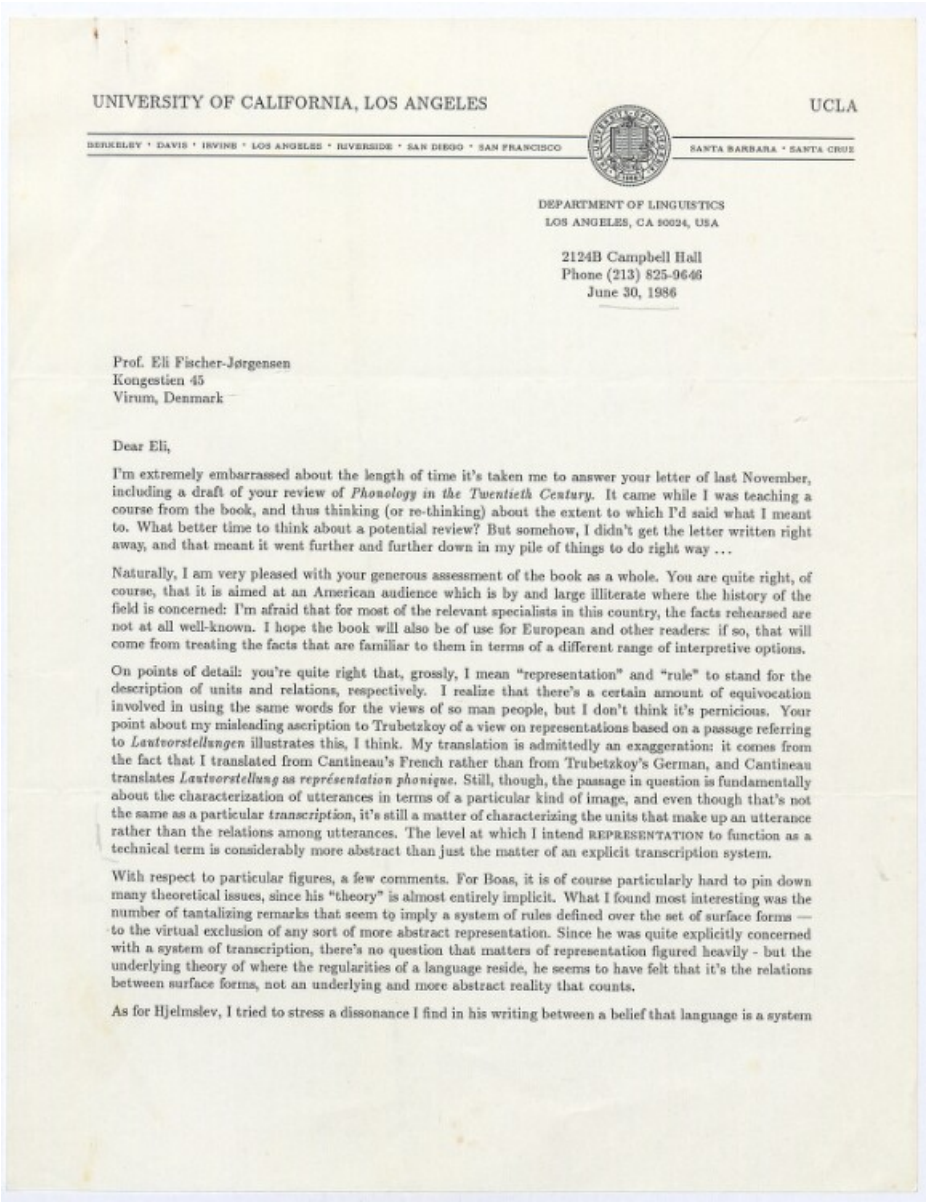
Titel: BREV TIL: Eli Fischer-Jørgensen FRA: Stephen Anderson (1986-06-30)



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DEPARTMENT OF LINGUISTICS
LOS ANGELES, CA 90024, USA

2124B Campbell Hall
Phone (213) 825-9646
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Prof. Eli Fischer-Jørgensen
Kongestien 45
Virum, Denmark

Dear Eli,

I'm extremely embarrassed about the length of time it's taken me to answer your letter of last November, including a draft of your review of *Phonology in the Twentieth Century*. It came while I was teaching a course from the book, and thus thinking (or re-thinking) about the extent to which I'd said what I meant to. What better time to think about a potential review? But somehow, I didn't get the letter written right away, and that meant it went further and further down in my pile of things to do right way ...

