (9), where the proposition that Felix smiled is removed from the set.

(9) $\cap (\{\text{smiled}(x) \mid x \in E\} - \{\text{smiled}(\text{Felix})\}) = \text{True}.$

This type of account lets us understand 'pied-piping' examples like (10) (his (72), (167)).

(10) Except for the parents of John, we talked to the parents of every student.

The quantifier which is being given an exception here is *every student*, but the *except* phrase seems to exclude from consideration John's parents, not John. Hoeksema can account for this by saying that what is really being excluded

from consideration is not an individual, but a PROPOSITION, that we talked to the parents of John. It would be interesting to investigate whether Hoeksema's analysis could be reproduced within a more traditional treatment of quantification.

Thus far we have concentrated on three of the most empirically oriented articles in the volume. They have, however, shown us how some of the more abstract issues in quantifier theory can pertain to linguistic issues. The bulk of the remaining papers do not focus on giving detailed analyses of natural language constructions, but rather look primarily to relate logical and linguistic issues. It is impossible to discuss these papers in the detail of those which I dealt with above, given the amount of technical groundwork which would have to be laid. Nevertheless, I hope to give a feel for some of the most significant of these papers.

The introduction 'Basic quantifier theory', by Jaap van der Does & Jan van Eijck, is the most explicit about relating logical and linguistic issues. The advertised goal of the introduction is to 'make the volume accessible to both linguists and logicians' (vii), but from this point of view it is only moderately successful. The coverage of the linguistic issues is quite sketchy and misses some central topics of concern in later papers (e.g. plurals, focus). On the other hand, the background on logic is very nicely done, and can serve as an overview of the field for anyone with a basic knowledge of logic. Nevertheless, there is not really enough material here to get the average linguist through all of the papers that follow.

Johan van Bentham's 'Quantifiers in the world of types' is a very interesting and comparatively accessible discussion of extensions of quantifier properties like Monotonicity and Conservativity in a more general environment. He asks what properties of larger phrases correspond to these properties, and how the larger expressions inherit them. The early discussion is pretty accessible, and much more focused on making the logical results relevant to the linguist than the introduction was, though the road gets more difficult by the end.

Jan van Eijck's 'Quantifiers and Partiality' has a similar orientation to van Bentham's paper. The links from logical analysis to natural language are made clear, and are set out in the first section. The paper focuses at first on transferring established quantifier properties (Conservativity, etc.) into a partial setting, where models may render sentences true, false, or neither. Based on this, the most interesting aspect of the paper comes in discussion of the logical consequences of information growth, that is, what type of semantics for quantifiers gives rise to an appropriate logic when we allow for sentences which were previously neither true nor false to become one or the other.

Another paper with a similar point of view to the last two is Edward Keenan's 'Further beyond the Frege boundary'. Fregean quantifiers are those which can be defined by the composition of unary quantifiers, where

unary quantifiers are roughly those which bind one variable in the familiar way. He then looks at types of quantification in natural language that may be non-Fregean. Some of these are more convincing than others. Quantification by mutually dependent NPs is convincing, as in (11) (his (5) (a), (183)).

(11) Different people like different things.

Others are less so. Keenan is careful to acknowledge that the categorization of a particular phenomenon as 'non-Fregean' depends on one's syntactic and pragmatic analysis as well as the semantics, and so isn't easily settled. Nevertheless, the tests he provides for non-Fregean status (given a broader analysis), and the number of at least plausible cases he brings up, do suggest that less logically-oriented linguists should consider the theoretical ramifications of allowing non-Fregean quantification.

One other paper in the volume is roughly in the same spirit as the last few, Frans Zwarts' 'Facets of negation'. He considers the interactions between quantifiers and what he labels as different varieties of negation: sentential negation, predicate negation and verb negation. For example, he builds a logic which explains the synonymy of (12), with sentence negation, and (13), with predicate negation (399).

(12) It's not the case that every cow moos.
(13) At least one cow doesn't moo.

It's not clear to me, however, what role the 'varieties' of negation play here. What seems to be at issue is the equivalence of sentences alike except for the scope of negation with respect to some NP. The relevant difference between (12) and (13) is that the former gives negation wide scope with respect to the subject, while the latter tends to give it narrow scope. Any other means of indicating the same scopal relations, such as intonation, would give rise to the same pattern, without allowing the appeal to different types of negation. Zwarts also uses his framework to provide an intriguing account of Negative Polarity licensing, one which is closely related to some of van Bentham's ideas in the paper discussed above, but he appears to find it untenable in the end (419).

For reasons of space, I will not have anything to say about the remaining papers. They fall within the family of approaches represented above, and to the extent that they are successful add weight to the idea that the logical and empirical approaches to quantification represented in the volume are indeed complementary.

In sum, this volume provides a useful and stimulating picture of the state of the relationship between linguistic semantics and logic in a particular place at a particular point in time. Some of the analyses are of course more convincing than others, and many will be rough going for those not of a highly formal bent. Nonetheless, the papers as a group do make a contribution to the ongoing line of research of which they are a part.

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T. Alan Hall, *The phonology of coronals*. Amsterdam/Philadelphia: John Benjamins, 1997. Pp. x + 176.

Reviewed by EDWIN G. PULLEYBLANK, University of British Columbia

In *The sound pattern of English* (Chomsky & Halle 1968, hereafter SPE) [±coronal] is defined (304) as a binary opposition between 'sounds that are produced with the blade of the tongue raised from its neutral position' and 'sounds ... produced with the blade of the tongue in the neutral position'. In spite of the reference to part of the tongue, it is used as a way of dividing the passive upper surface of the oral tract into contrastive 'places of articulation'. In more recent articulator based theory coronal (front of the tongue) is treated, along with labial (lower lip), dorsal (body of the tongue), and, for some, also radical (root of the tongue), as one of the three or four active articulators under an abstract place node. In this sense it is by definition privative or unary – [−coronal] can only mean lack of involvement on the part of the front of the tongue in the articulation of a sound and cannot contribute to the actual production of that sound. The possibility exists, however, of combining the action of two or more articulators in producing a single sound.

In his introduction T. Alan Hall refers to various properties of coronal segments which have been discussed in previous studies that he is not going to deal with, including the relations between coronal consonants and front vowels on which the present reviewer has had something to say (Pulleyblank 1989), and declares his intention of focusing mainly on the fact that the number of subplaces among [+coronal] consonants exceeds that of the subplaces among dorsal and labial consonants. By adopting the three-way classification, dorsal, coronal, and labial, he is evidently committed to an articulator based model and he expressly acknowledges this at one point (30). It seems to me, however, that he has not fully come to grips with the

implications of such a commitment. In his discussion of palatalization as a process he ends up by having to treat the feature [±back] as located under both dorsal and coronal. He acknowledges that this is contrary to the usual assumption that a feature [F] should be located under a single node but excuses himself by alleging that other scholars have also sometimes violated this principle and suggesting, without further discussion, that 'its very existence … needs to be rethought' (84).

Something clearly does need to be rethought. A full discussion is beyond the scope of a short review. It seems to me, however, that one source of misunderstanding that has led to the dilemma in which Hall finds himself is failure to take account of the fact that an articulator based analysis implies the possibility of combining the action of more than one articulator in a single segment. Another is that there are basic differences in the articulation of vowels and consonants in this respect. The representation of secondary articulations is a key question. In the SPE system palatalization (307) and labialization (309–311) of labial or coronal consonants simply add [−back, +high] and [+round] respectively to a single feature matrix. In an articulator based system, however, if we assume that these features are dominated by the dorsal and labial articulators respectively, adding them to coronally articulated consonants implies the participation of a second articulator, making a complex segment. An early discussion of such segments was that of E. Sagey (1986). She was particularly concerned to analyse the structure of doubly articulated stops and nasals such as [kp] and [ŋm] found in West African languages but she also recognized that consonants with a vocalic secondary articulation were much commoner worldwide. She regarded those also as doubly articulated, distinguishing the difference in degree of occlusion of the two articulators in such cases by the rather awkward device of drawing an arrow from the root to the major articulator node, that is, the one that was [+consonantal] in terms of stricture.

My own solution, proposed already in Pulleyblank (1984) and developed further in Pulleyblank (1989) is to posit separate [+consonantal] and [−consonantal] roots attached to the same timing slot, on the assumption that the vowel articulation will be transparent to the consonantal articulation with the possibility of emerging phonetically after the consonant when syllable initial or before the consonant where syllable final. This seems to fit the observation of Ladefoged & Maddieson (1996: 385) that, because approximant articulations lack the same landmarks of closure and release that are present in the case of obstruents, it is difficult to demarcate phonetically the difference between a consonant with secondary articulation and a sequence of a consonant and an approximant. I wonder, for example, whether one can measure significant timing differences between labiovelar [kʷ] in Cantonese and the sequence [kw] in Mandarin. The decision to call the former a single segment and the latter a sequence of two segments is primarily phonological, the fact that sequences of consonant and semivowel

are otherwise excluded in Cantonese while not only [w] but also [j] and [ɥ] can occur after coronals as well as velars in Mandarin.

Hall seriously misrepresents my position when he includes me without elaboration among those who 'argue that front vowels are coronal and back vowels dorsal' (149). What I proposed in my 1989 paper was that front vowels combine dorsal and coronal articulation while back-rounded vowels combine dorsal and labial. The essence of the problem lies in a mismatch based on the anatomy of the oral tract between the 'places of articulation' of the three corners of the quasi-universal i-a-u triangle and those of the three most universal consonant types, labial, dental and velar, represented by the stops p-t-k. While these consonants correspond to the points at which it is easiest to maintain a complete closure of the oral passage above the larynx either by closing the lips, pressing the front of the tongue against the back of the teeth or the alveolar ridge, or pressing the body of the tongue against the roof of the mouth (Lieberman 1991), the three extreme vowels represent acoustic maxima in terms of their formant structure and require more complicated manoeuvres which do not completely block the oral tract but vary the relative length of its anterior and posterior portions, partly by approaching the tongue to different points along its upper surface and partly by extending or retracting the lips.

All vowels have to be formed in the posterior portion of the vocal tract, the 'vowel space', in order to have an appropriate formant structure. Thus, a so-called labial vowel, [u], as well as the corresponding non-syllabic semivowel or glide, [w], combine protrusion of the lips with raising of the body of the tongue towards the velum. Without the concomitant dorsal action we get a labiodental approximant [ʋ], which can occur as syllabic [ʋ̩], for instance in the Bai language of South China, but which lacks the resonance of a true vowel. [i] similarly requires pushing the tongue as far forward towards the hard palate as is consistent with vocalic resonance but this cannot be as far as the point of contact of a typical dental or alveolar [t]. It is possible to make a stop consonant [c] in the so-called 'palatal' region but it is less stable than a [t] or a [k] and tends to become an affricate like alveolopalatal [tɕ] in Mandarin Chinese or palatoalveolar [tʃ] in English. Diachronically, and often synchronically in morphophonemic alternations, such 'palatal' consonants typically arise through the assimilatory effects of a high front vowel [i], or more often its nonsyllabic variant, the glide [j], on an adjacent consonant, velar, dental or even in some cases labial. Because even non-high front vowels require forward movement of the tongue, velars, though probably not dentals or labials, can also be assimilated to a 'palatal' (including alveolopalatal or palatoalveolar) place of articulation in the presence of non-high front vowels, as in the case of French *château* from Low Latin *castella*. Conversely, because palatalization of dentals involves raising the front of the tongue, these can sometimes be palatalized in the presence of even a high back vowel (Bhat 1978).

It is this sort of evidence that suggests that front vowels should be regarded as complex, combining gestures of the coronal and dorsal articulators, in the same way that, less controversially, back rounded vowels combine those of labial and dorsal. (I would now replace dorsal with dorsal and/or radical in this formulation but limitations of space will not permit developing this idea here.) In this formulation, the SPE features [−back] implying forward movement of the body of the tongue and [−anterior] implying upward movement of the front of the tongue are redundant. Both [−back] vowels and [−anterior] consonants can be regarded simply as the resultants of combined dorsal and coronal gestures.

Allowing for the possibility of combining dorsal and coronal articulation will help to dispose of other problems besides that of having to locate the feature [±back] separately under both [coronal] and [dorsal]. Hall is much concerned with how to define consonants classified as palatal and alveolopalatal in the IPA chart and finds himself forced to the rather surprising conclusion that the so-called 'palatal' stops [c] and [ɟ], as well as the nasal [ɲ] and the glide [j] are really alveolopalatal and [+coronal], leaving the fricatives, [ç] and [ʝ], as the only true 'palatal' consonants, [+dorsal] according to his formulation. His argument for these conclusions is based on cases in which, allegedly, [c] and [ɟ] form natural classes with coronals, while [ç] and [ʝ] form natural classes with velars. There are, however, cases in which even palatoalveolars can reveal dorsal affinities, for example, the fairly recent change of [ʃ] to [x] in Spanish. Compare French Don Quichotte for Don Quixote. This can be explained as deletion of the coronal link under the place node leaving the dorsal link intact, just as, in the opposite direction, French alveolar [s] in *cinq* 'five' derived from earlier [tʃ] as in Italian *cinque*, can be explained as deletion of the dorsal link.

