

NOTES AND DISCUSSION

**Disentangling autosegments: a response**

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I. INTRODUCTION

It may not matter that the authors of 'Disentangling autosegments from prosodies' (in this volume) misunderstood the point of my recent note in *JL* (Goldsmith 1992). I was at some pains there to state my goals clearly. I was trying to show that IMPORTANT IDEAS WILL OFTEN TRAVEL FROM ONE THEORETICAL FRAMEWORK TO ANOTHER VIA AREAL LINGUISTIC STUDIES. Even the title, 'A note on the genealogy of research traditions in modern phonology', with its Foucauldian overtones, carried the message that I was looking to uncover nontraditional corridors that linked superficially distinct research programs. The case that I examined was the transmission of certain conceptions of phonological structure from prosodic analysis to autosegmental analysis by way of the study of African languages. Along the way, I offered some suggestions as to why it was that these out-of-the-way detours should be necessary, and I made the suggestion (not an entirely happy one) that part of the reason for it was that membership in a theoretical school can bring with it a complaisance and a lack of concern for what is happening (or has happened) in other theoretical frameworks. Membership in a professional organization of a different kind, such as an organization basing its coherence not on theoretical inclination but on area of language study (e.g. African languages, signed languages of the Deaf, Southeast Asian languages) generally has the opposite effect, and does not, in any event, permit or excuse such theoretical myopia.

The authors of 'Disentangling', however, took my goal to be the comparison of firthian prosodic phonology and autosegmental phonology, and they read two parts into this effort: first, an effort to show that there are theoretical differences between these two schools that are of theoretical importance, and second, an effort to show that autosegmental phonology is more elaborated and sophisticated. Of these two, my own understanding was that the first of these was a real, though a minor, part of the larger enterprise, and in any event a side issue at best; Messrs. Ogden and Local agree with me on that point, though they believe that I do not succeed in expressing the firthian position in terms acceptable to firthians. The second effort was in no

way part of my goal, though I did point out that autosegmental phonology has been very successful in dealing with Bantu tone languages, for example, where firthian prosodic phonology has not.<sup>1</sup> It WAS my concern to discuss some cases drawn from the literature in which prosodic analyses directly influenced later phonologists who were developing ideas in an autosegmental context, and it would not be unfair to my note to say that in it I was using the language of generative phonology and autosegmental phonology to interpret Firthian Prosodic Analysis, as Ogden and Local put it. Like Ogden and Local, I too would not recommend it to anyone to try to learn the fundamentals of prosodic analysis from me, just as I would not recommend learning about American structuralist phonology by reading Chomsky's works on the subject.

These are the main reasons why I think 'Disentangling' is not so much a reply to MY note as it is a reply to a whole slough of theoretical positions, most of them associated with classical generative phonology (that is, the framework established in *The sound pattern of English* (Chomsky & Halle 1968), and developed over the period from 1965 to 1975). Autosegmental phonology as presented in Goldsmith (1976) rejected a few of these positions, accepted many of them (though with the seeds of dissent already present), and was agnostic with respect to the rest. I am not certain that there is a framework that I would call AUTOSEGMENTAL PHONOLOGY at this moment, at least among the phonologists doing current work that I am familiar with. Many of the suggestions made in Goldsmith (1976) and other work in the 1970s have become coin of the realm, so to speak – the common possession of phonologists.

I have reorganized the issues that Ogden and Local raise a little bit. I see a set of questions involving, first of all, what kind of representations are appropriate for phonology; second, what is the relationship between phonological units when we are comparing them ACROSS LEVELS; third, whether the rules that relate representations on distinct levels are best understood as DECLARATIVE, or MONOTONIC, or (to use a third, nearly synonymous term) STATIC; and finally, some random points, concluding with the question of whether any of us are being led around by the nose by the Lakovian metaphors that we are living by.

[1] There does not appear to be any controversy there, either, and I think it would require an unusually unsympathetic reader to see something objectionable in this remark. The reader will have to decide whether Ogden & Local bend over backwards to interpret my neutral remarks as criticisms; they peevishly wonder, for example, why I demeaned firthian proposals by calling them 'mere "insights"'. Frankly, I think calling something an 'insight' is an unadorned compliment.

