While one might wish for greater exploration of sonority's role in phenomena other than phonotactic restrictions, *The sonority controversy* is overall a very nice collection. It suffers from problems that seem common to collections—not enough space devoted to analyses that would benefit from greater development and exemplification, too much space devoted to points that could be made much more quickly, relative lack of direct contact between the views expressed in the different articles—but the variety of viewpoints expressed and the diversity of particular interests explored ensures that *The sonority controversy* will appeal to a wide range of researchers. Its papers will be of value to researchers interested in, for example, optimality theory, acoustic phonetics, articulatory phonetics, speech perception, language acquisition, computational modeling, sign language, markedness, and syllable structure. On the whole, the collection paints the picture of a lively and fruitful debate in a key area of investigation. It is well worth reading.

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The morphology and phonology of exponence. Ed. by JOCHEN TROMMER. (Oxford studies in theoretical linguistics 41.) Oxford: Oxford University Press, 2012. Pp. xvi, 592. ISBN 9780199573738. $55.

Reviewed by CHRIS GOLSTON, *California State University, Fresno*

This book brings together a number of articles about how morphosyntactic feature bundles get turned into things (un)pronounceable. The book begins with an excellent overview by the editor,

which is less opinionated than the one you are about to read (http://fds.oup.com/www.oup
.com/pdf/13/9780199573738_chapter1.pdf).

Ch. 2, 'The architecture of grammar and the division of labor in exponence', by RICARDO BER-
MÚDEZ-OTERO takes on a large and difficult array of issues from reduplication and morpheme-
specific phonology, to the productivity, variability, and exceptionality of phonological processes,
to classical blocking and cyclicity, to frequency effects and questions of lexical storage. The re-
sult is a complicated grammatical processing model based on symbolic rules and an articulated
lexicon that stores prosodic structure that most phonologists would derive; but it is this articu-
lated lexicon that allows it to model such a wide array of facts. Bermúdez-Otero ties together the
messy and the elegant using a dual-process model (storage plus processing) in a feed-forward
grammar based on lexical redundancy rules (Jackendoff 1975), stratal optimality theory
(Bermúdez-Otero 1999, Kiparsky 2000), and a dual-route processing model (Prasada & Pinker
1993). He argues for severe restrictions on how morphology and phonology interact and against
any kind of morpheme-specific phonology, treating all apparently nonconcatenative morphology
as concatenating defective phonological material—for example, treating reduplication as the af-
fixation of empty prosodic structure.

ANDREW NEVINS's 'Haplological dissimilation at distinct stages of exponence' (Ch. 3) collects
evidence that haplological dissimilation takes place at major stages in language processing.
Nevins assembles a formal model of spell-out based on Ackema & Neeleman 2003, Arregi &
Nevins 2007, and Richards 2010, with four stages of processing: (i) syntactic linearization, (ii)
prosodic phrasing, (iii) word formation, and (iv) vocabulary insertion. Dissimilation occurs when
things are too similar at whatever level is relevant: for example, syntactically too similar, or
prosodically too similar. I like the idea of the paper, but many of the details seem arbitrary: linear
adjacency is relevant for stages two and four but not for stages one and three—why does it disap-
pear and then come back? In what sense are French *que* 'than' and *que* 'that' 'completely featu-
rally identical' (93)? Or Romanian *-ul* 'DEF' and *-al* 'POSS.M.SG', which do not look 'completely
featurally identical' but have to be for the analysis? Much of the data in this paper seemed to me
cherry-picked or shoe-horned to fit the model; a separation of facts from model would have
helped me appreciate the paper more.

The major result of Ch. 4, 'Morphophonological polarity', by PAUL DE LACY is that it gets rid
of the clearest, best-studied case of morphophonological polarity, one involving stem-final voic-
ing alternations in Dholuo. The polarity is supposed to involve voicing in certain singular ~ plu-
ral pairs (*alap ~ œlœbe* 'open space(s)', *gɔt ~ gɔdɛ* 'hill(s)') but devoicing in others (*kitœbu ~
kitepe* 'book(s)', *kɛdɛ ~ kɛtɛ* 'twig(s)'). Building on unpublished work by Bye (2006) and Trom-
mer (2008), de Lacy clearly shows that 'root-final consonants devoice in the singular while in the
plural the rightmost consonant devoices only if it is non-final ... in the root ... There is no mor-
phophonological polarity' (124–25). That much is clear and neat, and it casts huge doubt on
whether any cases of morphophonological polarity will remain once good investigative work like
this is done. The actual analysis that derives the forms above, however, is technical and dense and
involves tricks of the trade that not every reader will find enlightening; we are probably done with
morphophonological polarity but not with Dholuo.

