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SHORTER NOTICES

John Archibald (ed.), *Phonological acquisition and phonological theory*, Hillsdale, NJ: Lawrence Erlbaum & Assoc. 1995. Pp. xxi + 204.

Reviewed by PATRICIA DONEGAN, University of Hawai'i at Mānoa

Recent interest in language learnability has prompted interaction between acquisition studies and syntactic theory; *Phonological acquisition and phonological theory* aims to provide a similar interchange between acquisition studies and phonology. It includes nine chapters, plus a preface and introduction by the editor, a unified author index, and a subject index. The volume is attractively printed, and it is generally free from typographical errors.[1]

The authors share a clear idea of what the object of phonological acquisition is; their theoretical framework is that of generative phonology, with a focus on autosegmental, feature-geometric, underspecified, and prosodic representations. The volume is a much-needed attempt to interrelate the representations of modern phonological theory, the principles and parameters approach to acquisition, and the problems encountered by a child acquiring a first-language phonology, but the chapters reveal some difficulties in achieving the desired synthesis.

John Archibald's introduction describes phonology as a system of interconnected levels of representation which also includes processes that map one type of representation onto another. His emphasis on representations and his view of phonology as a system entirely independent of phonetics are consistent with the rest of the volume.

Elan Dresher & Harry van der Hulst discuss 'global determinacy' – their term for the cases where a single phonetic form corresponds to multiple possible phonological representations and a 'global' knowledge of the language is required to determine a phonological representation. They cite various sources of global determinacy in modern phonological theory, including abstract underlying representations, floating material, underspecification, and variable grouping and dependency. These are problems of learnability 'because a lot must be known about the phonology of specific languages in order to determine which representation is adequate' (17). They note that much modern phonology assumes that the main burden of explanation rests with representations, not rules, and that the rules should follow from the segmental inventory, and they add that other hypotheses are implied: that representations can be established before any rules have been learned, and that representations increase in complexity by adding contrast. Dresher and van der Hulst formulate questions of learnability which phonological analyses should answer, but they offer no solutions here.

The authors who deal with segmental acquisition (Keren Rice & Peter Avery, David Ingram, E. Jane Fee, Daniel A. Dinnsen & Steven B. Chin) almost uniformly attribute children's production errors to limitations on their phonological representations. However, they do not reconcile these limited representations with children's perceptual accuracy, or interpret what this accuracy implies about children's phonologies. Some of the authors (like Rice & Avery) disregard this problem; others (Dinnsen & Chin) take it seriously. Most assume that adults' and children's phonological representations are underspecified, but none actually present any evidence from acquisition data for underspecification. In particular, no evidence is provided for the sorts of perceptual confusion that underspecification would be expected to cause.

Underspecification is accepted here despite the fact that it makes incorrect predictions about children's perceptual abilities, differential phonological effects of sounds that children pronounce the same, and the nature of changes in children's productions. A child with representations which lack information regarding Place, for example, should fail to distinguish *tea* from *key* perceptually, but does not. Identical (underspecified) representations should not be treated differently by phonological rules, but they often are treated differently. And correction of an error pattern should correct inappropriate forms as well as the appropriate forms if their

[1] One surprising error, however, (84) involves 'McCawley (1977) following Hayes … 1987, p. 278)' in his definition of the mora – when, of course, it is Hayes who followed McCawley.

underlying representations do not differ, but this rarely happens (for example, in a child who says [t] for adult [k], [t] and [k] are both supposedly unspecified for Place, but when the child learns to pronounce velars, only the adult-[k] words change to [k]). These indications regarding children's representations have been pointed out repeatedly (Stampe 1969, 1973, Smith 1973, Barton 1978, etc.). Daniel A. Dinnsen & Steven B. Chin are to be credited for recognizing and describing these problems. But, having criticized underspecification, Dinnsen & Chin make a surprising attempt to save it with a theory of 'shadow specification'. They say that, for any merged pair (or series), the segments the child produces correctly are UNspecified for the distinguishing feature, and the segments the child produces incorrectly are 'shadow' specified for the distinguishing feature (for example [t] is unspecified for Place, and [k] is specified for Place as [Coronal]). This proposal is hard to swallow: the child's feature specification reflects his or her own pronunciation, BUT ONLY WHEN IT IS WRONG. That is, the child is said to specify a segment as coronal only when he or she knows it to be NON-coronal. Further, no basis is provided for the regularity of children's mispronunciations – since they are not determined by universal processes or by redundancy rules, we still need an explanation of the fact that a child specifies adult velars REGULARLY as coronals, and not randomly as coronals, labials, glottals, etc. The proposal also predicts that a child who produces a three-way merger (such as [t] for [p, t, k] or [t] for [t, θ, s] would mark both 'erroneous' segments with the same wrong feature ([Coronal] or [−Continuant], respectively). When a new articulation is required, a change rule that affected the mis-specified segments would necessarily change them all to the newly acquired sound – which does not always happen. When the only proposal in the volume which confronts the problems of underspecification has these effects, one may well look again at a possibility Dinnsen & Chin reject: that the child's underlying forms are accurately specified and that articulatory constraints cause substitutions that prevent their accurate realization.

Prosodic and tonal issues are not neglected: Jane Fee provides a discussion of how UG rules for building melodic and prosodic structures may dictate the shape of children's early productions, John Archibald surveys the acquisition of stress in first and second languages, focusing on whether stress is learned lexically or by rule and whether there are default parameter settings regarding foot type, extrametricality, etc., and Katherine Demuth presents a case study of the acquisition of some morphologically conditioned tone rules in Sesotho. Both Fee and Archibald propose a bisyllabic trochee as the unmarked metrical unit, instead of a bimoraic trochee, on the grounds that open syllables are more common in many children's productions; neither considers early monosyllables with long vowels as potential evidence for a bimoraic unit.