## 2. REPRESENTATIONS: SYSTEM AND STRUCTURE

The notions of system and structure are important in prosodic analysis: SYSTEMS express which alternative possibilities are permitted by a language in a given position of an utterance, while STRUCTURES express relations between units in a given utterance. It comes as a surprise to read that this difference is not to be found in autosegmental analysis. A large part of Goldsmith (1990), cited by Ogden and Local but perhaps not read, is devoted to the task of determining how syllable structure governs (or to put it more agnostically, is related to) the greater and lesser range of possible consonants in different syllable positions. As J. R. Firth is my witness: asking such a question in such a way is precisely what prosodic analysis teaches us to do. Many languages put severe restrictions on what segments can appear in a syllable coda, though not on what can appear in the syllable onset. This is a prototypical example of a prosodic structure, but the mechanisms appropriate for dealing with it have nothing (or very little) in common with the mechanisms of tone placement, tone spreading, or the other characteristics of 'suprasegmentals' that have given rise to the rumors of parallelism between firthian and autosegmental analysis. That, at least is my claim; the elements of what I called autosegmental licensing (we could as well call it featural licensing) are simply different from the elements of tonal spreading. I could certainly be wrong, but the proof lies in the details, and Ogden and Local have not provided those details; and THEY think that I am not even aware of the existence of the problem.

There seems to have been some kind of misunderstanding between Ogden and Local and myself on the differences and similarities between nasality and tone. I don't wish to be taken to be saying that they are utterly different, and I do wish to be understood as saying that they are more similar than they are different. However, study of real languages (and not study of theory) informs us that there are many languages that do one thing with tone for which there is nothing comparable in the domain of nasality: and that is that tone solutions in some areas of the world build up long 'phrases' or 'melodies' (or concatenations) of four, five, and even more successive tonal specification that are spread over an entire word. It would be just fine if nasal-harmony languages did that, but the fact is that they don't. It is in this sense, and this sense only, that tonal systems present certain challenges that the study of nasal harmony languages do not. By the same token, we can learn interesting things about the capabilities of the phonological system by studying the more complex cases (at least more complex in certain respects), the tonal systems.

Ogden and Local give a list of seven differences as they see them between prosodies and autosegments or autosegmental tiers. I certainly have nothing against the enterprise of listing such differences (in fact, I'm all for it), but I don't agree with a number of the details that they suggest. Here is their list (with minor stylistic changes):

- (1) 'Phonological features in FPA are not well-ordered, but they are in AP.' But autosegments are not well-ordered in autosegmental phonology; only autosegments on a single tier are well-ordered.
- (2) 'Prosodies may be grammatically motivated; there is no such close connection between grammar and phonological form in AP.' Autosegmental tiers may be grammatically motivated, as I argued in Goldsmith (1976), and as McCarthy (1981, 1989) has detailed in subsequent work.
- (3) 'Prosodies are elements of structure, they don't "spread" over other units.' This seems to me to be purely a matter of choice of words, with no substantive difference.
- (4) 'There is no simplistic equivalence between long-domain phonetic features and prosodies. A long-domain phonetic feature need not necessarily be treated as prosodic.' Fine.
- (5) 'The exponents of a prosodic unit need not be long-domain phonetic features.' Spans of an autosegment need not be long-domain either.
- (6) 'A prosody can have discontinuous phonetic exponents, and may also be expounded by phonetic features of different kinds.' Fine.
- (7) 'Prosodies, like all phonological terms, need and are given an explicit phonetic interpretation; whereas in AP that can be left implicit.' I wasn't aware that there was a difference in kind in this respect, though the YorkTalk references that Ogden and Local cite substantiate serious work in the area of phonetics coming out of York. That's all well and good, though I still don't see how this difference can be reasonably said to grow out of the theories themselves. In any event, more power to the developers of YorkTalk!