There are, of course, other contrasts among both [+anterior] coronal consonants (those with no dorsal component) and [−anterior] coronal consonants (those with a dorsal component as well) that need to be accounted for. Most notably, these include the contrast between retroflexes and palatals or alveolopalatals and the corresponding contrast between apical alveolars and laminal dentals or interdentals which are handled by the feature [±distributed] in SPE. This again correlates with a basic contrast between vocalic and consonantal articulations. A typical anterior coronal [t] with no additional dorsal component has an apical contact between the tip of the tongue and the back of the teeth or the alveolar ridge. The vowel [i], on the other hand, requires a laminal approach of the tongue to the hard palate and this naturally extends itself to palatal and alveolopalatal consonants which are the products of palatalization. There are, however, also languages that contrast laminal with apical contact for stops and nasals made farther forward in the dental/alveolar region and even more languages that have retroflex consonants made by an apical contact in the prepalatal region. Retroflex vowels (which are simply called coronal in the SPE

system – an unexplained exception to the general rule that this feature is not available for vowels), are also found in English and Chinese as well as the languages of South India.

Drawing on the fact that the most universal contribution of coronal articulation to vowels was frontness and that retroflex consonants and vowels had a marked antagonism to this feature, I proposed in my 1989 paper to substitute the feature [±front] under coronal for both [±back] under dorsal and [±distributed] under coronal. As Hall reports (48–49), other scholars have also noticed an affinity between retroflex consonants and back vowels and have proposed that [−distributed] be replaced by [+back]. He rejects this but I am not persuaded by his argument. There is no space for a full discussion here but I offer the following points. It is claimed that since Polish [ʂ] behaves as a 'hard' consonant before following vowels, it must have velarization as a secondary articulation like other 'hard' consonants and should be transcribed more accurately as [ʂˠ]. However, if [ʂ], like palatoalveolar [ɕ], simply combines dorsal with coronal articulation but with apical rather than laminal contact, so that it has a feature that is contrary to the frontness of an [i] vowel, this will account for its effect in causing such a vowel to retract. Certainly there is no basis for the claim that retroflexes in Chinese have velarization as secondary articulation. One of the phenomena that shows the dorsal component of retroflexes in Chinese is the replacement of Mandarin *er* [ɚ], which can occur both as a separate syllable and as a suffix, by back unrounded [ɯ] in Hankou and Changli dialects among others. This can be explained simply as the deletion of the coronal component of a doubly articulated [Coronal−front, Dorsal+high] vowel.

On one point I find myself in agreement with the author, the need for the feature [±strident] as a subcategory for coronal fricatives and affricates. Although he rejects the claim that it defines the difference between palatal [ç] and alveolopalatal [ɕ], which he regards as on either side of the boundary line between dorsal and coronal, he accepts it as necessary to distinguish dental or interdental [θ] from alveolar [s]. Not only do I think it should be retained as the distinction between [ç] and [ɕ], I would maintain further that one can have non-strident retroflex affricates. This seems to be the essential distinction between orthographic *tr* and *ch* in southern Vietnamese which have merged in favour of the latter in standard Hanoi dialect. English /tr/ can also be characterized phonetically as a non-strident retroflex affricate, even though it is phonologically a cluster, since it contrasts with /pr/ and /kr/.

To sum up: while I do not think that Hall has succeeded in analysing the issues that he raises in a satisfactory way, he has gathered together a large amount of relevant material and raised stimulating questions. In this way he has made a useful contribution to the ongoing enterprise of discovering how the human vocal apparatus is used to set up the digital contrasts that are used as the basis for language.

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William J. Hardcastle & John Laver (eds.), *The handbook of phonetic sciences*. Oxford: Blackwell, 1997. Pp. vii + 904.

Reviewed by H. Timothy Bunnell, duPont Hospital for Children and the University of Delaware

Professors Hardcastle and Laver (henceforth H&L) have undertaken the task of compiling a handbook 'as an advanced tutorial introduction for students with a basic grounding in phonetics who are interested in acquiring a foundation for independent graduate level research in the phonetic sciences' (1). This is a difficult task. The field has grown rapidly in recent years, spurred by technological advances which have provided powerful new instrumental techniques, unavailable, or even unheard of just a few decades ago. Today the field of phonetics is strongly interdisciplinary, merging aspects of linguistics, psychology, physiology, speech and hearing sciences, and borrowing important concepts, methods and tools from computer science, electrical engineering, and physics.

To cover this broad territory, H&L have assembled a book of twenty-six chapters in five parts, written by thirty of the most active and well-respected researchers in the field. It covers experimental phonetics (6 chapters), biological perspectives (three chapters), models of speech production and perception (9 chapters), linguistic phonetics (four chapters), and speech technology (four chapters). Of the total 904 pages, about 10 percent (93

pages) comprise an extensive bibliography. The resulting handbook is an excellent broad reference to the many sub-disciplines of modern phonetic science and a fine starting point for in-depth study of most current research topics in phonetics.

After a brief introduction which sets out the editors' views on the field and briefly introduces each of the papers, the handbook begins with a section on experimental phonetics. In the first chapter of this section, Maureen Stone provides an overview of the state of the laboratory art in acquiring non-acoustic data of phonetic relevance. This is followed by a chapter on the aerodynamics of speech by Christine Shadle and a chapter on acoustic phonetics by Osamu Fujimura & Donna Erickson. These chapters, together with a chapter by Kenneth Stevens in Part III of the handbook 'Models of speech production and perception', provide a very good overview of aerodynamic and acoustic factors in speech production and how these relate to phonetics. The breadth and linguistic orientation of the Fujimura & Erickson chapter stands in complementary relationship to the depth of the physical detail which Stevens brings to what is, in name, the same topic, i.e., acoustic phonetics. In the fourth chapter of this section, Hajime Hirose provides an in-depth account of laryngeal physiology as it relates to speaking (whispering included!) and surveys instrumental techniques for studying the larynx. Peter Ladefoged describes, in the fifth chapter, instrumental techniques for fieldwork in linguistic phonetics. He stresses the often disregarded point that linguistic phonetics is about POPULATIONS of talkers, not individuals. Following Ladefoged's caution, the final chapter of the section by William Shearer covers the experimental design and statistical issues one encounters in both single and multiple subject studies.

Part II of the Handbook is entitled 'Biological perspectives' and comprises three chapters. The chapter by Janet Mackenzie Beck (third in this section) covers the organic variation in the structures of the vocal tract due to intra-speaker (e.g. growth or trauma) and inter-speaker differences. Beck describes anatomical variation in some depth, but provides less information regarding the acoustic consequences of that variation. In the section's central chapter, Ray Kent & Kristin Tjaden provide a very concise and clear discussion of brain anatomy and physiology as related to speech, drawing parallels with vocal behaviour in non-humans. They end with thoughts about neurological linkages between speech perception and speech production. The section begins with Gary Weismer's tutorial on motor speech disorders. It is refreshing to see this chapter in a handbook of phonetics. Not only are studies of disorders valuable for their clinical implications, but there is growing support for the view that studies of disordered speech can provide valuable insights for theories of normal speech production; for example, by illuminating which failure modes are and are not observed. The timeliness of this topic was reflected in the very strong attendance for a workshop on

the topic at the fall 1997 meeting of the Acoustical Society of America (co-chaired by Lynne Bernstein and Gary Weismer).

The most extensive section of the handbook (9 chapters) is devoted to models of speech production and speech perception. In the section's initial chapter, Peter MacNeilage discusses the acquisition of speech from a production standpoint, presenting the view that simple rhythmical jaw opening and closing gestures are gradually elaborated to include all articulatory gestures. Joseph Perkell presents an overview of studies of articulatory processes showing how the articulatory level fits into the mapping from linguistic message intended by the talker to the message received by the listener. He details the many degrees of freedom involved in the articulation of speech and how those degrees of freedom may be controlled. Following Perkell's chapter, theoretical issues related to coarticulation and variation in connected speech are presented by Edda Farnetani, Farnetani's chapter is a very good overview, touching on most of the issues surrounding coarticulation and processes in continuous speech, as well as the models advanced to explain them. Speech production models from a motor control perspective are covered in an impressively clear chapter by Anders Löfqvist who expands on both the physiological data presented by Perkell and on the theoretical issues developed by Farnetani from yet a third perspective, that of Action Theory. Ailbhe Ní & Christer Gobel, in their chapter, describe voice source variation from a modelling perspective. They first describe the Liljencrants–Fant voicing source model (Fant, Liljencrants & Lin 1985) and illustrate the relationships among model parameters and acoustic properties of the glottal source. Ní & Gobel then relate source characteristics, typically described in subjective perceptual terms such as breathy or creaky, in terms of acoustic features and model parameters.

Kenneth Stevens' chapter on articulatory-acoustic-auditory relations revisits acoustic phonetics to provide extensive detail on the mapping from vocal tract configuration to acoustic output for a variety of phonemes and sound classes. Stevens' chapter ends appropriately with a short discussion of relations between speech acoustics and perception. This serves as a bridge to the final three chapters of this section, related to speech perception.

The first of these three chapters, by Bertrand Delgutte, presents aspects of neurophysiology as related to speech perception. This covers neural coding of spectral information and includes mention of some possible neurophysiological substrates of categorical perception. Delgutte further mentions in passing the possible importance of using knowledge of the auditory representation of speech acoustic structure to design feature extraction for speech analysis and recognition, a topic which is of rapidly growing interest. Brian Moore discusses psychological acoustics and speech perception, showing how studies of human perception for relatively simple acoustic stimuli (correctly designed) can provide insight into both spectral and temporal processing of more complex stimuli like speech. Finally, cognitive

processes in speech perception and word recognition are presented by James McQueen & Anne Cutler. This chapter, much of it posed as data testing two conflicting theories of word recognition, provides an excellent overview of speech recognition from a psychological perspective.

In Part IV of the handbook, linguistic phonetics is presented in chapters by Peter Ladefoged ('Linguistic phonetic descriptions'), Ian Maddieson ('Phonetic universals'), Sieb Nooteboom ('Prosody of speech: melody and rhythm') and John Ohala ('The relation between phonetics and phonology'). Ladefoged's chapter details articulatory and acoustic features used to describe speech in phonological terms. Maddieson describes two broad classes of linguistic universals: those due to mechanical constraints which reflect the capabilities of human anatomy and physiology; and universals due to ecological constraints, that is, those which reflect the communicative function of language such as the need for contrast and differentiation within the acoustic speech signal. The chapter on prosody by Sieb Nooteboom is a fine introduction to another area experiencing considerable empirical and theoretical growth. The author's account of intonation is rooted firmly in the IPO (Institute for Perception Research) school which emphasizes pitch movements; essentially, stylized representations of portions of pitch contours. This description contrasts with a tone oriented account (Pierrehumbert 1980), which posit a smaller number of more abstract tonal targets (specifically 2) underlying observed f0 contours. A more extensive account of this latter view would be a valuable addition to the handbook since it represents a contrasting theoretical account and, more practically, it forms the basis for part of the ToBI method for transcribing prosodic structure which is becoming the standard for labelling large speech corpora. In the final chapter of this section, John Ohala explores the relationship between phonetics and phonology, tracing the historical differentiation of the two fields from their seventeenth century common root, critiquing the divorce which left phonology as an endeavor which '…endlessly recycles much the same data, trying out different labels and descriptive devices on it…' (684), and finally presents a series of issues and questions which illustrate how phonetics and phonology should, in Ohala's view, be integrated.

Finally, Part V of the handbook presents speech technology with overviews of digital speech processing techniques (Johan Liljencrants), approaches to automatic speech recognition (William Ainsworth), speaker recognition and forensic phonetics (Francis Nolan) and speech synthesis (Rolf Carlson & Björn Granström). Liljencrants' chapter presents an overview of digital speech processing techniques in analog to digital conversion, spectral analysis (the discrete Fourier and related transforms, linear predictive coding), the z transform, pitch extraction, and speech coding. Ainsworth provides a concise and very readable introduction to automatic speech recognition which touches on many of the concepts and techniques used in this area. Francis Nolan's contribution on speaker recognition and forensic

phonetics explores the various issues related to speaker identification and verification and, more generally, how one might characterize individual talkers. The final chapter in this section by Carlson & Granström is on speech synthesis. Speech synthesis is the most mature of all the speech technology applications and consequently has a number of well identified problems as well as notable successes. There are a broad range of synthesis techniques being widely used today, and the quality of synthesis (in terms of its intelligibility) is quite good. Still, naturalness is a problem for most present synthesizers, and there remain unsettled issues regarding how one ought to assess the quality of synthetic speech. Carlson & Granström contribute a very thorough overview of these issues.