On the second-language acquisition front, Ellen Broselow and Hye-Bae Park present an interesting analysis of vowel epenthesis in the Korean pronunciation of English words, which considers Korean speakers' perception and production in terms of parameter settings, and Thomas Scovel offers a chapter on the discrimination of foreign accents.

The learnability questions that Dresher & van der Hulst raise, and the objections to underspecification outlined by Dinnsen & Chin are important problems for phonological theory and for acquisition, and it is not clear how they can be resolved. Questions about the relationship between perception and representation – how a phonetic signal is processed into a phonological representation – go unanswered (Rice & Avery) or are insufficiently answered (Ingram, Archibald). And important aspects of the relationship between phonological representation and production are glossed over (as in Fee's claim that although phonology constrains production, 'lexically learned' forms may be produced which violate UG constraints on phonological form (60)).

Through much of the volume, there is an assumption (stated most clearly by Ingram (78)) that the principles that govern the acquisition of phonology should be the same as those which govern the acquisition of syntax. Some learning principles may, of course, be common to both, but this assumption overlooks the more direct relation of phonology to the learner's perception and production abilities – to phonetics. The authors hold the learner's phonological system responsible for his production errors, but the features, representations, prosodic structures, and rules of this system are unconnected to their phonetic realizations, where many of the learner's difficulties lie. Default parameter settings and the redundancy rules for default feature specification are simply attributed to UG, as if no articulatory or perceptual explanation were involved. Phonetic inabilities (absence of particular phonetic skills) are not even considered as a potential source of negative phonological constraints. This isolation of phonology from phonetics is assumed without discussion.

Phonological theory and studies of language acquisition often seem unconnected; this book

works from the phonology side to bridge that gap. Most of the chapters are attempts to show how the segmental, prosodic or tonal systems of a current model of phonology can be acquired. The attempt is welcome, but the bridge is incomplete; many difficulties regarding the connection remain.

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Noam Chomsky, *Language and thought*. Wakefield, RI & London: Moyer Bell, 1993. Pp. 96.

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This book consists of a lecture by Chomsky, a series of responses by Eric Wanner, Akeel Bilgrami, James Schwartz and George Miller, and Chomsky's reply to those. Chomsky's essay pursues not only the general themes which he has addressed throughout his career, but those aspects of them which he has been developing in recent years (cf. Chomsky 1991, 1992a, 1992b, 1995); it makes fascinating reading. Chomsky firstly addresses some general philosophical issues and then gives a general outline of his current conception of the language faculty. I will briefly outline the former and then consider the latter in more detail.

On the philosophical issues, Chomsky argues against Fregean externalism, the central tenets of which are the idea that language is external to the individual, and that each individual has an 'imperfect grasp' of that public language. Against this, Chomsky argues, as he always has, for a naturalistic internalism. The two main (and by now very familiar) ideas here are (a) that language is entirely internal to the organism; specifically, it is MIND-internal, and (b) that the scientific study of this aspect of the species constitutes the study of an aspect to the natural world. The version of naturalism which Chomsky seeks to defend (METHODOLOGICAL naturalism) is opposed to currently fashionable versions of physical reductionism.

To show this, Chomsky argues (as he has elsewhere) that Cartesian mental/physical dualism ran into difficulties, not because of problems with the notion 'mental', as has been commonly assumed in the twentieth century, but because the Cartesian 'clockwork', 'mechanistic', or 'contact' conception of the physical was shown, by Newton, to be untenable. Chomsky argues that we have, since then, yet to arrive at a coherent conception of the physical. Thus two sorts of naturalism are unformulable, according to Chomsky. The first is METAPHYSICAL naturalism of the sort adopted by Dennett, according to which accounts of the mental must be 'continuous with' and 'harmonious with', by which Dennet means REDUCIBLE TO the natural sciences AS WE NOW CONCEIVE OF THEM. The second is recent EPISTEMIC naturalism (also Baldwin's term), of the sort adopted by Quine, whereby the study of the mind must be incorporated within a narrow behaviourist psychology.

What Chomsky is (and always has been) arguing for is a present day version of EARLY MODERN epistemic naturalism, originating in the rationalist psychology of the seventeenth century. This version of epistemic naturalism postulates innately endowed mental faculties, and takes cognitive states to be aspects of the natural world.

Chomsky also addresses Descartes' problem, which can be put thus: if we admit of the reality of propositional attitudes, such as intentions, and other mental realities such as mental

representations, how do they relate to, on the one hand, externally observable behaviour and, on the other hand, neural events? For Chomsky, Descartes' problem is a problem of unification. Given that unification is, self-evidently, a desirable goal of the natural sciences, Chomsky's argument is that, in the history of the natural sciences, it is rarely the case that unification is achieved via reduction of one science to a more 'fundamental' science. The norm, he claims, is that the more fundamental sciences have to be re-conceived in the light of advances in the 'less fundamental' sciences. This is the core of his argument against reductive eliminative materialism and similar modern developments.

As far as Chomsky's outline of the language faculty is concerned, the main ideas will be familiar to most generative linguists. The language faculty is said to subsume a wide range of cognitive systems (too wide a range, one might argue). Its principal sub-parts are an I-language, a set of 'performance systems' (input/ouput systems, subsuming perceptual systems) and two interface levels (PF and LF) which constitute instructions to performance systems. An I-language is a cognitive system of a very specific sort, consisting of a computational mechanism and a lexicon. The I-language is

> a generative procedure that determines an infinite class of linguistic expressions, each a collection of instructions for performance systems. Particular signs, in the Fregean sense, are manifestations of linguistic expressions (spoken, written, signed, whatever); speech acts are manifestations of linguistic expressions in a broader sense. (49).