DIETER WUNDERLICH in 'Polarity and constraints on paradigmatic distinctness' (Ch. 5) argues
convincingly that morphological polarity (e.g. when *-a* marks masc.sg. but fem.pl., while *-o*
marks masc.pl. and fem.sg.) is always epiphenomenal and need not worry us, a welcome result.
He shows that the common type of (apparent) polarity actually arises from a special array of oth-
erwise unexciting things and that the one hard case (Old French) actually goes away, too, resolv-
ing itself into an intersection of rare things that results in one super rare thing. A comforting
article, clear and concise, that complements de Lacy's article nicely.

In Ch. 6, 'Contextual allomorphy', EULÀLIA BONET and DANIEL HARBOUR provide a survey of
allomorphy (*ox-en* vs. *cow-s*) and try to lay out what any theory needs to account for, abstracting
away from phonologically predictable forms of allomorphs (like *-s*, *-z*, *-ɨz* for the suffix in *cow-s*).
They divide the chapter into empirical characterization (§2) and theoretical issues (§3), but this did
not work for me. Their 'empirical' discussion focuses on SPE-type diacritic marking, I-grammars,
feature-deletion rules, zero-morphemes, relativized minimality, and inflectional parsimony, none

of which seem like empirical issues; after much rereading I am still unable to tease out of all of this 'what the facts are that we need to account for', which is one of the things they promise. Conversely, their 'theoretical issues' include whether roots display allomorphy (they do) and whether there is a limit to the number of allomorphs a morpheme can have (there is not); both of these are empirical questions that they quickly settle with nothing more than data. This lack of separation between data and theory made the paper hard for me to read and harder yet to evaluate. It seems to have been poorly edited, too, with the wrong candidate (*cho.ul*) selected in example 35, an unfortunate 'the optimal candidate ... is filtered out' (222), and other infelicities the editor should have caught.

In Ch. 7, 'Syncretism', ADAM ALBRIGHT and ERIC FUß provide a broad discussion of syncretism, that is, when 'morphology fails to mark a featural distinction that is syntactically relevant' (287). The article contains a wealth of nice data from various dialects of German and other languages and a good theory-neutral discussion of the types of syncretism that are found along with the problems they pose for various theories, including nasty polarity effects and 'directional syncretism', ugly data where the theoretical cure seems worse than the descriptive disease (§7.3). The authors convince me that we need underspecification (Jakobson 1984 [1936]) to understand syncretism, but I am left wondering why negative feature values like [–Speaker] do so much real work here while they are positively detrimental in phonology (Steriade 1995). Albright and Fuß make me hopeful that more research will make impoverishment (Noyer 1992) and rules of referral (Zwicky 1985) as unnecessary as they are dispiriting, while clarifying the hard data these dull tools pound into submission. Some hope is held out that optimality theory will ride to the rescue (Trommer 2001, Wunderlich 2004, McCarthy 2005). Albright and Fuß explore the implications that diachrony has for syncretism, via diachronic case studies of verbal morphology in English, Yiddish, and Alemannic, and urge considering first language acquisition as a way of better understanding it.

Ch. 8, 'Templatic and subtractive truncation', by BIRGET ALBER and SABINE ARNDT-LAPPE clarifies that there are two types of truncation: one where you end up with a certain size thing (reduce the word to a syllable, foot, etc.), and the other where you get rid of a certain size thing (delete the last syllable, foot, etc.). They argue convincingly that both types of truncation really are part of grammar (which has been disputed), and they give a typological perspective of what the field needs to account for. Their findings for the reduction-to-x type of truncation are that lots of constraints are needed, of various and sundry types, and that the resulting factorial typology does not really pan out. Their findings for the delete-an-x type of truncation are that a lot of subtractive morphology is not really that and goes away when you look at the language more closely, but that there is a solid residue of cases that do NOT go away and seem to require bludgeons like antifaithfulness (Alderete 1999, 2001) and worse, and that these still may not do the trick either (Wolf 2008).

JOCHEN TROMMER, in Ch. 9, 'Ø-exponence', attacks the thorny issue of zero morphs in current theory, bringing in data as window dressing for a rather theory-driven discussion. He begins with a dense introduction to already dense distributed morphology (Halle & Marantz 1993), and then moves on to parallels in other rule-based approaches (Zwicky 1985, Anderson 1992, Stump 2001), focusing less on the slings than on the arrows ($\rightarrow$, $\Rightarrow$) of their outrageous fortunes before turning to arrows (☞) that do the same thing in another framework. I find this article seriously disquieting. If 1 is the answer to a problem in our field, then we are in trouble.