Ideally, a handbook provides guidance and information on topics while maintaining balance in disputed areas. In this sense, H&L and their many contributing authors have done very well, providing a truly impressive amount of information with relatively few imbalances. Handbooks should also cover all of the topics considered important within a field. Here too, H&L have done well; however, there are a few omissions beyond those already noted. First, while development of speech production is well represented in MacNeilage's chapter, there is no parallel account for the development of speech perception. Secondly, there was virtually no discussion of second language acquisition. Further advances in acoustic analysis techniques grounded in human perceptual properties (e.g. Hermansky 1990), and discussion of computational models of auditory signal processing as applied to speech analysis (e.g. Shamma 1988) are topics which, possibly together, may deserve a full chapter, but are mentioned only briefly. Finally, perhaps not so much an omission as a missed opportunity: this volume cries for a companion CD-ROM containing sound and/or image files supplied by authors to accompany their chapters. The editors and most of the contributing authors are very familiar with the digital formats needed for this and it would have been a wonderful addition to the Handbook.

The previous criticisms not withstanding, *The handbook of phonetic sciences* is a most welcome and needed reference to this very active field. It should prove an invaluable aid to graduate students starting research projects, to active investigators who are planning to extend their research to a new area in the field, and as a general reference for professionals in the many areas that are now part of the broadly defined phonetic sciences. It will certainly not gather dust on this reviewer's bookshelf.

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Kathy Hirsh-Pasek & Roberta Michnick Golinkoff, *The origins of grammar: evidence from early language comprehension*. Cambridge, MA: MIT Press, 1996. Pp. x + 230.

Reviewed by ANNE VAINIKKA, Johns Hopkins University

A revolutionary new technique for studying children's early comprehension of syntax is presented in this exciting book. Consider an analogy of a luxury hotel: a beautiful, well-designed lobby (chapter 2 which contains an overview and analysis of existing acquisition theories); a breathtaking view (chapter 3 with the description of the new technique, the preferential looking paradigm); well-constructed and reliable rooms, even if somewhat disappointingly small (chapters 4–6 on the experimental results), and finally the restaurant with a tasty menu (chapter 7 on the theoretical framework proposed here).

At the outset of chapter 2, Hirsh-Pasek & Golinkoff state their aim, namely that they wish to develop a theory that provides a COMPROMISE between two traditionally distinct approaches to language development, represented by nativistic theories on one hand and interactionist theories on the other hand. The authors refer to nativistic theories as INSIDE-OUT THEORIES because innately present linguistic mechanisms in the child's brain must find realization 'outside', in the actual language to be acquired. Interactionist theories, on the other hand, are OUTSIDE-IN, since language is presumed to exist outside of the child's brain, in the environment, and it must be internalized. Although the stated aim may seem overly ambitious, by the end of the chapter one must concede that the authors have succeeded remarkably well in reaching their goal.

The result of chapter 2 is based on the demonstration that there are three hyperboles concerning early language acquisition, and that the existing theories fall along continua with respect to these hyperboles. HYPERBOLES I and III concern initial linguistic structure. As shown in the diagrams on p. 43, the source of initial linguistic structure and categories is assumed to be INNATE for inside-out theories, while outside-in theories claim that grammatical development is derived from COGNITIVE and SOCIAL CATEGORIES.

However, as indicative by the term 'hyperbole', neither side relies exclusively on the initial TYPE of categories nor on the initial SOURCE that they claim. Thus, even the most extreme inside-out theorists (e.g. Chomsky 1988) acknowledge that categories such as Theta-role (Agent, Patient) are cognitively related. Similarly, outside-in theorists (e.g. Schlesinger 1988) assume a sensitivity to inflectional markings in the input. Summarizing in Hirsh-Pasek & Golinkoff's words, 'both families of theories grant that the child has at least some linguistic sensitivities at the start (if not a full grammar) and is capable of conceptualizing the environment in terms of language-relevant cognitive and social categories' (42).

HYPERBOLE II deals with the mechanism used for language learning. As the starting point, outside-in theorists claim that learning is domain-general, whereas inside-out theorists assume that a language-specific acquisition mechanism exists in the human brain. The authors show how outside-in theorists in fact have built-in mechanisms and processes which are specific to language – otherwise the child could not form linguistic generalizations. Inside-out researchers, on the other hand, make use of domain-general learning processes in, for example, making reference to the frequency of a morphological element in the input, thus blurring the line between the two approaches to acquisition. Both types of approaches, in fact, make use of both domain-general and domain-specific learning.

Chapter 3 on the 'Intermodal Preferential Looking Paradigm' is a must for anyone looking to set up their own acquisition laboratory, since this technique is the only one that can be used to directly test the COMPREHENSION OF SPECIFIC SENTENCES by infants. (A related 'preferential listening' technique recently developed by Jusczyk and colleagues (Kemler Nelson et al. 1995) contrasts FULL PARAGRAPHS in infants' comprehension.) The problem with the previously existing experimental techniques (such as ELICITED IMITATION, ACT-OUT or PICTURE CHOICE) is that they cannot reliably be used with children around or under the age of two, the age at which syntactic acquisition commences. The new method presented here allows for controlled study of grammatical (syntactic) relationships through dynamic stimuli, without requiring any overt action on the child's part.

The preferential looking paradigm involves two video screens with separate scenes; for example, a woman kissing a set of keys on one screen and a different woman kissing a ball on the other screen (figure 3.1). The infant is seated on the parent's lap in the middle of the two screens, and hears a voice from a point in the middle of the screens which in this example might say, ''Hey! She's kissing the keys!''. In the research reported in chapters 4–6, it is shown that this method works consistently in that children will look LONGER at the screen with the picture which matches the sentence that they hear. Thus, it can be concluded that children who look longer at the matching picture understand the sentence, or at least that they understand the relevant part of the sentence that distinguishes between the two screens,

in this case *keys* vs. *ball*. Note that the location of the voice (middle) gives them no clue about which screen to look at, nor is the blindfolded parent able to provide any clues. Furthermore, neither picture is intrinsically more salient than the other; this is controlled by also testing with the same pictures but a different sentence (*Hey! She's kissing the ball*) and finding that in this case children look longer at the ball scene. Note also that both scenes are designed to be similar in prototypicality (both non-prototypical) in order to discourage watching a scene based on the level of familiarity. Further controls include INTERTRIAL INTERVALS during which the screens are blank and a light in the middle serves to draw the child's attention to the neutral middle area, introducing each screen SEPARATELY (with a neutral sentence such as *What's going on?*) as well as SIMULTANEOUSLY with a neutral sentence (*What are they doing?*), allowing the child ample time to study both screens before the actual test trial.

Although the preferential looking paradigm represents exciting progress in the field of early syntactic development, there are certain drawbacks to the technique. Both setting up the laboratory and creating the videotapes for the individual experiments are much more involved than traditional comprehension methods – but since the traditional methods can hardly be used to study very early acquisition, the new method cannot be directly compared with them. A constraint of the new method is that only FOUR TO SIX PAIRS of stimuli can be compared in one study (using forced choice), given the extensive intertrial material and subject fatigue. Subject loss is also high given that relatively complacent children are required as test subjects.

The main experiment reported in chapter 4 involved scenarios similar to the example case already discussed, except that both the ball and the keys were present in each scene. Thus, for the sentence *Look, she's kissing the ball!* a woman kissed the BALL while moving the keys in the foreground on one screen, while on the other screen a woman kissed the KEYS while moving the ball in the foreground. Having both items present and salient in both scenes prevented the child from finding the match just based on the object NP; in fact, the child had to also understand something about the action (*kissing*) in order to pick out the matching scene. The mean age of the test subjects (16 boys and 16 girls) was 14 months. 84% of the test subjects were at the one-word stage in their production.

The overall results of Experiment 1 show that these 1-year-old infants preferred to watch the matching screen (2.72 seconds) as opposed to the non-matching screen (2.23 seconds) (table 4.3), whereas no such overall preference was found with the same screens when the sentence heard in the middle was a neutral one (table 4.2). However, a further analysis by sex showed that the effect was carried by the *girls* in the sample, perhaps due to them being at a slightly more advanced stage than the boys in the sample. Chapter 4 also reports the results of a control study which proved that the match could not have been found by just attending to the end of the test sentence. The results

reported in chapter 4 allow the authors to convincingly argue that 14-month-old infants comprehend the verb-object combination in a sentence (i.e. the traditional VP).

The main experiment in chapter 5 examines the comprehension of word order in slightly older infants than already discussed (mean age 17.5 months); about half of the test subjects were still at the one-word stage, while the other half were producing word combinations. For example, for the test sentence *Oh! Big Bird's washing Cookie Monster!*, on one of the screens Cookie Monster (CM) is washing Big Bird's (BB) face as BB waves CM away, and on the other screen the actors are reversed. In the control pair of this scenario, the video scenes are identical, but the test sentence is *Oh! CM's washing BB!*. As predicted, these infants also preferred to watch the matching screen (table 5.4). The results thus seem to show that 'infants who produce limited or no two-word speech can comprehend word order in active reversible sentences' (113). A further analysis was conducted to show that the main effect of this experiment was NOT carried by the test subjects whose production was more advanced; in addition, a control study was run to rule out the possibility that children found the match by just attending to the first NP in the sentence.

Although the results reported in chapters 4–5 convincingly show that infants comprehend more than they produce, it turns out that (contrary to the authors' view) the results from BOTH sets of experiments can be interpreted as showing that children understand the verb-object sequence, and nothing else. As shown in chapter 4, the 14-month-old girls (but not boys) are able to find the match based on comprehending the verb and the object. Not surprisingly, three months later at the age of 17 months both girls and boys can find the match based on the same verb-object sequence; that is, the matching scene for a sentence such as *BB's washing CM* can be found by just finding a scene in which CM is the object of washing. The control study that was run in chapter 5 does not rule out this alternative interpretation of the results.

Chapter 6 is written with a third author (Naigles), and it clearly does not fit the flow of the rest of the book. Three general findings emerge from the four experiments reported in this chapter. First, by 28 months of age children are able to use the transitive syntactic frame (SVO) in finding the matching scene, where the meaning of the verb is unknown (either rare or nonsense), as shown by experiments 5 and 6, but younger children use syntactic frames as a guide in verb learning (cf. Gleitman 1990).

The second result of chapter 6 is the by-now familiar one that children can find the match when provided with a transitive sentence, although with the twist that the youngest group (19 months) failed to find the match (Experiment 5). The third finding of chapter 6 is a rather surprising one, namely that INTRANSITIVE verbs seem to be more difficult to comprehend than transitive ones; however, this was probably due to the complex sentence

types used to test for intransitive verbs, as is also assumed by the authors (Experiments 5, 7 and 8).

Except for the finding that 28-month-olds are sensitive to syntactic frames, all the positive results reported in chapters 4–6 can be reanalyzed as involving interpretation of the verb-object sequence in a transitive sentence. Chapter 4 showed that 14-month-old girls were able to comprehend the verb-object sequence, while chapter 5 showed that at 17 months of age even the boys comprehended similar sentences. For some reason (perhaps because the stimulus sentences were embedded under a matrix verb such as *find*) this result was not replicable with the 19-month-olds in chapter 6. Thus, although the final theoretical chapter provides much food for thought, it is weak in that it relies on a much broader interpretation of the results in the preceding chapters than is warranted by the actual results under the interpretation provided here (which might of course be ruled out by future experimentation); chapter 5 with word order, and chapter 6 with syntactic frames. However, the narrow experimental results do not detract from the invaluable contribution of the authors in providing the world with a wonderful new tool. The view from the hotel room is equally breathtaking even if the room is smaller than expected.

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D. Robert Ladd, *Intonational phonology* (Cambridge Studies in Linguistics **79**). Cambridge: Cambridge University Press, 1996. Pp. x + 334.

Reviewed by Janet Bing, Old Dominion University

Intonational phonology is a valuable book because it fully addresses prevailing theories about intonation by asking and clarifying important questions, identifying assumptions, and summarizing similarities and differences. The book is written in Ladd's usual lucid style with helpful examples, figures,

charts and tables. I found most of the book interesting, informative and relatively easy to read. Ladd's knowledge of intonation studies and his ability to clarify the underlying issues are impressive.

He begins by defining and discussing the implications of most of the terms used throughout the book, including INTONATIONAL PHONOLOGY. In chapter 1, Ladd raises the traditional issue of which parts of intonation are linguistic and categorical, which paralinguistic and gradient; he returns to this issue at the end of the book in the context of speech recognition and production. He identifies the common assumptions behind 'instrumental' studies (such as those done at the Institute for Perception Research in Eindhoven, also known as IPO) and 'impressionistic' studies (such as the work of Bolinger), noting that instrumental studies often treat intonational meaning as 'paralinguistic'.