By 'sign in the Fregean sense', Chomsky means mind-external physical signals (he uses the terms 'sign' and 'signal' interchangeably). Note that a 'linguistic expression' as conceived of here is NOT a mind-external physical object or event (such as a physical sign/signal, an utterance). Rather, it is a collection of instructions, generated by the I-language, which may be accessed and implemented by performance systems. It is regrettable that Chomsky elsewhere in recent work uses 'expression' to mean mind-external object or event. For instance, Chomsky (1991: 11), having noted that American stucturalism took the units of language to be 'segments of utterances', then says that

> Generative grammar adopted a very different standpoint. From this point of view, expressions and their properties are just data, standing alongside other data, to be interpreted as evidence for determining the I-language, the real object of inquiry. The structural descriptions generated by the I-language are abstract representations of expressions.'.

Similarly, Chomsky (1992b: 211) says that 'we may take the linguistic expressions of a given I-language to be the S(tructural) D(escriptions) generated by it', while stating elsewhere that 'Jones has … capacities of mind that allow him to produce and interpret linguistic expresions' (1992a: 121). The inconsistency is clear. On the one hand, linguistic expressions are said to be mind-internal; specifically, they are structural descriptions, conceived of as instructions to performance systems. On the other hand, linguistic expressions are said to be mind-EXTERNAL, and are DISTINCT FROM structural descriptions, such that structural descriptions are DESCRIPTIONS OF linguistic expressions. (See also Chomsky (1995: 3) for this use of 'expression'. There, mind-external expressions are said to be assigned an interpretation.) Similarly, 'particular signs' are said to be MANIFESTATIONS OF linguistic expressions; on the other hand, they are said to BE linguistic expressions. The least one can accuse Chomsky of here is less-than-careful writing. But one worries that there is, in fact, conceptual equivocation underlying this (pervasive) inconsistency.

Evidence for such conceptual equivocation can be seen in a related part of Chomsky's current conception of I-language. Two aspects of that conception appear to be in conflict with one another. The first is that the computational mechanism within the I-language is said to be 'austere' (Chomsky 1995: 15) in the sense that it does not have access to the sorts of notion that the input/output systems may have access to (such as counting items in a sequence). The other is the idea that the expressions provided for the performance systems by the I-language are constituted as INSTRUCTIONS to those systems. This is expressed very clearly in his essay in this book:

> In the study of language, there is new understanding of the computational systems of the mind/brain, including those commonly called 'phonetic' or 'semantic', though in fact, all are 'syntactic' in the broader sense that they have to do with mental representations. A good deal is known about the acquisition of these systems, and about how perceptual-articulatory systems interpret and use the instructions they provide. (52)

But how is it possible to sustain the eminently desirable claim that the 'innermost' aspect of the language faculty 'is not tied to specific sensory modalities' when one is also claiming that the I-language contains a lexicon which 'is a collection of items, each a complex of properties (called "features"), such as the property "bilabial stop"' (Chomsky 1995:13), where such properties are conceived of as INSTRUCTIONS TO ARTICULATORS? Even if we could satisfactorily generalise over instructions to speech organs on the one hand and instructions to arms and hands on the other (as in 'sign' languages), it is not at all clear that those generalizations would take anything like the form of present day conceptions of phonological representations. Nor is it clear that those generalizations would be in any way narrowly linguistic in nature. Indeed, PF is a rather odd candidate for inclusion in the expressions generated by the 'innermost', 'purely syntactic' part of 'the language faculty'. If linguistic expressions contain anything as modality-specific as instructions to the organs of speech, it is hard to see how the 'austere' conception of an I-language can be sustained. Either the I-language is genuinely austere or it does not contain instructions to the speech organs. For, if it did, it would surely constitute part of a perceptual system, rather than part of I-language, conceived of as something distinct from perceptual systems.

If, by 'pure syntax' Chomsky really does mean only that the computational system 'has to do with mental representations', then that is a very weak sense of 'pure syntax', one that isn't at all austere. Most versions of Empiricism could accommodate computations over mental representations of the 'sound image' sort, arrived at solely on the basis of general (not specifically linguistic) cognitive capacities and repeated exposure to sensory input. Such acoustic images have to do with mental representations of a sort, but must surely fall outside of the intended scope of an 'austere' I-language. The European wren, with a brain weight of less than a gramme, presumably has very limited cognitive capacities, and yet, being a remarkably good mimic, must have acoustic images and instructions to its singing apparatus somewhere in its cognitive makeup. Whatever the nature of mental images of acoustic properties, and mental instructions to articulators, they cannot surely exist as part of an advanced cognitive state which is specifically linguistic and peculiar to homo sapiens. Given the vast amount of repetition of uses of the speech organs, 'instructions' to the speech organs must surely lie practically at the motor level, as, literally, 'speech habits', as behavioural abilities. But Chomsky has rightly rejected any such conception of the linguistic.

It is easier to conceive of a truly 'austere', strictly linguistic, cognitive capacity, and indeed a STRICTLY (rather than 'largely') invariant one, if it excludes images of acoustic properties and instructions to body parts. That would allow a genuinely narrow conception of modularity, narrower than the modularity of different input/output systems discussed by Jerry Fodor or David Marr (which is precisely what Chomsky wants). But the question then arises where the lexicon fits into the overall picture. One argument for excluding the lexicon from a strictly austere, purely syntactic, narrowly modular, linguistic capacity is this: as it currently stands, Chomsky claims that this linguistic capacity is a natural object, while at the same time allowing that it contains the lexicon, which is the locus of Saussurean arbitrariness, an entirely conventional, NON-natural relation (my thanks to Noël Burton-Roberts for putting this important point most strikingly). Furthermore, if PF is an interface level, and thus not located within I-language per se, and if the Saussurean arbitrariness relation holds between phonological forms and lexical meanings, then phonological forms within the lexicon must be something other than PF, but Chomsky's gives no indication as to what the distinction or relation between the two might be.