### 3. LEVELS AND POLYSYSTEMATICITY

I think it is fair to say that the central achievement of structuralist<sup>2</sup> linguistic analysis earlier in this century was the treatment of language 'as a set of interacting systems, each of which may be stated independently of others... what is true of one system may not be true of another; and [such a] statement may be more economical' than one that fails to break up the analysis into autonomous but interactive systems – but this citation, from Ogden and Local (section 2.2.1), does not characterize just firthian practice, as they suggest: far from it! One of the most fundamental of the premises of generative grammar as it was originally proposed was that the rep-

[2] Here I use the term as a nonlinguist would normally use the term: structuralism is an intellectual current in this century which displaced historical studies; it includes firthian and chomskian analysis; outside of linguistics, it is viewed as *passé* (certainly) and morally degenerate (most likely).

resentations on the different levels of a grammar would be related not directly by a set of rules, but in a stepwise fashion, with rules applying in a particular order; this kind of stepwise relation was called, of course, a derivation. More recently, the proposal has been made that there is a relationship between depth of rules in this ordering and morphological positioning, brought together in a theory (lexical phonology) in which a set of phonological rules and a set of morphological together is understood to constitute a stratum, and its output a level. Now, while I discussed none of these things in 'Genealogy', and few of them in Goldsmith (1976), I have been critical more recently of most aspects of derivationalism in phonology (in Goldsmith (1990: chapters 4 and 6) and now in much greater detail in Goldsmith (1993: chapters 1 and 2)).

Which of this material it is that Ogden & Local wish to challenge is difficult to determine from their remarks; all they have to offer is the thought that 'levels' involves an 'unhelpful metaphor'. If by 'levels' they mean what the linguistic profession meant during most of the century (including during Firth's lifetime), then they are themselves indeed deeply committed to the importance of levels of linguistic analysis; if they are concerned, rather, with derivationalism, that's fine, but they do not provide any evidence for their position, something that the rest of us are trying to do. But what they turn to next (having treated derivationalism inadequately) is the apparent fact that languages may have phonological generalizations that cover just a portion of the vocabulary. Sometimes the portion that obeys certain generalizations is defined grammatically ('only verbs have penultimate stress'), and sometimes historically ('only Germanic vocabulary begins with [θ]'). When employed thoughtlessly, this option can become a gateway to horrific analyses ('stress in English falls on the penultimate syllable, except in words of Class A, all of whose members regularly have antepenultimate stress, and Class B, which regularly have final stress...'), though when employed with care, this kind of analysis can be quite interesting, and indeed, insightful.<sup>3</sup> How this issue relates to autosegmental phonology, or to the differences between firthian prosodic phonology and generative phonology (of one generation or another) is not addressed by Ogden & Local, and I myself cannot disentangle it. All theories seem to allow pretty much the same range of possibilities, as far as I can see, and Ogden & Local seem to feel that way, too ('interestingly, some degree of polysystematicity has been accepted by contemporary phonology', they say.<sup>4</sup>)

[3] I have learned a good deal about this from a recent reading of a contribution by Junko Ito and Armin Mester on Japanese phonology, to appear in *The handbook of phonological theory*, from Blackwell's. On my use of the term 'insight' in the text, see note 1.

[4] I can't resist pointing out that in that sentence, Ogden & Local do what they elsewhere chastise me for doing – they rhetorically draw a distinction between what firthians do, on the one hand, and what is done, on the other, in 'contemporary phonology', as they put it, meaning 'metrical-lexical phonology'.

## 4. RELATIONS ACROSS LEVELS: DECLARATIVE PHONOLOGY

One of the points of 'Disentangling', I think, is to show how prosodic analysis takes on some of the long-standing issues in phonological analysis, many of which have become very much alive and debated after a period of dormancy in the era of classical generative phonology. I do not think that autosegmental phonology, as a theory of phonological representation, is a particularly apt target for these firthians' critique, but that may be more a matter of taste or personal judgement.