Chapter 2 describes the Autosegmental-Metrical (AM) theory and its basic tenets. It includes a good review of the literature on stress and accent, pitch accent and tune-text association. Ladd shows how distinctions such as ALIGNMENT and ASSOCIATION (55) can easily be expressed in AM theory. He updates the 'levels-vs.-configurations' debate, comparing the level-pitch analysis of Pierrehumbert, Hirschberg and Beckman (who assume linearity, local rules and downstep) to the analyses of some of the Scandinavian and Dutch IPO researchers such as Bruce, Gårding and t'Hart (who assume global configurations and declination). Ladd discusses justifications for both approaches, but prefers the AM hypothesis.

Chapter 3 is primarily an exposition of Janet Pierrehumbert's intonational taxonomy, not only the original (1980) dissertation, but also later revisions by Pierrehumbert and her colleagues. Ladd charts correspondences between Pierrehumbert's ACCENTS and British-style NUCLEAR TONES, noting that Pierrehumbert does not distinguish between nuclear and pre-nuclear tones. Although Ladd does not point out that the number of 'legal' tunes in Pierrehumbert's system is far greater than the 22 logical combinations he lists (82), he later modifies her finite state grammar (81) for a model (211) that not only distinguishes between nuclear and non-nuclear accents, but also predicts fewer possible tunes. Ladd criticizes other aspects of Pierrehumbert's system, including the fact that it represents 'impressionistically similar contours very differently, and impressionistically very different contours very similarly' (99). This chapter also contains good discussions of downstep, the 'intermediate phrases', the ToBI (Tones and Boundary Indices) system of transcription and abstractness in representing tones.

Chapter 4, 'Cross-language comparison of intonation' begins with the question of whether different claims about universals in intonation are justified. Ladd gives evidence that they are not. He describes the falling intonation of yes–no questions in Hungarian, a clear exception to universalists' claims that all yes–no questions rise and all statements fall. He discusses phonetic differences between languages, such as the fact that in

English local peaks occur near ends of accented syllables, but in Italian near the beginnings. Ladd notes that some languages compress intonation contours, whereas others truncate them, and he discusses differences in the association of accents with stressed and unstressed syllables. Throughout the chapter he successfully demonstrates the usefulness of AM notation for comparing different systems and for testing claims about universals.

The issue of universals extends into chapter 5 with a discussion of how languages differ systematically in their patterns of sentence-level accentuation. Ladd contrasts syntactically based assumptions of 'normal stress' to hypotheses which attribute the distribution of accents to semantics and context. The chapter compares the prosody of questions and of given information in a number of languages; the examples are excellent. However, readers not familiar with the literature on focus and prominence (discussed in chapter 4 of Ladd 1978) may find some of this chapter confusing or contradictory unless they have skipped ahead to the final section, where Ladd clarifies some apparent contradictions.

Chapter 6 centers on a discussion of the internal structure of intonation contours. Ladd disputes Pierrehumbert's claims that there is no difference between nuclear and non-nuclear accents and argues that languages have distinct 'tunes'. Most of this chapter deals with the nature of prosodic hierarchies; Ladd's claim is that the Strict Layer Hypothesis must be revised to allow for what he calls COMPOUND PROSODIC DOMAINS (CPDs). CPDs allow Ladd to re-examine a number of traditional prosodic problems. For example, by accepting the possibility of recursive intermediate phrases, Ladd reframes a number of problematic issues such as sentences with multiple foci, the role of 'sentence stress', differences in boundary strength in different syntactic structures and the possible 'normal' patterns of prominence in sentences such as *Dogs must be carried*.

Chapter 7 deals with the phonetics of pitch range variation and with how to normalize pitch ranges across speakers; this chapter will especially interest those working on speech recognition and synthesis. In this final chapter Ladd discusses relative pitch ranges, downstep, raised peaks, and register. The book ends where it began, with questions about which parts of intonation contours are linguistic and categorical and which are paralanguage.

One of the strengths of this book is Ladd's understanding of different approaches and his ability to describe them clearly; the price of the clarity and simplicity of his explanations is that he represents many issues as dichotomies. This understandable need to simplify, perhaps sometimes oversimplify, is counterbalanced by the fact that after he discusses the strengths and weaknesses of different hypotheses, Ladd usually proposes his own solutions to the problems being addressed. Readers may not agree with all the solutions, but they will understand them.

I applaud any writer who defines key terms when introducing them, and in most cases, Ladd does. One exception is his use of word *focus*. With the

exception of Dik (1981), many linguists use the term FOCUS without definition, despite the fact that some identify focus with accent, others with particular syntactic structures and still others in terms of scope. Focus is a widely-accepted metaphor, but a poorly defined term, and, like many others, Ladd works from 'a purely intuitive notion of focus' (162), defining it only by exemplification.

Two other concepts I felt needed both more definition and explanation were those of TONAL SPACE, 'intended as a name for a construct that is crucial to making AM intonational phonology work' (73) and SCALING. Ladd proposes these concepts in lieu of global phenomena elsewhere called DECLINATION, REGISTER, RANGE, and TONE LEVEL FRAME. In order to understand what Ladd means by tonal space, I found it necessary to refer to Ladd 1990.

Interestingly, in that article Ladd assumes a three-element rather than two-element analysis. In Ladd (1990: 30), his proposed model includes NEUTRAL PITCH 'from which F_0 movements to H or L begin, and to which F_0 tends to return after H or L'; in *Intonational phonology*, he assumes the elements H, M and L for intonation in English (153), French (57) and for tone in Yoruba (62, 254) with no explanation for the presence of a third element. In *Intonational phonology*, Ladd explicitly claims to use the two phonological elements, H and L, found in most AM analyses: he claims that 'two abstract levels and a moderately elaborate set of phonetic realization rules can successfully model intonation contours in languages like English' (272). A number of arguments can be made for a three-element system, particularly for analyses of tone (see references in Bing 1993), but a three-element model makes different claims and predictions than a two-element model.

Other readers will undoubtedly take issue with other aspects of this book, but those who hope to study and better understand intonation should read it, regardless of their theoretical biases. When I began this book, I was uncertain there would be a wide audience for it; having read it, I agree with the promotional description on the cover: 'This book will appeal to phonologists and phoneticians as an original contribution to the debates it discusses, and it will be welcomed by a wide range of students and researchers as an ideal overview of recent work'.

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Shalom Lappin (ed.), *The handbook of contemporary semantic theory*. Oxford: Blackwell, 1996. Pp xvii + 670.

Reviewed by HENRIETTE DE SWART, University of Utrecht

This handbook gives an overview of some recent developments in semantic theory. The book is organized into 11 sections, which cover important topics in semantics. The articles are written by well-known semanticists, who are invariably at the top of their field in the domain at hand. This leads to high quality papers that are almost without exception a great pleasure to read. Consider for instance Partee's paper, which constitutes a perfect opening of the book. This first-hand report of a pioneer of the field demonstrates how Montague's work found its way into the linguistic community, and how formal semantics developed from there into a serious branch of modern linguistics. The book continues nicely with section II, which lists contributions by scholars who made generalized quantifier theory accessible to linguists. Keenan offers an overview of the results of the study of generalized quantifiers over the past fifteen years. The article focusses on linguistic constraints and generalizations that the theory allows us to formulate, and that play a role in the analysis of existential contexts, negative polarity, partitivity, etc. Cooper gives a situation theoretic treatment of generalized quantifiers. He shows how we can use resource situations to account for contextual restrictions on the domain of quantification. The two papers in this section have strong ties with Sher's contribution in section XI. Sher describes the role of Tarski's conception of truth in semantic theory. She discusses a number of general issues like the logical nature of Tarskian semantics and the definition of logical terms, but she also includes more specific topics such as logical connectives and generalized quantifiers.

The authors seem to have received a large amount of freedom in their writing. They were clearly encouraged to present their own insights, but also to situate their own work in the context of current semantic research, and to make it accessible to a wider public. In some cases, this leads to very nice papers that nevertheless present a more idiosyncratic view of a particular topic than one would expect from a handbook. An example is the section on tense. Enc's paper addresses a rather specific problem in the area of tense and

modality, namely the question whether English *will* is a future tense or a modal. Her contribution is interesting, but it reads as a research paper, rather than a handbook article. Given that the section does not contain other articles, this leads to a somewhat unbalanced structure of the book as a handbook. Other sections which consist of just one article are more like overview articles with an personal flavor. In the section on plurality for instance, Landman develops his own analysis of collectivity and distributivity, but also presents interesting comparisons with previous work on these topics. Similar remarks can be made about Nerbonne's paper on computational semantics, which reflects his own views, but also addresses general questions in the field, and discusses the division of labor between theoretical linguists and computational linguists. Maybe the structure of the book would have been more transparant if the number of sections had been smaller. For instance, Nerbonne's paper could have been integrated in section XI, the section on semantics and related domains, as a discussion of the relations between semantics and computer science. Along similar lines, the paper by Levin & Rappaport Hovav could have been included in section III. The paper contains an insightful discussion of the connections between lexical semantics and syntactic structure and focusses on so-called LINKING rules. It presents interesting ways to associate arguments bearing certain semantic roles with certain syntactic expressions. Given that lexical semantics is a branch of semantics, and predicate-argument structure is part of both syntactic and semantic theory, it would have been natural to include the paper in the section on the interface between syntax and semantics.

Note however that the personal approach works out very nicely in those sections in which several papers highlight different aspects of a particular research domain. Beautiful examples are section III on the interface between syntax and semantics, section IV on anaphora, discourse and modality, and section VII on questions. In section VII for instance, we find a paper by Higginbotham that focusses on the relation between form and content of questions. Accordingly, it is concerned with the distinction between direct and indirect questions, quantifying-in, and presuppositions, and analyzes these phenemena extending classical frameworks in which interrogatives introduce a partition of the answer space. In the same section, we find a paper by Ginzburg, who rejects the notion of partition in favor of a more agent-dependent notion of answerhood that is useful in the analysis of dialogues. He introduces questions, propositions and facts as separate ontological categories in a situation-based framework. Each one of these papers provides important insights into the nature of questions and interrogatives. Moreover, the complementary nature of pairs of well-written papers like these makes them very suitable for classroom use, where they can be used to present rather different perspectives on one and the same phenomenon.

Something similar is true for section III, which discusses binding, anaphora and ellipsis from different points of view. Roughly, Jacobson represents the

categorial grammar approach, in which a surface syntactic structure is directly mapped onto a model-theoretic interpretation. She uses type-lifting and function composition to demonstrate that unbounded extraction, antecedent-contained deletion and right node raising can be analyzed without abstract levels of representation like LF or abstract elements like traces. Fiengo & May discuss the question whether anaphoric coreference is a linguistic rule in the sense in which this notion is defined by generative grammar. They want to know whether the coindexation system used by syntacticians reflects linguistic knowledge of the speaker, or speaker's intentions. The cognitivist perspective they adopt is closely related to Jackendoff's approach in section XI, although the issues addressed are somewhat different. Lappin discusses a number of incomplete constituents. He treats exception phrases as NP storage, VP ellipsis as reconstruction at LF and bare argument ellipsis as direct (semantic) anaphora resolution. Although at different levels, all three papers give an interpretation of elided structures. In fact, there is a fourth paper which addresses the issue of ellipsis, namely Kempson's paper in part XI. If we compare the papers, we see that Jacobson defends a syntactic account of ellipsis which respects surface compositionality throughout, Fiengo & May treat ellipsis as syntactic reconstruction at LF, Kempson uses an inference mechanism driven by natural deduction, and Lappin develops a differentiated account which mixes syntactic and semantic operations. The four papers taken together give an interesting overview of the insights on ellipsis obtained with different methods of analysis.

Section IV is also very nice in the way it brings together various issues that arise when we extend our semantic theory to meaning at the discourse level. The study of discourse came up as a central theme of research in the eighties, and dynamic theories of meaning continue to dominate the semantic stage in the nineties. Discourse proved to be an area of research which falls outside of traditional syntactic theory, and in which classical logical theories fail to give a correct description of linguistic observations. As a case study, the papers by Roberts on the one hand and Groenendijk, Stokhof & Veltman on the other hand treat constraints that modal expressions impose on anaphorical relations at the discourse level. Gawron's paper is concerned with the way quantified expressions set up reference domains for further discourse. He works out a system of constraints on the domain of quantification which accounts for quantificational subordination. The notions of partial information and information growth, context change and update are central to the dynamic semantic analyses these authors develop. Gawron's article is further related to Cooper's contribution (section II) by its incorporation of situation theoretic insights, and with Rooth's paper (section V) by its concerns with focus and alternatives.