There is also a problem with the idea that the specifically linguistic cognitive system 'stores information', since the notion of 'storage' is a fundamentally Empiricist notion: what is 'stored' is first gathered, via the senses. And what is gathered and stored must be ontologically distinct from a specifically linguistic cognitive capacity that is innately endowed, which must surely be of a kind that is not adequately described either as 'information' or as being 'stored'.

Chomsky has consistently proposed a radical distinction between, on the one hand, a radically internalist conception of the linguistic and, on the other, observable behaviour. The question is: what is the relation between the two, between language understood in a narrowly modular way and observable speech events? For Chomsky, the relation is one of manifestation, (otherwise known as externalization, instantiation, realization, implementation or exponence). But the problem with this conception is that it commits us to allowing that mind-external events may be linguistic (cf. Burton-Roberts 1994). This is so since the 'intension/extension' conception of that relation which Chomsky adopts, as in 'I(ntensional)-language' vs. 'E(xtensional)-

language') is essentially a type/token conception, under which acoustic events, considered as tokens of linguistic types, are INSTANTIATIONS of those types, and are thus linguistic (have linguistic properties). This undermines radical internalism. It is arguable that, to sustain a genuinely radical internalism, Chomsky needs a different conception of the relation between, on the one hand, the radically internal linguistic knowledge (divorced from mental behaviour and sensory perception) which constitutes UG and, on the other hand, externally observable acoustic events.

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Paul Gorrell, *Syntax and parsing*. (Cambridge Studies in Linguistics **76**.) Cambridge: Cambridge University Press, 1995. Pp. xi + 180.

Reviewed by BRADLEY L. PRITCHETT, Cambridge, MA

Syntax and parsing, by Paul Gorrell, is a useful addition to the body of work which might be characterized as linguistically oriented psycholinguistics. What sets this research program apart from more traditional psycholinguistics is a serious attention to the structures and analyses proposed within theoretical linguistics, something which had until recently largely been abandoned among psychologists due to the numerous, albeit poorly motivated, attacks on the Derivational Theory of Complexity formulated decades ago.

Consequently, while bearing its influence, Gorrell's adopted approach squarely distinguishes itself from what has arguably been the single most influential theory of human sentence processing, the work of Lyn Frazier and colleagues (beginning with Frazier (1978)). While Frazier and her associates have long paid nominal lip service to syntax, close examination reveals that the actual models proposed have been essentially divorced from any serious linguistic theory from the start. This problem has only been exacerbated as syntactic theories have evolved while the familiar MINIMAL ATTACHMENT approach has remained static even in the light of numerous empirical difficulties, as Gorrell discusses.

In *Syntax & parsing,* Paul Gorrell investigates the principles that govern the recovery of configurational structure during on-line processing, holding such information to form a crucial component of the ultimate goal of comprehension. Indeed, if one takes linguistic theory seriously, this conclusion seems inescapable given that both semantic roles and logical relationships are determined configurationally. The author's primary data is drawn from the well established body of psycholinguistic evidence (including his own experimental work) concerning the interpretation of locally ambiguous sentences, that is, sentences which display a temporary ambiguity as considered word by word. As is well known, some such examples present no difficulty for the processor:

(1) The students knew the truth would set them free,

while some are powerful garden paths:

(2) Because the students knew the truth would set them free.

A brief introduction followed by a quick summary of the Government and Binding theory

constitute the first two chapters of the book. Along the way, the author also argues that two particular structural relationships, DOMINANCE and PRECEDENCE, should be considered primitive in the sense that they are themselves structurally irreducible and are sufficient for the statement of a variety of secondary constructs, such as government and c-command. This will become crucial to his parsing theory.

The third chapter reviews a variety of earlier models of human sentence processing, introducing a range of data his own approach will later be responsible for, with space devoted to a critical examination of three theories which very directly influence Gorrell's own, Pritchett's Generalized Theta-Attachment system (1987, 1992), Gibson's (1991) ranked parallel parser, and Weinberg's (1991) minimal commitment model. Gorrell ultimately adopts several fundamental principles of these theories, while also noting the areas where they encounter empirical difficulties.

The fourth chapter introduces the author's own particular model, which is steered by two fundamental principles:

(3) SIMPLICITY: No vacuous structure building.
(4) INCREMENTAL LICENSING: The parser attempts incrementally to satisfy the principles of grammar.

The first, SIMPLICITY, is clearly related to the minimal commitment approach of Weinberg. Minimal commitment has its own roots in Frazier's minimal attachment, which in turn owes a heavy debt to Kimball (1973). Gorrell, like Weinberg, adopts a form of determinism which stipulates that structural assertions once made may never be retracted. Because ambiguity and determinism are fundamentally at odds, the parser is given the leeway to build underspecified trees in terms of the predicates DOMINATE and PRECEDE. It is an innovation of Gorrell's to introduce statements of precedence in addition to the more commonly employed dominance.

The author's second principle, incremental licensing, is largely a reinterpretation of Pritchett's (1987, 1992) (Generalized) Theta-Attachment, which has been adopted in various forms by a number of other researchers as well. To a large extent, what is unique to Gorrell are not the principles he invokes, but the way he combines minimalism and licensing. An example may clarify.

Consider a local ambiguity as in example (1) above. Upon its appearance, the grammatical function of the NP, *the truth*, is indeterminate between direct object and subject of an as yet unidentified clause. In Pritchett's model, the DO attachment is made immediately in order to satisfy grammatical principles governing theta-role and Case assignment. Consequently, a fully specified partial parse tree is built in order to allow a local incremental interpretation. Gorrell's parser, however, is more conservative and simply asserts that there is a VP which dominates both the V, *know* as well as an NP, *the truth*. The parser also asserts that the V precedes the NP. Crucially, it turns out that these statements remain true whatever the ultimate grammatical role of *the truth*. Even if the NP proves to be the subject of an embedded clause, information about additional clausal nodes may be added later without falsifying any previous assertions.

