Disentangling autosegments: a response

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1. Introduction

It may not matter that the authors of 'Disentangling autosegments from prototypes' (in this volume) misunderstood the point of my recent note in JL (Goldschmid 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 axial 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 non-traditional 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 complacency 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
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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 forshian prosodic phonology has not. It was my concern to discuss in 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 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 syllable code, 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 "supersegmentals" 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 call 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 a 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 have 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 patterns) that are spread over an entire word. It would be just fine if nasal-words did that, but the fact is that they don't. It is in this sense, and in 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 autosegmentals or autosegmental tiers. I certainly have nothing against the enterprise of listing such differences (in fact, I'm 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. [There does not appear to be any controversy there, either, and I think it would require an unusually sympathetic reader to see something objectionable in this remark. The reader will have to decide whether Ogden & Local look back into my review as criticisms; they probably won't. For example, why I defined further prosody by calling them "more "insights"."
2. 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 (1989), cited by Ogden and Local but perhaps not read, is devoted to the task of determining how syllable structure governs (or puts up more agonistically, 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 code, 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 "supersegmentals" 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 call 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.

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3. LEVELS AND POLYSYNTACTICITY

I think it is fair to say that the central achievement of structuralist linguistic analysis earlier in this century was the treatment of language as a set of interacting systems, each of which may be studied 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 Locan (section 2.1-2), does not characterize just finnish practice, as they suggest: far from it! One of the most fundamental premises of generative grammar as it was originally proposed was that the rep-...
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sense, not 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. There is a rule that deletes a stem-final vowel when a suffix follows, followed by a second, 'later' rule which devotes an obstruct when a voiceless obstruct precedes. This latter rule is a perfectly ordinary, garden-variety rule of voicing assimilation in obstruct clusters, but for it to work, it requires a representation in which the underlying intervening vowel should no longer appear. Thus underlyingly "sounding vowel" surfaces as [s][t]. It won't do, either, to say that the deleted vowel is not really there underlyingly (that is, it won't do to write this vowel-less in due to stem-allomorphy), because there is another phonological rule, one that deletes obstruents that are immediately followed by sonorants (underlyingly /buge/na/ surfaces as [bugendo]), 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, /tuto-go/, which surfaces as [tuto]. 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 me 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 I think 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 pet formulas are (as we say on this side of the Atlantic) rather mickey mouse. Do I worry too much about using 'space' for 'time' in my phonological representations? You bet I do, and that concern is not modified by the fact that all current theories of phonology do the same, including prosodic analysis. 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 (rel. or

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3. See Kuhn (1962: 95 f.).

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

5. I should point out that the metaphor at play isn't 'true-st Quesque', if 'space' is taken to be three-dimensional, as it usually is, what's at issue is modelling time as a line, that is, as a 1-dimensional 'space'.

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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 resident of the Journals of Linguistics can easily be tricked into thinking that there is more solid common ground to be shared by members of the LGCS 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 not only uneasy about talking about finnissinsns as a generative point of view, and have no problem 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 finnissinsns use to talk about phonology. But I'm not bowed (none of us are bowed), when discussing finnissinsns in the 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 closer together, whereas Ogden & Local's response aims at increasing the sense of incomensurability between the frameworks. I don't buy that incomensurability, by and large; I think we're all working on the same problems, and we have 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 finnissinsns and practice, and in which they make a plea for preserving finnissinsns 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 in the commitment to an ongoing dialog with other schools of thought, and not all of those dialog will be conducted in prosodic theory's native tongue.