Section V is somewhat loosely structured, but contains good overview articles of issues that go beyond semantics proper (compare also Kempson's

paper in section XI). Rooth discusses semantic and pragmatic meaning effects that arise in various contexts in which we find association with focus, and points out important problems in the analysis of these phenomena. This is a perfect introductory paper for a seminar on focus. Horn sketches the background of two important notions at the borderline of semantics and (formal) pragmatics, namely presupposition and implicature. One of the attractive features of Horn's contribution (as of much of his other work) is the insightful way in which he presents the historical sources and developments in both the philosophical and the linguistic literature. Ladusaw's article is a survey of some recent proposals and results in the analysis of natural language negation. What is particularly nice is the way the paper structures the kind of research questions one needs to address in the analysis of (clausal) negation, negative polarity and negative concord.

Section XI is clearly intended to present semantics from different points of view. We have already seen that the interface between syntax and semantics is treated separately in section III. The last section of the book discusses issues in the relation between semantics and logic (Sher), semantics and cognition (Jackendoff), semantics and pragmatics (Kempson), and semantics and philosophy (Katz). The collection of papers in this section nicely mirrors Partee's opening article of the book, in which formal semantics is taken to be a bridge between logic, philosophy and cognitive science. Besides the fact that they all study semantics in relation to some other domain, the coherence relations between the individual papers are relatively weak, except maybe for the contributions by Jackendoff and Katz. These articles reflect rather different opinions on the cognitive status of concepts and the relation between language and the world, but they share a rejection of the purely extensional, referential approach. Interestingly, there are important connections with articles elsewhere in the handbook, most of which I have attempted to draw attention to along the way in this review. Note further that the variety of approaches and frameworks highlights the more general question of where semantics fits into the overall picture of language and mind, and how certain choices drive semantic research. The field of semantics has always shown a great tolerance for divergence. In my experience, students often find it confusing that semantics does not have a 'standard' overall theory that most or all researchers adhere to. The section on relations with other domains shows that what might look like a shattered image is actually more like the many sides of a diamond. One of the values of a handbook like this is to show how different approaches hang together and are part of a collective effort to disentangle the mystery of meaning.

Without aiming for completeness (an ideal which remains out of reach, even for a book containing almost 700 pages!), the handbook gives a representative overview of contemporary semantic theory. One can quibble about the organization of certain sections (see above) or the limited number of languages discussed (nearly all papers talk exclusively about English, but

is semantics really that universal?). One can deplore the absence or marginal attention paid to certain topics (like comparatives, mood, aspect, genericity, adverbs, scope), or certain theories often used in mainstream work (like type theory, discourse representation theory). However, the choices one makes are at least partially a matter of individual taste. Overall, the editor did a great job of bringing leading scholars together, and the content of the articles they contributed is good to excellent. I am sure the book will be widely used for reference and initial introduction to new topics by both researchers and students.

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Andrew Lock & Charles Peters (eds.), *Handbook of human symbolic evolution* (Oxford Science Publications). Oxford: Clarendon Press, 1996. Pp. xxix + 906.

Reviewed by JAMES HURFORD, University of Edinburgh

The following excerpts from the editors' Preface indicate what they set out to achieve.

> Our primary purpose is to provide the reference materials that would help in developing a clearer picture of *what* has occurred over time in the performance and elaboration of human symbolic abilities. We see this as a necessary pre-requisite to any theorizing as to *how* that elaboration is to be explained. …The number of disciplines that contribute, either directly or indirectly, to this area of inquiry is such that no single investigator can reasonably hope to judge all the sources of evidence that bear on the topic. This is one of the motivating factors behind this volume: to have evaluated and make [sic] accessible as much of this material as possible. We hope, then, that *ex post* this volume, a palaeoanthropologist, for example, will be better able to invoke the role of, say, linguistic factors as important in hominid evolution, and to do so in a more informed way. (vii–viii)

This *Handbook* is somewhat richly illustrated, with two 'photogalleries', in black-and-white, one of fossil skulls, and another of contemporary hunter-gatherer rock art. There are also ample line-drawing illustrations and many summary tables and charts. The *Handbook* contains many individual articles that are interesting in themselves, often well written or cogently argued, in some cases written by acknowledged world experts in the specialism

concerned. Overall, however, from the point of view of Linguistics, the *Handbook* is a disaster, a view which I will defend later below. But first, I turn to some of the *Handbook*'s good points and also some neutral ones.

The editors have clearly put a lot of work into this edition. This shows in the level of detail at which they intertwine the various contributed chapters with their own editorial introductions, commentary and appendices. There are substantial editorial appendices to several of the articles, which typically seek to relate the content of the article in question more generally to the themes of the handbook. In addition, the editors are themselves, apparently, responsible for writing the abstracts to some of the articles. They have also provided several informative sections of summary of the principal established facts in some subfield (such as the hominid fossil record or hunter-gatherer rock art), often in the form of a 'photogallery'. The guiding and conscientious editorial hand can also be seen, I believe, in the unusually high number of summary tables, often running to several pages, which many of the contributors have incorporated into their chapters; if one takes the appropriate precautions about interpreting the typical simplified one-or-two-phrase entries that such tables, by their nature, consist of, these tables are potentially very useful.

The difficulties faced by the editors poke through in several places. One planned, and important, contribution, on the evolution of the vocal tract, was not forthcoming, and the editors made a valiant attempt to plug the gap themselves from notes supplied by the would-be contributor; this is not very satisfactory. But any work of encyclopedic proportions is prone to such problems. In general, it seems clear that this handbook has been a long time in the making. Though published in late 1996, one contribution mentions 'completion of the main manuscript in … 1993' (50). Very few of the entries in any of the individual lists of references come from the 1990's. Indeed, many of the chapters survey work that dates predominantly from the 1970's and early 1980's. This is perhaps justifiable, in that the reader of a handbook wants to know that the results summarized have stood the test of time and are not just recent fashions. On the other hand, many of the fields reviewed are developing very fast and it would be surprising if there were not more interesting and important results and theories to report from the early 1990's.

At least six of the 35 contributors to the *Handbook* are or have been at the University of Lancaster, where Lock once taught. This might suggest that personal acquaintance played an important role in selecting contributors. Not all of the contributors associated with Lancaster are obvious choices as authoritative figures, on the basis of their scholarly records in the field. For four of the contributors, private, non-institutional addresses are given, possibly indicating retired status.

The *Handbook* is divided into four major parts, entitled 'I: Palaeo-anthropology', 'II: Social and socio-cultural systems', 'III: Ontogeny and symbolism', and 'IV: Language systems'. This arrangement looks promising,

as it seemingly recognizes the central place of ontogeny in accounting for modern human adult capacities, as linguists recognize the central place of language acquisition in a theory of language. And, promisingly to a linguist, just under half of the tome falls in the 'Language systems' part. I will discuss the preceding parts first.

The first major article, 'An outline of human phylogeny', by Bernard Campbell, is straightforward, clear and duly cautious, outlining a story no doubt already familiar to those with an interest in human evolution. As with many such summary articles in this collection, its functional equivalent could have been found elsewhere (e.g. in Richards 1987), but the material was worth recapitulating in a collection with the goals of this volume.

A second major article, 'Evolutionary trees of apes and humans from DNA sequences', by Peter J. Waddell & David Penny, brings us nearly up to date (once its epilogue is read) on a topic that is often hard for linguists to penetrate. Its conclusions are worth quoting:

> We conclude that, despite recent controversies, these and other molecular data are consistent with the hypotheses that *Homo sapiens sapiens*:
>
> 1. is a very recent species (less than 200000 years old);
> 2. originated in a localized region in Africa; and
> 3. close to 100000 years ago spread out of Africa to replace all other hominids living in Europe (Neanderthals) and Asia (for example, the Solo specimens). (54)

The DNA field moves very fast and there is now exciting new work (Krings et al. 1997), dating the *sapiens sapiens*/Neanderthal split to around 600,000 BP, earlier than previously estimated; this work also confirms Waddell & Penny's conclusions, quoted above, on the dating of the emergence of *Homo sapiens sapiens* and our spread out of Africa.

Thomas Wynn is a leading authority on hominid stone tools. His chapter on 'The evolution of tools and symbolic behaviour' is informative, systematic and clear. He draws duly cautions, but still suggestive conclusions, such as

> By 300000 BP, … tool geometry required an essentially modern intelligence. … While tools tell us little about language, they do inform us about other aspects of semiotic behaviour. The use of tools in display, especially in the social maintenance displays of chimpanzees, is an indexical use of tools. (284)

Mainstream linguists generally ignore Saussure's view that linguistics is a branch of the wider field of semiology. Wynn's paper, like many others in this volume, reminds us of a wider perspective which we linguists, who stand so close to language, tend not to see.

The editors' view of symbolic behaviour takes in a panorama which includes a lot of material on art. There is Margaret Conkey's 'A history of

the interpretation of European ''palaeolithic art'', a photogallery of contemporary hunter-gather rock art, Gavin Bremner's 'Children's drawings and the evolution of art'. Art is a relative newcomer on the human scene, and the emphasis laid here on art is consistent with a view which pervades the *Handbook*, that the modern complexities of language are also relatively recent products of elaborated human cultures. Significantly, the final major chapter in the book is 'Social and cognitive factors in the historical elaboration of writing', by David Barton & Mary Hamilton. Art and writing are seen as the culminations of a continuous evolutionary process, of which an early phase was the emergence of language.

One contribution stands out from the others in espousing an autonomous, modular view of language systems, and facing up to the implication of at least some discontinuity in the evolution of language. This is the chapter entitled 'Symbols and structures in language acquisition' by Carolyn Johnson, Henry Davis & Marlys Macken. This chapter is one of the most detailed and linguistically well-informed contributions to the *Handbook*, showing, in 50 pages, an awareness of much of the detail of language structure, touching phonology, lexical semantics, syntax and pragmatics. Thus, one of the *Handbook*'s better chapters on language is at least somewhat at odds in its general view of language and its evolution from that which pervades the collection as a whole, the view held by the editors. This chapter barely gets a mention in the editors' introduction to the part which contains it.

The idea that the modern complexities of language are relatively recent products of elaborated human cultures is, on the other hand, stressed fairly strongly by the editors, both in their own commentary and in their choice of contributions. If we accept, following the DNA evidence quoted above, that our species is only about 200,000 years old, the question arises as to how long it took for fully-fledged languages of modern type to appear. This is a legitimate question, which the editors are right to emphasize. The suggested answer of this *Handbook* is found mainly in chapters which I will discuss later below.

In addition to the thoughtful and useful editorial introduction on phylogeny and ontogeny, there are good individual chapters on the evolution of human socio-cultural systems (by Randall K. White); theories of symbolization and development (by Chris Sinha, though this article says more about the theories of the past century than about evolution); the relation between speech and sign (Margaret Deuchar); the gestural origins hypothesis (Gordon Hewes); and animal language and cognition experiments (Carolyn Ristau).

I turn now to the bad points of the volume. To understand the evolution of the language faculty and of languages, one needs to know what they are, and in some detail. Noone can seriously embark on speculation about the evolution of language without mastery of at least the amount of material

contained in good introductory linguistics textbooks. Just as this *Handbook* gives us basic syntheses of the facts about the hominid fossil record, stone tool technology, DNA classification of *Homo sapiens*, and the social structures of great apes, at least as much space should have been devoted to separate surveys of the phonetics, phonology, morphology, syntax, semantics and pragmatics of languages. Little hint of the kinds of rich patterning that linguists have found in languages emerges from this collection. Even an incomplete sketch of the grammar of a single language can take, as we linguists know, hundreds of pages. One would also have liked a handbook such as this to have included a hefty chapter or two on language typology, of the kind that, for example, Bernard Comrie or William Croft might have provided.

The dominant paradigm in theoretical linguistics is still generative grammar. Any serious attempt to relate generative grammar to the evolution of language is important. In the last decade, there have been several such important attempts. The most influential paper is undoubtedly Pinker & Bloom (1990), while Newmeyer (1991) also provided a valuable detailed discussion of the issues raised when evolutionary theory meets generative theory. These papers, central to the topic of the *Handbook*, and available well before it went into print, are not mentioned anywhere in its 900-odd pages.

As noted above, the question of how quickly fully modern languages emerged after the rise of our own species about 200,000 years age is a central one, correctly raised by the editors. In one of their editorial introductions, they briefly discuss the most well known proposal in this area, namely Bickerton's (1990, 1995, 1998) suggestion that full human language appeared in a single step from protolanguage. As one of the most salient proposals in the field, one would have expected it to have received some fleshing out, perhaps in a chapter by Bickerton himself, or in a contribution by another author critically discussing it. It is certainly an idea which needs some critical analysis. But, rather than present the reader with extant controversies, the editors have chosen to fill this gap with several very speculative chapters by authors who have made no previous impact on the field, Mary Foster and Len Rolfe.

In her chapter, 'The reconstruction of the evolution of human spoken language', Mary LeCron Foster starts with a relatively unobjectionable survey of historical linguistics and its methods of reconstruction, and then embarks on her own idiosyncratic brand of speculation. In her own words:

> Since I am the only linguist who to my knowledge has published on the subject of reconstruction based on an assumption of monogenesis, I will in this section rely solely on my own experience of thirty years work in this area. (762)

Clearly, this material should not have been included in a handbook claiming to present reliably representative introductions from scholars in particular

disciplines to scholars in others. The later part of Foster's chapter is indeed not the kind of thing I have seen anywhere else in linguistics, historical or synchronic.