Consider in contrast a garden path sentence such as (2) above. Again, the parser first asserts that there is a VP which dominates the verb *know* and the noun phrase *the truth*. The subsequent appearance of an additional predicate indicates that this is not the full story. In Pritchett's model this necessitates backtracking of a sort which violates an On-Line Locality Constraint, resulting in a garden path. In Gorrell's model, since structural assertions cannot be retracted, an irrecoverable garden path results since the statement that the VP headed by *know* dominates *the truth* cannot be revised.

As mentioned, the invocation of precedence is one factor which sets Gorrell's model apart from Weinberg's earlier work. The primary data which have led to this revision concern sentences claimed to be problematic for both Weinberg's minimal commitment model as well as Pritchett's backtracking approach. For the most part these involve an array of double complement verbs of various types (*give*, *warn*, *put*, etc.). While the correct treatment of these data are crucial, there is unfortunately not room to begin to debate individual cases here. Suffice it to say here that while Gorrell's analyses are always well reasoned, there remains ample room for counterargument, which is perhaps not surprising.[1]

[1] There are of course numerous other questions of data which could be raised not relating to double complement verbs. To mention only one instance, it is not apparent how the

Issues of empirical coverage aside, a fundamental theoretical question about the model concerns the interaction of structure building and interpretation. Gorrell's parser (like Weinberg's) employs dominance rather than direct dominance during structure building in order to adhere to a form of determinism. However, the processor is given by the Principle of Full Interpretation, similar to Pritchett's Generalized Theta-Attachment, and, consequently assigns theta roles, for example, on-line as required for incremental interpretation. However, this necessitates the tacit use of direct dominance by the parser since thematic roles (and Cases, etc.) are determined configurationally. Gorrell sidesteps this apparent paradox essentially by stipulating that the structure building component and structural interpretation component are separate – with the latter not subject to structural determinism. Thus the interpreter can essentially read dominance as direct dominance in the absence of conflicting information. This, however, is necessary only in order to allow incremental interpretation on-line while still adhering to the determinism hypothesis in order to account for the garden path data. As such, it has a feeling of a circularity. There seems to be a fundamental conflict between full interpretation and underspecification which no deterministic theory has addressed satisfactorily.

Overall, however, Gorrell's model is well thought out and cleverly combines some of the most successful aspects of previous parsing theories in order to extend their theoretical coverage. The fact that this approach buys so much ground based on previous models means that the work will be of particular interest to researchers already involved in the intricacies of those models and who are eager to examine the fairly narrow (albeit important) range of data which distinguishes them empirically. The book is also useful as a quick survey or review of several current theories of human sentence processing.

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Nelleke Oostdijk & Pieter de Haan (eds.), *Corpus-based research into language*. Amsterdam & Atlanta, GA: Rodopi, 1994. Pp. vii + 279.

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This volume of contributions, collected by the editors in honour of Jan Aarts, the prime mover of the TOSCA group at Nijmegen, is intended to 'cover those topics that Jan Aarts has always

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author would distinguish the overly familiar garden path in (i) from the equally ambiguous but perfectly acceptable example in (ii).

(i) The horse raced past the barn fell.
(ii) The spaceship destroyed in the battle disintegrated.

Both would appear to violate structural determinism (specifically, precedence) in the precisely same fashion yet must be handled quite differently.

had a keen interest in', particularly syntactic analysis. It does this, but in fact it does much more, providing the interested non-specialist with an excellent introduction to the rapidly expanding field of corpus-based research. Although a few of the papers are highly specialized, and a few (surprisingly few, given how fast the field is evolving) are outdated, the majority are accessible to a general audience with a modest familiarity with (computational) linguistic terminology and issues.

One caveat is required – the corpora under consideration in this book are all TEXT corpora. Spoken language figures significantly only in the last two papers, and then on the basis of transcripts, not digitally sampled speech itself. In this one area the book shows its age: I dare say one would be unlikely now to study disfluencies without reference to the actual speech, and with the advantage of hindsight it is most unfortunate that the compilers of the London-Lund Corpus (which is the basis of both papers at issue here) were obliged to permanently embargo the recordings on which their transcriptions are based.

The book is divided into three sections: 'The encoding and tagging of corpora', 'Parsing and databases', and 'Linguistic exploration of the data'. The first is largely concerned with issues of encoding, both structural and taxonomic, the second with parsing and/or assessing parsed corpora, and the third with the use of corpora in linguistic research. A few papers, particularly the last in each of the first two sections, sit somewhat uneasily in this categorisation, but are valuable in their own right nonetheless.

The encoding and tagging of corpora

The introduction, by Nelleke Oostdijk & Pieter de Haan, sets the stage for the rest of the book with a somewhat narrow definition of corpus linguistics, declaring it to be '[synonymous with] descriptive linguistics'. But in fact by the end a distinction between pragmatic and theoretical uses of corpora has been introduced, which belies the earlier claim.

'Continuity and change in the encoding of computer corpora' by Stig Johansson motives a number of aspects of the design of the Text Encoding Initiative (TEI) recommendations by contrasting the markup of a fragment from the Lancaster-Oslo/Bergen (LOB) corpus with the TEI proposals. Despite being out-of-date (even when delivered for printing, as the TEI was evolving rapidly at that time), this serves as a useful, example-grounded introduction to what has become a standard with considerable impact on corpus linguistics.

'Tagging the British ICE Corpus: English word classes' by the late Sidney Greenbaum & Ni Yibin is a discursive introduction to the tagset used in the British English component of the International Corpus of English (CE), largely by way of comparison with the CLAWS1 tagset used inter alia in the LOB corpus. The anecdotal nature of the presentation of how difficult decisions were taken leaves some uncertainty as to the rigour of the tagset specification, in comparison, for example, to the lengthy explicit style manuals used by the SUSANNE (see below) and Penn Treebank projects.