(1) $[+S] \rightarrow Ø / [+Agr \_\_ +1] [+Agr +O -1 +2]$

The optimality theory equivalent (2) is no better for its small caps and lack of derivational arrow.

(2) IMPOVERISH$^{[+S]\,[+Agr\,+1]}$ // $[+Agr +O -1 +2]$

Trommer succeeded in convincing me that this really is the best we can currently do, that this is the state of the art; but it made me want to pack up and go home. Additional refinements to theory (§9.3) often seem like sweeping problems from under one part of the theoretical rug to another without resolving them, like the fine line between INSERTING A ZERO and NOT INSERTING ANYTHING (339ff.). Trommer sums it up nicely when he says: 'While there is widespread agreement that Ø-exponence must be substantially restricted in some way by general principles of the

grammar, the nature of these principles still awaits thorough conceptual and empirical investigations' (354).

Sharon Inkelas's 'Reduplication' (Ch. 10) is how an article should be, in my view: a calm discussion of clear facts that argue for a full-copy approach to morphology vs. a templatic one, sparing the reader the gruesome details and focusing on the processes that make one analysis more likely than another. She demonstrates that some facts argue for a full-copy approach in which reduplication is compounding (*banana-banana*) with a hefty dose of loss (*ba-banana*), while other facts argue for a templatic/phonological approach, in which the reduplicant starts life as a small affix (e.g. σ_μ- or RED- or the like) and ends up sounding like part of the base through spreading/copying/faithfulness. Inkelas focuses us on what the data look like and how the models (fail to) fit them; this focuses us on what to look for next and braces us for the possibility that both approaches may be needed (Yu 2005, Inkelas 2008).

In Ch. 11, 'Iconicity', Laura J. Downing and Barbara Stiebels discuss a lot of data and provide a (sometimes more than) even-handed view of current opinions on iconicity. I was more convinced of what we might call ABSTRACT iconicity: that the mirror principle (Baker 1985) can be seen as an iconic reflection on heads of scope relations in phrases and that generalized template theory (McCarthy & Prince 1994, 1995, Downing 2006) can be seen as iconically relating the semantic bulk of roots and affixes to their greater and lesser typical prosodic sizes. I was less convinced of what we might call CONCRETE iconicity, for example, that rising intonation in questions and falling intonation in statements has to do with the submission of the questioner and dominance of the answerer (Ohala 1994). Rising (L…H) and falling (H…L) intonation both have an H and an L component, so any iconicity would have to come from little things coming after big things or the reverse, either of which is absurd. Similarly, the iconic principle of reduplication (Kouwenberg & LaCharité 2005) should predict that partial reduplication means a little more of something while full reduplication means a lot more, but that is not how the semantics actually works out. The authors could have skewered such proposals, but they treat them with kid gloves. This article left me convinced that concrete iconicity in grammar is marginal and of little interest.

Nonconcatenative morphology is a big field, and Patrik Bye and Peter Svenonius do not actually cover it all in their chapter, 'Non-concatenative morphology as epiphenomenon' (Ch. 12), so their claim that it is epiphenomenal is a little premature, though I hope it is right. Their general idea is that syntax puts morphemes where they go and phonology makes them pronounceable (and that is it); but what I learned from this article is that this lovely picture comes at a high cost and that you have to swallow bags of nails to buy it. Some errors did not help the paper: Bye and Svenonius say for Latin, for instance, that 'if a preposition is phonologically light (smaller than the lexical word minimum), *-que* appears after the following word' (479); but *-que* appears right after phonologically light prepositions quite commonly in Latin (Spevak 2010:19), and the standard story is that ALL words in Latin obey the lexical word minimum (Mester 1994). 'Second-position' elements in Latin and in early Indo-European languages generally need not follow prosodic words; they just cannot be phrase-initial (Agbayani & Golston 2010). Again, Bye and Svenonius say for Latin that 'movement rules generally do not affect one conjunct without affecting all conjuncts' (479), but this is manifestly not true; it is very common in Latin and even has a name: 'conjunct hyperbaton' (Devine & Stephens 2006:586ff.).

Overall, this collection is thought-provoking and challenging; if you think you understand how meaning is turned into sound, read the book and think again.

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