To conclude, I regret that I find the whole of this *Handbook* to be less than the sum of its parts. If some linguist, as opposed to a psychologist or an anthropologist, had had the vision and ambition that Lock (a psychologist) and Peters (an anthropologist) had when they embarked on this enterprise, and had been prepared to devote as much effort to it over almost a decade, the result could have been very different. Such a (now hypothetical, alas) handbook could have much more accurately represented WHAT LANGUAGES ARE, in all their many-layered complexity of use and structure, and given a perspective which is sorely lacking in this *Handbook* as it now stands, and as it will probably be consulted by non-linguists in the decade to come.

But, at the time, where was there a linguist with any such vision, ambition and energy? And where are they still? A linguist might take the view that such psychologists and anthropologists are fools who rush in where angelic linguists fear to tread. Lock and Peters themselves are aware that their enterprise might have been premature and that things might look very different ten years from now. One might argue that any linguist who decided not to attempt the kind of overview and synthesis that Lock and Peters have attempted was actually right not to have tried, because there was not yet enough relevant material to gather together. But the very existence of Lock & Peters' *Handbook* testifies against that cautious view. Their *Handbook* will be picked up from library shelves and taken as representing the state of the subject of language evolution as of the mid-1990's, but there is a gaping hole in it where Linguistics should be.[1]

We can't leave language to psychologists, anthropologists and others – they don't know enough about it. On the other hand, we linguists can take valuable lessons from neighbouring disciplines, which can provide carefully thought-out discussions of many relevant matters, including the relation of phylogeny to ontogeny, the relation of language use to theory of mind, the dissociability of elements of human cognition, the comparative structures of human and ape societies, and the comparative neurology of human and ape laterality and handedness, to mention only several. Evidence on the evolution of language and the language faculty is indeed sparse, but (a) other disciplines are now providing much more hope of data that could lead to a fruitful synthesis than was the case even twenty years ago, and (b) within Linguistics, we have been conspicuously (and laudably) unafraid of theorizing about matters where the data is either sparse or hard to interpret (e.g. the psychological structure of the modern human language faculty, or

[1] To exemplify the real danger, we have to hope that the first clause of that last sentence doesn't get dishonestly quoted out of context by the book's publishers.

the shape of long dead ancestral languages). It is vital that linguists engage in the enterprise of building up a sensible picture of how the language faculty might have evolved, and subsequently how whole languages themselves then emerged, as linguists are the specialists who, better than anyone, know what languages are and what the language faculty is. It is also vital for the health of the science of language itself that, moving forward from the isolating idealizations of this Saussurean and Chomskyan century, linguists give due thought to how the human language faculty and the individual complex systems we know as languages are related to their evolutionary context.

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Fernando Martínez-Gil & Alfonso Morales-Front (eds.), *Issues in the phonology and morphology of the major Iberian languages* (Georgetown Studies in Romance Linguistics **5**). Washington: Georgetown University Press, 1997. Pp. xiv + 694.

Reviewed by Giovanna Marotta, University of Pisa

The book is divided into sections corresponding to the Iberian languages which are dealt with, namely Basque, Catalan, Galician, Portuguese and Spanish; among them, the last one is the most represented (seven articles). Despite the title, the phonological aspects of the Iberian languages are much more present than the morphological ones: of the total of twenty articles, few deal with morphology sensu stricto, whereas others stress the interaction

between morphological and prosodic structures. The main topics concern stress structure and intonation. Segmental processes are investigated too, for instance palatalization, epenthesis or dissimilation.

The theoretical frameworks employed are different, although autosegmental theory, applied both to syllable and tone tiers, as well as metrical theory and optimality theory (OT) are specially represented. It is obviously impossible to give an exhaustive account of all the book, given the wide range of subjects involved. Space constraints allow only cursory attention to the individual contributions. However, we will discuss at least some topics that, in our opinion, are of particular interest for the contemporary theoretical debate.

We start by considering the contributions on stress. As far as Spanish word stress is concerned, the narrowing of QUANTITY SENSITIVITY (henceforth QS) seems to be a common feature. Lipski reconsiders this parameter within the moraic framework, showing that in Spanish its scope has to be reduced to the rightmost foot. The assumption of parametric variation allows the author to account for all the rhythmic patterns of the language with hardly any recourse to extrametricality: all feet are minimally bimoraic; a bimoraic trochee with only one mora in the head is therefore the default prosodic pattern. The setting 'off' of the Head_{min} parameter makes a bimoraic head possible in the case of antepenultimate stress, whereas the final stress in vowel-final words is derived from the setting 'off' of Foot_{min}. The proposal is perfectly compatible with the so-called uneven trochee (i.e. a trimoraic foot) as proposed by Dresher & Lahiri (1991) for the Germanic foot. However, one might criticize the association of the bimoraic head with two different syllables, even if they are contiguous, in the case of the proparoxitone pattern. One may wonder whether it would not be better to suspend the Foot_{max} parameter, thus allowing a dactylic foot, as proposed for example by Burzio (1994).

QS is more strongly banned from the metrical representation of Spanish nominals by Roca. In his opinion, the lack of phonological contrast between long and short vowels together with the occurrence of proparoxytones with heavy penult clearly indicate that this language is not quantity sensitive. Roca's proposal is original in more than one aspect: first of all, he attempts to reappraise the word stress rules; second, given the extrametricality of the final nominal morpheme, the right position for the foot head is recognized as standard within the stem domain, whereas the leftheaded foot is assigned a marked status. Three accent rules account for Spanish stress structure; their idiosyncratic suspension is possible and gives rise to lexical exceptionality (e.g. initial stress with a heavy rhyme in the penultimate syllable: *Frómista*). However, the assumption of the stem as the relevant domain for accent rules and prosodic parameters might be questionable.

QS as well as foot boundedness are rejected by Saltarelli in his analysis of Spanish stress. In particular, the pattern of imparisyllables found in Spanish

(such as *régimen*) is examined, showing how the standard view, based on the assumption of a (binary) bounded parameter, is not able to account for it in a simple manner. The prosodic projection of morphological paradigms allows a more exhaustive explanation. Paradigm is here defined as the result of the interaction between the members of stem set and the inflectional rules of the language. An historical excursus is presented too, starting from Old Latin, where the unbounded parameter of the Indo-European was narrowed to a pre-antepenultimate pattern, going through Classical Latin, where the canonical three-syllable window was imposed.

In the contribution by d'Andrade a review of relevant works on Portuguese stress is presented, beginning with early grammarians up to the most recent studies. Brazilian and European varieties are discussed, in the belief that a limited set of principles and parameters may interpret the stress structure of all dialects, without reference to the different phonetic cues they show. Stress structure is also investigated by Serra, who believes that the Latin stress mechanism has basically been preserved in Catalan. Therefore, in both languages the moraic trochee is assumed as the basic foot, while the difference concerns the scope of extrametricality: in Latin it is general and applied to the final syllable; in Catalan it is exceptional and affects not only syllables, but also morae. In such a picture, the major problem becomes the treatment of proparoxitones. According to the author, the adoption of an optimality framework, where a few violations of general principles are permitted, gives the analysis a greater explanatory value than traditional metrical theory. The constraints proposed are those already classic within OT, such as FOOT BINARITY, NONFINALITY, with the addition of ONE STRESS PER WORD and ANALYSIS BY MORPHEMES; the latter two give special attention to the morphological structure of the word. However, assuming the moraic trochee as pivot of the metrical structure is questionable, in Catalan as well as in Spanish. In my opinion, the data presented and discussed in all the articles considered so far challenge the traditional metrical account proposed for Iberian Romance languages and based crucially on QS and binary foot boundedness. As suggested by Trubetzkoy (1939), the sensitivity to syllable quantity in stress assignment is normally based on vowel length contrast. This was the case with Latin, whereas in Romance languages stress is lexical, although in part morphologically constrained.

The contribution by García Bellido may be considered as a bridge between the two main topics dealt with in the book, that is stress and intonation. In her study the relationship between inherent and structural prominence in Spanish is indeed investigated according to the autosegmental theory proposed by Beckman & Pierrehumbert (1986). The alignment of the pitch accents with the prosodic constituents sheds light on aspects of stress structure traditionally problematic, such as proparoxitones or clitic adjunction.

The studies on intonation are all based mainly on the quoted auto-

segmental framework. Elordieta investigates the prosodic system of Lekeitio Basque, assuming a lexical pitch accent of the shape H* + L as a phonological property of the word level. At higher prosodic levels, the accentual phrase is characterized by an initial L% boundary tone followed by a complex pitch accent H* + L. An interesting outcome of this long contribution concerns the lack of isomorphism between syntactic and intonational constituents: the experimental data show that an accentual phrase may span more than the relative syntactic phrase. However, the weight of syntax in the prosodic hierarchy is widely recognized; for instance, every syntactic maximal projection constitutes an intermediate phrase, whose break has the effect of blocking the catathesis. The intonational contours of the major types of sentences in Lekeitio Basque are discussed in the final part of the article. The most intriguing result is perhaps a final F_0 lowering in *wh*-questions and yes/no interrogatives, while only *wh*-echo questions exhibit a raising at the end of the utterance. The relevant cue for the discrimination between declarative and interrogatives sentences is therefore the initial H% pitch accent, which shows a higher value in the latter case. Duration plays a role too, since the final syllable of an interrogative utterance is significantly lengthened. The interpretation of the numerous figures relative to F_0 contours would have been easier for the reader if coupled with sentence transcription. Moreover, it is not clear which autosegmental notation the author has followed: in the first 24 figures (relative to declarative sentences), each syllable, both stressed and unstressed, is associated with a tone, whereas in the following 21 figures only the boundary tones and the complex pitch accent (H* + L) are singled out.

The aim of Prieto's study is to discover how Catalan speakers can disambiguate sentences involving right-branching or left-branching syntactic structures. A set of ambiguous utterances like *la vella llança l'amenaça* ('The old lady threatens him/her' or 'The old lance threatens him/her') have been recorded and then acoustically analyzed within the theory of intonational phonology. The experimental results are in line with previous analogous studies relative to other languages inasmuch as the syntactic boundaries constrain the intonational phrasing. In Catalan in particular, an obligatory high boundary tone on the stressed syllable marks the syntactic break, thus allowing the disambiguation of the utterance. According to the author, this finding, together with the lack of F_0 scaling in constituent-final accents, confirms the so-called Strict Layer Hypothesis. Unexpectedly, the duration patterns of the two branching structures are found to be similar. However, the data concerning duration have been measured in only one speaker, so we might wonder whether the results could be different for other subjects. A final note: is the mean syllable duration really given in ms in table 3?

The contribution by Frota may be considered as the first attempt at a formal characterization of the phonological prosody in European Portuguese (EP). The primary topic investigated is focus. In EP it is characterized by a

H*+L pitch accent, instead of H+L*, which is the neutral declarative nucleus. The starred tone is therefore associated with the focused constituent labelled as strong in prosodic structure. On the other hand, focus does not change prosodic phrasing in EP, unlike in other languages. The different relation between focus and phrasing allows the author to group languages into two major types: the morpho-syntactic type (e.g. Hungarian and Hausa), with focus having an obligatory phrasing effect, and the phonological type, in which stress and intonation suffice to mark a constituent as focused (e.g. EP, English and Italian).

Only a mention can be given to the articles not yet discussed. Hualde investigates palatalization in Biscayan Basque, comparing a rule-based approach with a constraint-based approach as in OT, and coming to the conclusion that phonological processes are better understood as well-formedness constraints than as rules. Bonet & Mascarò analyze the distribution of trills and flaps in the Iberian Romance languages, claiming that the flap, and not the trill, has to be considered as the more marked segment. Lloret examines the properties of sonorant dissimilation in different Iberian languages within the framework of feature geometry, arguing for a subtle but convincing hierarchy of sonorant features. Spirantization processes occurring in Catalan, Spanish and Portuguese are considered as the product of [continuant] spreading in the contribution by Palmada, who makes crucial reference to the Oral Cavity node.

Epenthesis and deletion occurring in Galician are investigated by Colina within the OT framework. A different ranking of the constraints on syllable constituents is able to predict the right context for both processes: deletion is preferred word-medially, while epenthesis is preferred in initial and final position. Final epenthesis in Galician is studied specifically in the contribution by Martinez-Gil, who distinguishes between two different kinds of epenthesis; one is optional, conditioned by prosodic factors, the other obligatory and lexical, governed by syllable structure. From the historical point of view, both the lexical and the optional types of final epenthesis constitute a case of rule inversion.