'The large-scale grammatical tagging of text: experience with the British National Corpus' by Geoffrey Leech, Roger Garside & Michael Bryant describes the largest use to date of automatic assignment of part-of-speech tags to large amounts of unrestricted text 'under the gun', as it were, of a formal commitment to public distribution of the results. Unfortunately, the account is weak in a number of technical areas, limiting its utility for anyone embarking on a similar effort: detailed information about workrate of posteditors is not provided and the algorithm used is called a 'hidden' Markov model despite the explicit absence from the statistical model of P(word|tag) (ambiguous words had information about P(tag|word) tabulated only in terms of three qualitative categories: 'common', 'infrequent' and 'rare'). Performance is also difficult to judge (a common problem with tagging papers, it must be said), because insufficient information is given about what the baseline is (i.e., what the level of ambiguity is BEFORE tag selection is performed).

'Computerized lexicons and theoretical models' by Willem Meijs is a brief introduction to extracting computationally usable information from machine-readable dictionaries (MRDs). A substantial discussion of the whys and wherefores of such an effort is given, followed by a very useful description of what worked and what didn't and why in the author's projects to extract information from the Longman Dictionary of Contemporary English (LDOCE).

'Resolving lexical ambiguity' by Louise Guthrie, Joe Guthrie & Jim Cowie is a thorough introduction to the major issues in automatic word-sense disambiguation, but again has some weakness in the area of evaluation of performance levels. A good discussion of how proper names impact on the problem concludes the paper.

Parsing and databases

'Prospects for practical parsing of unrestricted text: robust statistical parsing techniques' by Ted Briscoe is a good introduction to the problems of broad-coverage automatic parsing, with pointers to several possible ways forward. It includes an admirable attempt to describe Viterbi search and the Forward-Backward re-estimation algorithm in words, which may at least serve as a useful first step before diving in to the technical literature on these subjects. One crucial area which is not addressed (but see Gale & Church below) is that of how the algorithms described cope with gaps in their statistical models. The author's own work with N. Waegner is introduced, but so tersely as to make understanding difficult.

'Robust parsing of unconstrained text' by Fred Karlsson is an interesting contrast to the explicit phrase-structure orientation of the previous paper. The work reported here pioneered the use of non-statistical finite-state technology to tag text with both morpho-lexical AND syntactic/functional tags, thus providing a partial parse. The emphasis here is on a large number of finite-state constraints which RULE OUT particular tag sequences, which taken together lead to a (nearly) unambiguous analysis. As in other presentations of this work, the account benefits from substantial amounts of concrete exemplary output, and suffers from weaknesses in the presentation of performance statistics, in failing to clarify whether the tests were open or closed, in the use of post-hoc analysis to determine errors (as opposed to comparing automatic output with a previously hand-validated analysis) and in not including unresolved ambiguities in the tabulation of errors.

'Using parsed corpora: a review of current practice' by Clive Souter & Eric Atwell is a necessarily partial listing of available resources, including some comparisons with respect to issues such as notation and coverage. It misleadingly identifies the Penn Treebank as containing automatically produced partial parse information, when in fact the automatic parses were subject to extensive manual post-editing.

'An experiment in customizing the Lancaster Treebank' by Ezra Black explores the benefits in terms of parser coverage of carefully tuning a grammar and a set of training material to one another. This is a useful exposition for specialists in parser coverage, which includes a brief introduction to a widespread approach to parser coverage evaluation.

'SUSANNE: a Domesday Book of English grammar' by Geoffrey Sampson is a good introduction to the only substantial publicly available hand-parsed corpus of British English by the person responsible for it. An intriguing aside refers to a decision to abandon Stockwell, Schachter & Partee's instantiation of Fillmore's Case Grammar, on the grounds that in practice no general inventory of cases, i.e. semantic roles for complements, could be found.

'What is wrong with adding one?' by William Gale & Ken Church: as mentioned above, a common problem in statistically based corpus processing is missing data: if your process depends on word frequencies for probability estimation, what do you do when you encounter a word in the test corpus which was not present in the training corpus? This article comprehensively demolishes the naive solution of assuming the missing item(s) occurred once, using a number of carefully chosen relevant examples. No concessions to the less than totally numerate are made, however.

Linguistic exploration of the data

'Intra-textual variation within medical research articles' by Douglas Biber & Edward Finegan is a good example of how the availability of corpora in electronic form has made a qualitative difference in what kinds of science can be undertaken. The work reported here explores the variation in soi disant genre-specific linguistic features WITHIN individual medical research articles, finding differences which correlate with the rhetorical purpose of the different sections of the articles.

'On the functions of *such* in spoken and written English' by Bengt Altenberg focuses on the differing distributions of two senses of *such* in spoken (the London-Lund Corpus (LLC)) and written (LOB) English. Much of the work is traditional painstaking lexical description, but the availability of large-scale tabulation of the overall results lends particular weight to the conclusions.

'Imparsable speech: repeats and other nonfluencies in spoken English' by Anna-Brita Stenström & Jan Svartvik somewhat curiously investigates the nature of repetitions in spoken English by way of material available only in the form of extended orthographic transcriptions, i.e. LLC. To be fair it is only in the last two years that substantial corpus material which includes

both transcripts AND digitised speech has become available. The most valuable part of this paper is the detailed taxonomy of repetition types and a convincing analysis of the genre-dependencies of their relative frequencies.

In summary, this book provides an excellent snapshot of the state of the art in text-oriented corpus-based research into language, and can be read with profit by anyone interested in this area.

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Edward P. Stabler, Jr., *The logical approach to syntax: foundations, specifications, and implementations of theories of Government and Binding*. (Series in Natural Language Processing.) Cambridge, MA: MIT Press, 1992. Pp. xi + 433.