Firthian prosodic analysis, we are informed, is a DECLARATIVE brand of phonology, unlike any 'derivational (or procedural)' approach – presumably including autosegmental phonology. The declarative nature is 'based on its search for a single invariant phonological description of any given item' while the differences in the realization of the item are to be chalked up to the differences in 'phonetic exponents ... under differing structural conditions' or by other means, discussed in other publications. Well: so far there's nothing to alarm even a conservative generativist, since generative phonology also is based on a strong desire (indeed, at times a perversely monomaniacal obsession) to find a single invariant phonological description of any given item, though the realization of that phonological description is affected by the context it is in. The Disentanglers have not disentangled their prosodic analysis from generative phonology. There is lurking here, I think, an issue which is taken on in the recent literature by such authors as Coleman (whom they cite in another context) and Scobbie. That issue is this: is it possible to rethink phonological theory in a fashion parallel to the ways in which Generalized Phrase Structure Grammar (which, again, they cite in different context) brought about a rethinking of derivations in syntactic theory in the early 1980s? Such a rethinking would lead to a declarative theory of phonology, some of whose properties we can dimly see: it would likely be monotonic, in the sense that phonological information from the components parts would be put together additively, and there would be no cases where a given phonological unit was explicitly specified in one fashion (say, as voiceless) at a phonological level of description, and explicitly specified in the opposite fashion (voiced, perhaps) at a phonetic level of description. Perhaps such a claim is what Ogden & Local have in mind, though they do not say it at all clearly. It seems to me that their view about the difference between a phonological description and a phonetic description (to wit: the two are expressed in quite different terms, related by principles of exponency, but fundamentally distinct) makes it a good deal harder to know whether a given analysis is declarative or not, since we never know directly whether a piece of phonological information is directly realized phonetically, or only indirectly. Perhaps I'm wrong about that, but there's no evidence produced that the firthians have thought through this problem.

I might add that I myself don't think that phonology is declarative in this

sense, nor that a one- or two-level theory of phonology will be sufficient for adequate phonological analysis. One of the reasons for my belief is the fact that we commonly find situations of the following sort. Consider a pair of rules from Tangale, a Chadic language.<sup>5</sup> There is a rule that deletes a stem-final vowel when a suffix follows, followed by a second, 'later' rule which devoices an obstruent when a voiceless obstruent precedes. This latter rule is a perfectly ordinary, garden-variety rule of voicing assimilation in obstruent clusters, but for it to work, it requires a representation in which the underlyingly intervening vowel should no longer appear.<sup>6</sup> Thus underlying /lutu-go/ surfaces as [lutko]. It won't do, either, to say that the deleted vowel is not really there underlyingly (that is, it won't do to say that this vowel-loss is due to stem-allomorphy), because there is another phonological rule, one that voices obstruents that are immediately followed by sonorants (underlying /bugat-no/ surfaces as [bugadno]), only this rule takes the underlying representation as its input, and hence will fail to apply over a stem-final vowel that ('later') deletes, as in /lutu-no/, which surfaces as [lutno], not \*[ludno]. This example from Tangale is not unusual in the slightest; I'm sure that many readers can recall examples of languages where (for example) certain consonants delete intervocally, feeding some other rule of vowel fusion. These feeding relations between rules of deletion and rules of assimilation or fusion are thoroughly problematic for declarative approaches to phonology, it seems to me.

## 5. METAPHORS

I used to have a high regard for the Lakoff & Johnson style of conceptual analysis, which allowed one to summarize certain conceptual schemes in handy phrases, like 'SPACE = TIME', or 'HIGH is associated with ABSTRACT'. Being for once on the receiving end, however, I must say that from here it looks rather silly. I don't doubt that all workers are in various ways trapped in the tacit assumptions of their conceptual structures, but these pat formulas are (as we say on this side of the Atlantic) rather mickey mouse. Do I worry about using 'space' for 'time' in my phonological representations? You bet I do, and that concern is not mollified by the fact that all current theories of phonology do the same, including prosodic analysis.<sup>7</sup> The only way to get away from using linear space for time in our theory of phonology is to move away from representations on paper, and using formal models in a (real or

[5] See Kenstowicz (1994: 95 ff.).

[6] This situation is particularly troubling for optimality theory, as it has been developed over the past year or two, in which deleted elements remain in the derived phonological representation.