The analysis of Portuguese pluralization by Morales-Front & Holt is also conducted in OT terms. The authors argue that the surface alternations in plural nominal forms arise from the conflict between morphological and prosodic constraints. Although pluralization is a simple process of concatenation of an *-s* morpheme, the candidates are chosen on the basis of syllabic and metrical appropriateness. Redenbarger's contribution concerns the morphophonological processes of apocope and lenition in Portuguese verbal and nominal forms. Crucial reference to the interaction between Word Formation Rules and Phonological Rules is made. Harris analyzes the verb forms of imperative sentences in Spanish within the framework of Distributed Morphology, while Pensado shows that the Spanish depalatalization of /ʎ/ and /ɲ/ is not a productive phonological rule, but rather is governed by a

general principle of transparency. Finally, coda variability in Caribbean Spanish dialects is interpreted by Guitart in the light of the multilectalism of the speakers. Uneven control over the different modes of pronouncing coda consonants is the reason for the variability observed.

This book edited by Martínez-Gil and Morales-Front shows several points of interest, both on the descriptive and theoretical sides. The variety of the topics discussed does not prevent it from reaching a substantial uniformity; rather, it becomes an advantage, inasmuch as the reader may enlarge his knowledge of the Iberian languages. The book is well produced, with very few misprints. It is recommended for university libraries as well as for scholars of phonology.

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Iggy Roca (ed.), *Derivations and constraints in phonology*. Oxford: Clarendon Press, 1997. Pp. xii + 601.

Reviewed by CHARLES REISS, Concordia University, Montréal

Exactly half of the papers in this collection derive from presentations at a workshop at the University of Essex in 1995. Except for chapter 17, 'Non-transparent constraint effects in Gere: from cycles to derivations' by Carole Paradis, the constraint-based papers assume some version of Optimality Theory (OT). However, in the spirit of the unbridled chaos reigning in the phonological realm, it is possible to find among these eighteen papers enough combinations and permutations of both rules and constraints, and derivational and non-derivational systems to please the most theoretically promiscuous phonologist around. Given the youth of OT at the time of the workshop, one might expect the current volume to be already somewhat obsolete. This is not the case – OT has not solved the opacity problem in an

insightful manner, there is still debate over whether separate strata of constraints are required, and the scourge of functionalism continues to stalk the phonological landscape. Several papers in the volume are noteworthy in that they transcend the declared theme of comparing rule-based and constraint based theories by offering theory-independent insights. Rather than discussing all the papers, I have chosen to comment in more detail on a few representative ones.

Iggy Roca's first chapter 'Derivations or constraints, or derivations and constraints?' is an evenhanded overview of phonological theory including a thumbnail sketch of OT, rule-ordering and opacity, and how the remaining chapters address these issues. Nicholas Sherrard is not to be envied his job of providing in chapter 2, 'Questions of priorities: an introductory overview of optimality theory in phonology', an introduction to OT, with some of its brief history. This survey is based largely on unpublished (and thus unrefereed) work from the *Rutgers Optimality Archive* (http://ruccs.rutgers.edu/roa.html). I recommend these first two chapters to anyone trying to get a handle on the field.

Chapter 14, 'Correspondence and compositionality: The Ga-gyô variation in Japanese phonology' by Junko Itô & Armin Mester makes bold claims concerning the superiority of non-derivational OT over its competitors. To account for the complex distribution of [g] and [ŋ] in Japanese, the authors invoke a series of devices outside the domain of 'orthodox' OT, in which the grammar contains a fully ranked set of constraints which either demand input/output faithfulness or surface wellformedness. Itô & Mester posit an output-output correspondence constraint, IdentSS, which requires identity between SEGMENTS in morphologically related surface forms. Unfortunately Itô & Mester do not define the segment, and it remains unclear whether both featural and segmental faithfulness constraints are necessary. (The importance of developing an explicit account of the semantics of a notational system (e.g. What is a segment?) is discussed by Sylvain Bromberger & Morris Halle in chapter 3, 'The content of phonological signs'.)

In Itô & Mester's model, the second member of a compound is in a correspondence relation with the free form of the same morpheme, if such a free form exists (implying, thus, that free forms must be derived in parallel with bound forms – this issue is not discussed in the paper). Unfortunately the existing output-output analyses that Itô & Mester build on are fatally flawed. For example, the phase alternations of Rotuman are phonologically triggered, not governed by 'syntactico-semantic' conditions as assumed by McCarthy 1996 (see Hale, Kissock & Reiss 1998 for critical discussion of the output-output correspondence literature). Furthermore, Itô & Mester's assumptions concerning the structure of the Japanese lexicon and the use of diacritics to distinguish lexical strata have been shown to be untenable by Rice (1997) and by Sharon Inkelas, Orhan Orgun & Cheryl Zoll in chapter 13 (see below).

Itô & Mester attempt to model apparent free-variation between [g] and [ŋ] in certain contexts. They do not entertain an extragrammatical explanation (register switching, etc.) but rather claim that the choice is 'left open by the grammar' (432). Thus, their view precludes research into what actually does trigger the variation. Note also that by proposing a non-deterministic grammar, they reject without comment a well-established tradition of viewing grammars as functions mapping inputs to outputs, since the mathematical definition of a function requires that a given input have a unique output.

In order to allow the grammar its indeterminacy, Itô & Mester call upon two separate devices: multiple inputs and free ranking. The use of multiple inputs (underlying representations) has several problems including rendering obscure the term 'morpheme'. The multiple inputs (some with /g/ and some with /ŋ/) are, lo and behold! able to generate variation between surface [g] and [ŋ]. This position is not unreasonable, and it may even be correct (though it would help to have a theory of what determines the use of one version of the morpheme in a particular case), but their evaluation of competing theories loses credibility when they criticise a rule-based account for relying on the existence of underlying /ŋ/, since, they insist here, 'there is no underlying /ŋ/ in Japanese' – the rule responsible for the distribution of [g] and [ŋ] is '(semi-) allophonic' [sic] (446). A coherent comparison to their OT proposal would obviously allow for the possibility of underlying /ŋ/ in a rule-based grammar.

Under free ranking, the constraint set is not fully ranked and outputs derived from any of the undetermined rankings are considered grammatical. Again, in the absence of a theory of how this randomization works, the idea of free ranking is too unconstrained to be interesting – is the variation not dependent on speaker or social context or elicitation method? This paper leaves so many questions unanswered, and so many assumptions unexplored, that it is impossible to accept it as the 'streamlined OT analysis' (422) the authors promise.

Probably the most important contribution is chapter 13, 'The implications of lexical exceptions for the nature of grammar' by Inkelas, Orgun & Zoll. The claim that the paper demonstrates the superiority of OT over rule-based phonology is unfounded, but the authors raise a more important question: What is phonology a theory of? This paper might have been entitled 'The resurrection of the null hypothesis' because it reminds us that whatever is idiosyncratic and unpredictable in the output of the phonology should be 'prespecified' in underlying representations. The authors argue convincingly against morpheme-specific co-phonologies to account for lexical exceptions to alternations and static patterns. In other words, phonologists should not waste time trying to model in the grammar what should be relegated to the lexicon.

It is interesting to note that this paper and the one by Itô & Mester both

tout the superiority of OT, yet they are at odds in some of the most basic parts of their analyses. Itô & Mester disapprove of the use of underspecification, including a ternary distinction of binary features (+/−/∅) to 'diacritically' pre-specify underlying representations so that they 'come out right'. However, they are not averse to dividing the lexicon into various strata. Only OT, they argue, can incorporate these views in an insightful manner, and thus these assumptions lead them to proclaim the superiority of OT. Inkelas, Orgun & Zoll, in contrast, demonstrate the NECESSITY of prespecification involving a ternary distinction of binary features, and they categorically reject the type of lexical stratification that Itô & Mester propose. They similarly invoke their results in support of OT.

In chapter 4, 'Expressing phonetic naturalness in phonology', Scott Myers argues that the fact that so much of phonology can be 'explained' in terms of phonetics means that we should derive the phonological constraints from phonetic facts. Myers' proposal fails to distinguish the set of computationally possible grammars from the more restricted subset of grammars that could possibly arise diachronically. In other words, Myers' approach can never lead to a full understanding of phonology as computation. This approach has been criticized for cognitive science in general by Pylyshyn (1984: 207): 'the observed constraint on [a system's] behavior [may be] due not to its intrinsic capability but to what its states represent'. If we are interested in studying the phonology 'computer' then we need to distinguish a POSSIBLE phonological computation from an IMPOSSIBLE one. The set of attested phonological patterns and their distribution may be somewhat skewed by the accidents of language change, as lucidly discussed by Juliette Blevins in chapter 7, 'Rules in optimality theory: two case studies'. Real explanation of the nature of phonological computation requires us to see beyond such epiphenomena as 'markedness tendencies'.

A further problem arises when Myers attempts to explain sound patterns in language by appealing to the competing forces of 'discriminability' and 'expenditure of effort'. Note that 'effort' is typically not defined in functionalist work, and even if it were, it is not clear that we should believe that humans strive to minimize their expenditure of effort – consider Don Juan, Mother Teresa and Sir Edmund Hillary. An additional problem with such functionalist models (as well as other non-scientific theories based on a struggle between competing forces like Yin and Yang, Light and Darkness, Good and Evil, etc.) is that we can turn them around to contain principles like OBFUSCATE (confuse the listener – instead of increase discriminability) and NO PAIN-NO GAIN (put out some effort – instead of minimize effort). We then find that these 'dysfunctionalist' principles generate the same types of grammars as the functionalist ones.

Myers claims that certain types of behavior, such as better performance at hitting targets in careful speech, provide evidence that 'phonological representation[s] are phonetic *targets*' (130). This confuses the competence/

performance distinction, and it also entails a glib rejection of the possibility that grammar may be informationally encapsulated, as supposed under the modularity hypothesis. Speakers can do many things to disambiguate their message, even pronounce *knight* as [knajt] or point with their fingers, if they think it will help. There is no reason to believe, however, that this reflects access to their grammars. Furthermore, it is clear that phonology cannot be derived from phonetics, since phonetics relies on the logically prior categories of phonology in order to make any generalizations at all (Hammarberg 1976).

Myers proposes that a 'speaker's knowledge of what is difficult to produce or to perceive is directly incorporated into the grammar as criteria for the evaluation of potential phonological representations' (146). Consider an alternative account of how a learner figures out what patterns are present in the target language: s/he listens to it. This brings us to an evaluation of the oft-repeated claim that OT's universal constraint set aids the learner in the acquisition process. What advantage does a child have in learning the target language when innately endowed with a universal constraint like *[−bk, +rd] ('no front round vowels')? Born into an English-speaking environment, such a child will have this knowledge, but never be able to put it to any use at all since the primary linguistic data contains no front, rounded vowels. Born into a French-speaking environment, the child also 'knows' that front, rounded vowels are 'marked', but alas, s/he must be resigned to a life of simultaneous labial and coronal exertion. From the learner's point of view (as well as from the linguist's) a theory without a constraint that is either irrelevant (for English) or misleading (for French) is to be preferred.

Chapter 5, 'Gradient retreat', by Douglas Pulleyblank & William Turkel, is committed to acquisition and learnability issues. The paper contains some interesting ideas concerning the modeling of acquisition in OT, but it suffers from fundamental problems. First, in considering a proposal that all Faithfulness constraints must outrank all Wellformedness constraints at the initial state of the grammar, they come to the invalid conclusion that 'to assume that all inputs are identical to observed outputs is effectively to not assume inputs at all' (164). Of course, what input-output identity really means is that there is no phonology, so to speak – no rules, or in OT terms no Faithfulness constraints dominated by 'competing' Wellformedness constraints.

These authors also propagate an error found in (at least some of) their sources. They propose that children, WITHOUT LEARNING ANY LEXICAL ITEMS, begin to distinguish 'the acoustic properties that serve to contrast a given language's phonemes' (164). Given the definition of 'contrastive' (e.g. phonemes or features are contrastive if they can signal a difference in meaning) this position is not coherent: 'In general, it should be observed that ''minimal pair'' is not an elementary notion. It cannot be defined in phonetic

terms, but only in terms of a completed phonemic analysis' (Chomsky 1964: 97; see also Hammarberg 1976).

Blevins' paper and '*r*, hypercorrection, and the elsewhere condition' by Morris Halle & William J. Idsardi both discuss McCarthy's (1993) notorious OT account of [r] insertion in Eastern Massachusetts English. As McCarthy himself notes, 'r is demonstrably not the default consonant in English' (1993: 189). That is, it is not the maximally unmarked consonant that an OT account predicts would emerge in such a situation. In order to account for the insertion of [r], McCarthy proposes a special RULE of *r*-insertion: 'a phonologically arbitrary stipulation, one that is outside the system of Optimality' (1993: 190). Halle & Idsardi point out that this solution 'is equivalent to giving up on the whole enterprise' (337). Blevins is more accommodating. She first provides an excellent discussion of how such phonologically arbitrary processes arise diachronically. Since such phenomena are incompatible with a phonology restricted to a universal constraint set, she develops a version of OT that incorporates learned rules as a 'periphery' to the universal constraints at the 'core' of the grammar.