Reviewed by TED J. BRISCOE, Computer Laboratory, University of Cambridge

In this book Edward Stabler attempts to develop a formalization of GB theory, specifically the *Barriers* framework, as a theory of first-order logic. Some may be tempted at this point to move on, as *Barriers* has now been supplanted by *Minimalism*. However, this would be a mistake. Part I of Stabler's book presents a reasonably accessible introduction to formal specification, first-order logic and (semi-)automatic theorem proving using problems of formal and natural language as examples. These techniques and tools are now well-established in theoretical computer science and generally seen as the most promising foundation for the development of (probably) correct and, therefore, reliable programs. Stabler argues that the same techniques can and should be used in the development of linguistic theories in order that the consistency, coherence and empirical consequences of such (complex) theories can be fully appreciated and firmly established. As a secondary goal, Stabler also partially develops a processing theory for his formalization of *Barriers*, which rather than focusing on performance limitations, such as self-embedding or garden path sentences, attempts to provide an efficient algorithm for parsing with the theory; that is, relating PFs to LFs with reasonable computational resources.

Formal specification certainly provides one promising method of formalization particularly appropriate to theories couched in terms of principles or constraints such as GB or HPSG, and it has been applied to the latter with some success and utility (e.g. Blackburn 1992, Carpenter 1992). Stabler provides an introduction to the essential techniques, but a linguist unfamiliar with Prolog or (computational) logic will probably need to supplement their reading with textbooks such as Pereira & Schieber (1987) and Bundy (1983). Nevertheless, his descriptions are detailed and backed up by Prolog code listings so that this book will be valuable to someone attempting to make the step from a general grounding in formal methods to a linguistically interesting application. Ultimately, however, the linguist hoping for practical assistance with the exploration of the empirical consequences of the *Barriers* framework would be disappointed: Stabler's formalization involves significant use of disjunction and negation in formulae precluding any straightforward mapping to clausal form and forcing him to adopt, in Part III, a semi-automatic guided-inference approach to theorem proving. The significant problem of providing an interface to such a theorem prover that does not require considerable knowledge of the design and search strategy it utilizes is mentioned but not addressed. The second chapter in Part III addresses the development of a parsing theory in terms of a constraint satisfaction model, and various theory-specific properties are discussed which might serve to reduce the computation required. However, since Stabler does not even attempt to prove the restricted decidability result required to guarantee that a terminating parsing algorithm can be defined, this discussion is no more than a series of remarks which might guide further work. Not surprisingly, terms like 'efficient' are misused in this context: 'It is crucial to distinguish this informal sense of "efficient" from formal senses of the term such as "computable in polynomial time in the worst case".' (284) But, of course, no such fine distinction is crucial when a theory admits infinite numbers of derivations for some sentences.

Much of Part I uses context-free grammars as an example and the final chapter of this most useful part of Stabler's book discusses some 'superficial phrase-structure' (83) based parsing systems developed within the logicist framework. Stabler presents some standard evidence against the context-freeness of natural language (cross-serial dependencies in Swiss German) but argues against the use of augmented phrase-structure parsing systems more on the grounds of their inability to express appropriate constraints on unbounded extraction. It is perverse that here he considers in detail only Extraposition Grammar (e.g. Pereira 1981), because of its heritage in Prolog, Definite Clause Grammar and the 'parsing as deduction' school of computational linguistics, and not, for example, GPSG (Gazdar et al. 1985), LFG (Bresnan 1982) or even the more recent formulation of Pereira's technique of 'gap threading' in Pereira & Shieber (1987). Here and in other crucial parts of the book the clear and careful argumentation that characterizes the discussion of formalization and formal methods is replaced with weak rhetoric and selective consideration of the literature.

Stabler's major goal is formalization of *Barriers* and he begins with a strong claim: 'Very little elaboration of the explicit principles of *Barriers* and other proposals is required to allow the formal deduction of interesting results. The demonstration of this point should squelch the unwarranted but repeated rumors that recent theories are not worked out in enough detail to have interesting empirical consequences.' (4) Nevertheless, even a casual reading of Part II, in which the formalization is presented, should be enough to convince the reader otherwise. The formalization process is replete with interpretative decisions which the discussion makes clear are not clear-cut given the presentation in Chomsky (1986) and elsewhere.

To take formalization of the X-bar component, we are told that Chomsky (1986: 91, fn. 3) suggests that ordering of specifiers, complements and heads might be fixed by language-specific 'parameters' which can be violated by 'marked' constructions, though others have suggested that order is only specified at the level of PF. Stabler (120) 'formalizes' these vague but presumably contradictory statements by defining two predicates whose binary argument determines whether heads precede/follow specifiers and complements, respectively. The first-order monotonic specification language used does not provide a means to formalize the non-monotonic, default status of these predicates, implied by the mention of 'marked' constructions, and is ignored. The possibility of ordering statements at PF is dealt with by suggesting these 'restrictions' could be relocated to PF (115) but how they might be expressed in the absence of constructs such as specifier, complement and head is not addressed.

Total lack of even vague conflicting specification also thwarts Stabler; sticking with X-bar, he is forced to adopt some explicit theory of the syntactic features which constitute categories in most if not all versions of X-bar theory. However, since this has never been addressed within the REST/GB/*Barriers* literature on X-bar (to my knowledge), he falls back on the account emanating from GPSG: 'feature lists … deserve a little more attention. We construe them as "attribute:value" pairs as is standard in the literature' (119) – which literature is not made explicit. Despite the statement that 'Attribute-value structures have been extensively studied, but they do not play a large role in the theory developed here' (120), the principles (x0, x1, x2) of 'X-bar theory proper' are formulated (121) in terms of feature propagation between phrase structure tree nodes and the formal statement of x2 is 'the most complex definition in our account, and one of the most complex in syntactic theory' (123). But this definition, of course, is based on nothing to be found in *Barriers*.