[7] I should point out that the metaphor at play isn't TIME = SPACE, if 'space' is taken to be three-dimensional, as it usually is; what's at issue is modeling time as a line, that is, as a 1-dimensional 'space'.

conceptual) computer that actually evolve over time. I have indeed explored this, concerned, as I am, with the potentially noxious effects of modeling time as linear space. But I don't see Ogden & Local joining that issue in a concrete fashion. Much the same goes, mutatis mutandis, for other uses of the oversimplifying aspects of the Lakovian analysis of my paper, linking LOW and CONCRETE aspects of my thoughts, etc.

#### 6. AN END, IF NOT A CONCLUSION

It has been said that the United States and England are two countries separated by a common language. Let me tell you, an overseas reader of the *Journal of Linguistics* can easily be tricked into thinking that there is more solid common ground to be shared by members of the LAGB and the LSA than there really is, as the essay by Ogden & Local establishes.

I hope that it is reasonably clear that my own view about fragmentation and solidarity in the field is rather different from Ogden & Local's; that, at least, is my conclusion upon reading their essay. I am obviously not unhappy about talking about firthian insights from a generative point of view, and have no trouble at all with using terms like 'inventory' when discussing prosodic phonology. What's wrong with the word 'inventory'? People know what it means, by and large and roughly; what Ogden & Local seem not to like about it is that it isn't the word that firthians use to talk about phonology. But I'm not bound (none of us are bound), when discussing firthian phonology (or government binding theory, or optimality theory, or you name it), to using only the terms that someone lets me use. To put the point slightly differently, Ogden & Local say that, 'one of the tasks which Goldsmith sets himself in his discussion is that of distancing AP from FPA'. I think they're wrong about that; I think that there is, correctly, a considerable gap perceived by most phonologists between the two frameworks, and I think that Goldsmith (1992) could be viewed as succeeding in bringing the two rather CLOSER TOGETHER, whereas Ogden & Local's response aims at increasing the sense of incommensurability between the frameworks. I don't buy that incommensurability, by and large; I think we're all working on the same problems, and we have both the responsibility and the opportunity to express ourselves in a way that makes ourselves comprehensible to the widest audience possible.

To draw this to a close. I agree with Ogden & Local's final remarks, in which they note that this interchange in a public forum bears witness to the enduring value of firthian theory and practice, and in which they make a plea for presenting firthian ideas in their own terms. I would only add to this that the most important external force that will serve to make prosodic analysis stronger and more vibrant than it is is the commitment to an ongoing dialog with other schools of thought, and not all of those dialogs will be conducted in prosodic theory's native tongue.

## REFERENCES

- Chomsky, N. & Halle, M. (1968). *The sound pattern of English*. New York: Harper and Row.
- Goldsmith, J. (1976). *Autosegmental phonology*. Ph.D. dissertation, MIT. (Published in 1979, New York: Garland Press.)
- Goldsmith, J. (1990). *Autosegmental and metrical phonology*. Oxford: Basil Blackwell.
- Goldsmith, J. (1992). A note on the genealogy of research traditions in modern phonology. *Journal of Linguistics* 28. 149-163.
- Goldsmith, J. (ed.) (1993). *The last phonological rule*. Chicago: University of Chicago Press.
- Kenstowicz, M. (1994). *Phonology in generative grammar*. Oxford: Basil Blackwell.
- McCarthy, J. (1981). The prosodic theory of nonconcatenative morphology. *Linguistic Inquiry* 12. 373-418.
- McCarthy, J. (1989). Linear order in phonological representation. *Linguistic Inquiry* 20. 71-99.
- Lakoff, G. & Johnson, M. (1980). *Metaphors we live by*. Chicago: University of Chicago Press.
- Scobbie, J. (1991). *Attribute value phonology*. Ph.D. dissertation, University of Edinburgh.
- Scobbie, J. (1992). Licensing and inalterability in Tiberian Hebrew. In Canakis, C., Chan, G. & Denton, J. (eds.) *Papers from the Chicago Linguistic Society Meeting* 28. 457-471.
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