Blevins convincingly demonstrates that synchronic phonology needs to be able to construct arbitrary rules due to the accidents of history. However, she does not show that we need OT at all. She states that OT can account for 'conspiracies, emergence of the unmarked, and the general property of languages to instantiate certain phonotactic targets again and again by distinct strategies' (228). Note that the first and third of these properties are basically the same – there may appear to be multiple mechanisms leading to a systematic distinction between the shape of underlying representations and the shape of surface forms. Given the vagueness of terms like 'again and again' and 'conspiracy' (how many rules make a conspiracy?), these two properties cannot be evaluated scientifically. The notion of 'emergence of the unmarked', especially in its application to child phonology, derives from a confusion of linguistic competence and performance, as well as from a failure to recognize that children's early representations must be quite rich (see Hale & Reiss 1998). So a question arises: if we need language-specific rules, and we don't have any strong motivation for constraints of the OT type (e.g. learnability considerations), and OT cannot (yet) generate opacity in a straightforward manner, then why should we entertain OT at all?

In line with the transcendent goal of delimiting phonological theory, Blevins provides a reanalysis of Prince & Smolensky's (1993) account of vowel deletion in Lardil nominatives. She rejects their somewhat mysterious proposal that this process is a 'slightly altered form of a universal phonological constraint' (228) viz., 'Word-final vowels must not be parsed (in the nominative)'. Instead she adopts the reasonable view that Lardil has a language-specific morphological process of nominative formation. In addition to showing how this process arose via diachronic rule inversion, she addresses the importance of constraining OT, or at least being explicit in

modeling the interaction of phonological and morphological processes/constraints. Note that Itô & Mester dub these central problems 'some distracting technical complications' of their analysis (435, fn. 17).

OT has revived interest in phonology and especially in important areas like acquisition and learnability. However, many of the claims of superiority for OT are unfounded, and the comparisons to rule-based systems are spurious. It is no surprise if a vague, unprincipled theory can 'account for' data that a constrained theory cannot (though it is not clear that this is shown by any of the authors in this volume). We should compare explicit, well-constrained theories which use rules to explicit, well-constrained theories which use constraints.

One could argue that the apparent sociological success of OT reflects more a lack of intellectual integrity and rigor on the part of the phonological community than any conceptual or explanatory advantage of the theory itself. Clearly, we need to maintain scientific standards if change is going to be accompanied by progress. Of course, none of this criticism of OT is meant to imply that rule-based phonology is without its flaws – the question is whether OT is the right solution. There is a lot of interesting discussion and good linguistics in this book, some of it in the articles I did not mention, but any reader who finds the arguments for OT to be convincing should read more carefully.

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Pieter A. M. Seuren, *Semantic Syntax*. Oxford: Blackwell Publishers, 1996. Pp. xv + 379.

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This book covers a vast array of constructions which are at the forefront of current theoretical discussions. After providing some historical and intellectual context to the framework adopted in chapter 1, and giving formal definitions of the main notions and operations in chapter 2, Seuren launches into the analysis of the auxiliary and complementation system of English (chapter 3), French (chapter 4), Dutch (chapter 5), German (chapter 6), with detailed discussions of passivization, dativization, question formation, the placement of negation and of adverbs, the distribution of modals, 'easy-to-please' constructions, subject raising, predicate raising of causative verbs, clitic movement, tense marking, basic orders (SVO, SOV?) and counterfactuals. There is even a seventh chapter that deals with 'a few left-overs' such as quantification, clefting, conjunction reduction, subordinate clauses other than complement clauses and, finally, a quick glance at some Turkish data.

What the book intends to cover is therefore quite ambitious. But even more ambitious, and surprising, is how Seuren intends to analyze this material. As he indicates in the first paragraph of the Preface, *Semantic Syntax* is 'a direct continuation of what used to be called, since the late '60s, Generative Semantics' (xi). With construction-specific rules galore, the analyses give the impression of travelling back into time, to some 30 years ago, complete with the politics and the tone of the period to boot. This will have many raise the question of why one should read this material at all, especially since reading on for just a few pages may at first give the impression that the author is out of touch with much of what has happened in linguistics in the last three decades: works that have become standards in the discussion of the phenomena listed above are conspicuously absent from the references.

I suspect that many of these omissions are intentional, since Seuren seems to have a rather poor opinion of much of what is currently going on in syntax. For instance, the following passage from his discussion of thematic functions is indicative:

> There are serious reasons for assuming that argument functions alone are not sufficient to catch the regularities found in syntactic processes. It seems for example, that the subject term of true intransitive verbs (also called, unappealingly, "unaccusatives" in a particular school), such as *arrive*, *die*, *be born*, may, in some languages, show different syntactic behaviour from that of quasi-intransitive verbs (called "unergatives" in that school, equally unappealingly), such as *sleep*, *laugh*, *jump* or *live*. These latter verbs

> allow for paraphrases with an internal object, such as, respectively, *have a sleep*, *have a laugh*, *take a jump*, *have a life*. Yet whether the syntactic phenomena that have been observed in this connection are indeed adequately explained by the distinction between ''unaccusatives'' and ''unergatives'' is very much a moot point, due to the lack of independent criteria for the distinction. (25–26)

This passage brings out several aspects of Seuren's work. First, he appears to be aware of fairly recent proposals about thematic functions. He obviously does not approve of these proposals: he is entitled to his opinion and it may be the case that there are serious flaws in these proposals. But without even a sketchy discussion of precise problems in the proposals by authors from as diverse backgrounds as Perlmutter (1978), Burzio (1985), Jackendoff (1990), Levin & Rappaport Hovav (1995), to name a few, this is no more than a vague expression of an opinion, and is not helpful. A second aspect of the work reflected in this passage is the level of argumentation. Too often, Seuren relies on vague paraphrases and bases his proposals on impressions rather than facts. For instance, unaccusative verbs are just as subject to paraphrasing (*make an arrival*, *meet one's death* or *drop dead*, *prendre naissance* (for some uses of French *naître*): why then aren't these paraphrases as significant as the ones given for unergative verbs? A third property of the quoted passage is its unfortunate tone, which is recurrent in the book.

However, there may be a very good reason to carry on reading, despite the gaps in the references, since there is another aspect of this work which is even more surprising. Despite the superficial differences due to the fact that this framework appears to be frozen in time, as is reflected in its terminology and its theoretical tools, the results it obtains and the way in which they are obtained are eerily similar to what is found in current approaches like Principles & Parameters and the Minimalist Program. Once one realizes this, the book becomes a fascinating challenge. As the reader goes through one analysis after another and compares them with current analyses, a haunting paradox emerges. The advocates of Semantic Syntax and of Principles & Parameters emphasize that their conceptualizations of grammatical theory are highly different, the former praising the 'old values', the latter dwelling at length on the conceptual gains made in the last thirty years (this is crystallized in the introductions of *Semantic Syntax* and of Chomsky 1995). But why is it that, if we concentrate on the mechanisms that actually make the analyses work rather than the surface presentation of the theories, the end results are so similar? A thorough comparison of how these results are obtained may help us get a better measure of the accomplishments made.

The distribution of markers of sentential negation have been widely discussed in recent years and will serve as a good illustration of how this book may be used as a measure of progress of theorizing. Restricting ourselves to English and French, a first similarity between the analysis put

forward in Semantic Syntax (SemSyn) and in Minimalism is the unique (maybe universal) position for Neg in the syntactic level which feeds semantic interpretation: because of its scopal properties and the c-command conditions to which they are assumed to be subjected, Neg is fairly high in the structure, a sister to a sentential projection at Deep Structure for SemSyn, a projection above VP but below TP in Minimalism. On the surface, this does not correspond to the distribution of Neg in French and English. Both analyses have the same corrective device to account for this: a transformational operation that relates the surface and an interpretive level. Although the vocabulary to describe the operation is quite different in the two analyses, these are just the two logical ways, two symmetrical perspectives from which this same process may be viewed: lowering A below B, or raising B above A. SemSyn adopts the lowering perspective: Neg is displaced from its interpretive position and lowered below the 'Aux complex' in both French and English. Minimalism adopts the raising perspective: the V is raised in French overtly in French, covertly in English. But where one begins is not of fundamental importance in a symmetrical relation: it is just a presentational artifact and the apparent differences that it introduces should not distract us from the basic similarities.

Both analyses try to link the surface differences between French and English to morphological differences between the two languages. In SemSyn, the richer morphology of French introduces an additional affix node in the syntax: this modifies what counts as an Aux complex, making it crucially include the main verb, so that lowering Neg below the Aux complex puts it below the main verb; in English, the main verb is outside the Aux complex, so Neg appears above it and prevents it from linking with the tense affix: this triggers *do* support. In Minimalism, the richer morphology of French must be checked on the surface, so it triggers a movement of the V above Neg; in English, morphology is not strong enough to trigger movement of the V: *do* support is a means to realize the stranded inflection. Both analyses are only quite indirectly linked to the morphological properties. In particular, the triggering process is not morphological. In SemSyn, lowering of Neg below the Aux complex is a lexical feature of the lexical negative item. In Minimalism, the feature in T that attracts the V is also a second order feature, a feature on categorial features (either [+strong] or a non-inherent feature added to T), and 'the sole function of these feature checkers is to force movement' (Chomsky 1995: 278). It therefore turns out that in both approaches, the key feature of an intricate analytical apparatus – the trigger for movements – is a stipulation, an *ad hoc* auxiliary assumption with the sole purpose of protecting the theory from a threatening falsification – a surface order that does not correspond to the way scopal properties are represented.

The motivations for displacement operations are the same in both analyses. Because of the structural conditions assumed to hold for scopal

properties, it is not possible to do surface semantics (21). Moreover, these structural conditions are assumed to be universal, so some means to reach identical interpretive structures must be proposed, given that '[t]he languages of the world show vast structural differences for the expression of identical or near-identical meanings' (29). The concept of a structural universal is dependent on the use of paraphrase as a heuristic principle, which dates back to traditional grammarians and was introduced in Generative Grammar by Katz & Postal (1964). It is assumed that 'there is always a paraphrase, taken to be a more precise and more analytical rendering of the meaning of the sentence in question' (33). These intuitive paraphrases form the basis to 'construct grammatical rules that relate the original sentence and its paraphrases in such a way that each of these sentences has the same sequence of underlying P-markers' (Katz & Postal 1964: 157). However, the use of intuitive paraphrases is a very poor heuristic principle: it is based on the idea that 'two elements with the same use tend to receive the same semantic interpretation. But [...] expressions that are equivalent in some of their uses are not conceptually equivalent. [I]t is not possible for two paraphrases to be exactly equivalent. Moreover, no clear criteria are given to determine whether or not a paraphrase is indicative of an underlying semantic representation' (Bouchard 1995: 46). The reason why scopal and many other semantic properties are seen as STRUCTURAL universals is that sentences that are near semantic equivalents – translation equivalents – tend to be represented as a paraphrase in one of the languages, typically a language with syntactic structure as a dominant mode of coding information because of the native languages of those presently leading dominant schools of thought (as opposed to Latinists of some time ago: abstract case is an interesting remnant of that influence).

In models with universals strongly based on structure, such as SemSyn and Minimalism, a special status is given to the linear (temporal) ordering of sets of sounds (words or constituents) in the auditory-oral channel of the sensorimotor apparatus (see Kayne 1994 and Chomsky (1995: 335–340)). To have the computational system determined by temporal ordering is understandable as an early hypothesis about language, given that linguistic elements end up ordered in time with respect to others in actual speech. However, one must not confuse the functional aspect of order – the use of order to express that a relation holds between two elements – and the articulatory aspect to order: some order is required in oral languages since we cannot produce more than one sound at a time, and there is a low limit on simultaneous signs in sign languages due to the restricted number of articulators. This is a contingent property of language production, distinct from the first, and is due to bare output conditions of our articulatory-perceptual system. On the other hand, the functional use of order is by no means a necessary property of language: there are other means allowed by the articulatory–perceptual interface which a language could use to convey

information about the combination of constituents, such as morphological markings, intonation, various uses of space in sign languages. Therefore, it should not be assumed a priori that the functional use of order is a universal property of language, nor that all languages have a basic order, and even less that there is a single universal order for all languages. These auxiliary hypotheses are conceptually costly, since they depart from the ideal situation in which all and only the conceptually necessary choices are allowed. Indeed, if order is but an artifact of the medium of articulation, as is implicitly acknowledged by Chomsky (1995: 334), it should not be attributed a foundational role in the computational system.

Semantic Syntax may be a very useful tool to measure the progress made in linguistic theory, and to identify areas where things got stuck. To get out of the rut, what may be required is a level of semantic analysis far more abstract than what the paraphrase strategy suggests, and a notion of universal removed from contingent properties of particular languages, such as a high use of structural representation.

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