These examples are by no means exceptional. Stabler's attempt to formalize *Barriers* simply serves to underline the vague, inconsistent and incomplete nature of this 'theory'. Since it is not possible to formalize the initial interpretative step which gets the formalization process underway, how faithful this interpretation is remains a very moot question. Formal logical specification is widely applicable, and other techniques of formalization, familiar from formal language theory, such as production systems, tree transducers, and so forth can be seen as special cases, albeit often at the expense of insights into efficient processing. However, the crucial starting point for the enterprise is a precise, consistent and coherent specification of a theory, if it is not to dissolve into an exercise in hermeneutics. Linguistics does not lack theories with the required degree of rigour, such as GPSG, HPSG, LFG, CCG (Steedman 1987) or TAG (Joshi 1987) to name but a few of the better known. Stabler might have been able to make a more convincing case if he had made one of them his starting point.

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Paul Westney, *Modals and periphrastics in English: an investigation into the semantic correspondence between certain English modal verbs and their periphrastic equivalents*. Tübingen: Niemeyer, 1995. Pp. viii + 225.

Reviewed by Michael R. Perkins, University of Sheffield

As one would expect, given the well charted waters of linguistic modality in English, Westney's book is more concerned with filling out areas of detail and clarifying inconsistencies in previous work than with offering innovative approaches or new models. His primary area of focus is on the semantic relationship between certain English modals and a small subset of their periphrastic counterparts. (He distinguishes 'periphrastics' from paraphrases in terms of idiomaticity – i.e. the meaning of periphrastics is regarded as not simply a function of the sum of their parts, unlike the meaning of paraphrases.) Most attention is given to the relationship between *must*, *have to*, *have got to* and *need* (*to*), though *should*, *ought to*, *be supposed to* and *had better* as well as *will*, *be going to*, *is to* and *be willing to* also receive fairly extensive discussion, and other modal expressions such as *can* and *be able to* and *be bound to* have more than a passing mention.

Westney describes his account as being 'primarily descriptive in intention' (6), and more concerned with the nature of modal-periphrastic alternations than the syntax and semantics of the English modals. His starting point is the hypothesis that a greater degree of speaker involvement is evident in the use of modals as compared with that of their periphrastic counterparts, a view which has often been discussed in terms of a subjectivity-objectivity distinction. To examine this, he adopts broadly the general framework developed by Perkins (1983) which identifies for each modal expression a monosemantic frame whose variables are interpreted pragmatically, and proposes that the modal auxiliaries are maximally unmarked in relation to their periphrastic counterparts. His arguments are illustrated and supported with examples from Svartvik and Quirk's 220,000 word Corpus of English Conversation (a component of the Survey of English Usage) and miscellaneous written and spoken material from a range of sources.

Westney succeeds in clarifying a range of issues and extends our detailed knowledge of modals and periphrastics in a number of areas. I will just mention some of them. The general proposal

that the modals are maximally unmarked in relation to their periphrastic counterparts is amply substantiated, though in the case of *should* and *ought to*, which are commonly regarded as more synonymous than most modal pairs, Westney argues that *should* is less marked than *ought to* in that '*should* simply indicates that the fulfilment of some proposition can reasonably be expected, whereas *ought to* suggests that there is some good reason for this expectation' (166). *Have to* and *have got to*, also commonly conflated, are likewise teased apart as semantically distinct periphrastics of *must*, although the status of *must* as least marked form is not unambiguously reflected in use, since it appears to be used in a narrower range of senses than *have to* and *have got to* in speech, but in a wider range in writing. The association of modals with subjectivity, when compared with their periphrastic equivalents, is given only qualified support. Westney argues that the notion needs unpacking and that 'speaker's point of view' and 'empathy' need to be more explicitly taken into account.

My general reaction to Westney's book is positive. His approach is careful and scholarly; he has an extensive and detailed knowledge of the literature in this area and represents it fairly; he writes clearly; he provides a range of new insights at a detailed level; he tidies up various loose ends and is good at identifying and clarifying previous inconsistencies and confusions. Because of such strengths I have no doubt that this work will be read with interest and profit by linguists who work in this area. At the same time, however, the ground covered by Westney is very familiar territory to anyone who has done research on modality and I felt a little disappointed that the book was not as innovative as it might have been. I will briefly mention two examples. First, the use made of corpus material is primarily for illustrative purposes, and the main corpus used is rather small and dates from 1980. To echo a comment by Coates (1980: 338) in her review of the first edition of Palmer's *Modality and the English modals*, 'having decided to use a corpus, why did [he] not use it more thoroughly?' In other words, corpora – and particularly the massive electronic corpora now available – provide a rich and powerful resource for establishing patterns of language use in both statistical and collocational terms. Although Westney pays lip service to the Firthian notion of word meaning as a 'product of a word's collocational possibilities' (8), rather than attempting to establish what these meanings are empirically by starting with a large representative corpus, his starting point instead is a set of notions based largely on linguists' intuitions which his corpus is simply used to verify. The second point concerns what Westney refers to as 'variation in use according to style' (39). Here again, although his intentions are laudable in that he rightly acknowledges the tendency of much work on modality to concentrate on decontextualized sentences and tries to counter this by taking into account variables such as discourse context and convention, he does not in fact move much beyond the accounts he criticizes. For Westney, style still appears to be no more than the simplistic dichotomous contrasts between formal/informal and speech/writing found in the older standard treatments of modal meaning. Reality is unfortunately more complex than this and the way to advance the study of modality in language use is to take on board recent developments in pragmatics and sociolinguistics in conjunction with serious and extensive use of large electronic corpora. I accept, however, that this is not so much a direct criticism of the book that Westney has written – which achieves its modest aims admirably – as a blueprint for the book I would have preferred him to write.

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