Naturally, I am very pleased with your generous assessment of the book as a whole. You are quite right, of course, that it is aimed at an American audience which is by and large illiterate where the history of the field is concerned: I'm afraid that for most of the relevant specialists in this country, the facts rehearsed are not at all well-known. I hope the book will also be of use for European and other readers: if so, that will come from treating the facts that are familiar to them in terms of a different range of interpretive options.

On points of detail: you're quite right that, grossly, I mean "representation" and "rule" to stand for the description of units and relations, respectively. I realize that there's a certain amount of equivocation involved in using the same words for the views of so man people, but I don't think it's pernicious. Your point about my misleading ascription to Trubetzkoy of a view on representations based on a passage referring to *Lautvorstellungen* illustrates this, I think. My translation is admittedly an exaggeration: it comes from the fact that I translated from Cantineau's French rather than from Trubetzkoy's German, and Cantineau translates *Lautvorstellung as représentation phonique*. Still, though, the passage in question is fundamentally about the characterization of utterances in terms of a particular kind of image, and even though that's not the same as a particular *transcription*, it's still a matter of characterizing the units that make up an utterance rather than the relations among utterances. The level at which I intend REPRESENTATION to function as a technical term is considerably more abstract than just the matter of an explicit transcription system.

With respect to particular figures, a few comments. For Boas, it is of course particularly hard to pin down many theoretical issues, since his "theory" is almost entirely implicit. What I found most interesting was the number of tantalizing remarks that seem to imply a system of rules defined over the set of surface forms — to the virtual exclusion of any sort of more abstract representation. Since he was quite explicitly concerned with a system of transcription, there's no question that matters of representation figured heavily - but the underlying theory of where the regularities of a language reside, he seems to have felt that it's the relations between surface forms, not an underlying and more abstract reality that counts.

As for Hjelmslev, I tried to stress a dissonance I find in his writing between a belief that language is a system

of relations, and a practice that is oriented toward defining a system of units. We all wind up constructing theories that only partially realize our notion of what theories are about: in my own case, all of my efforts to build a theory of morphological structure as a system of relationships among words (rather than rules for concatenating units taken from an inventory) have been only partially successful, since I find it virtually impossible to avoid a descriptive paradigm that takes a basic form and makes changes in it on the way to the surface — re-introducing, apparently, an analysis of words in constituents that I think fundamentally misconstrues things. But I don't know how to do it in another, conceptually more nearly adequate way. I'm not trying to draw a real parallel between myself and Hjelmslev (!), but I do think we sometimes wind up with theories that say things in ways we don't want them to simply because it's next to impossible to rethink the whole project of what it is to do linguistics *de novo*. Lord knows, if anybody did it was Hjelmslev, but I think the fact that even he wound up with a theory that focused less on relationships than he intended it to is an interesting comment on the reason one ought to study the history of the field.

With respect to my characterization of Saussure and Boas as having a "fully specified surface variant theory", I agree that the term was not well chosen. The word "variant" obviously implies that these variants are somehow variants *of something else*, and that's just what I deny Saussure, Boas, etc. saw as the truth. The only way to defend my usage is to appeal to the fact that elements of linguistically significant form are specified to a degree that *most subsequent theories* would treat as that of non-distinctive variation: somewhat more abstract for Saussure than for Boas, but in both cases a level that others would think of as derived. But of course these "surface variants" (from a later perspective) were not at all "variants" of something else for Boas or Saussure.

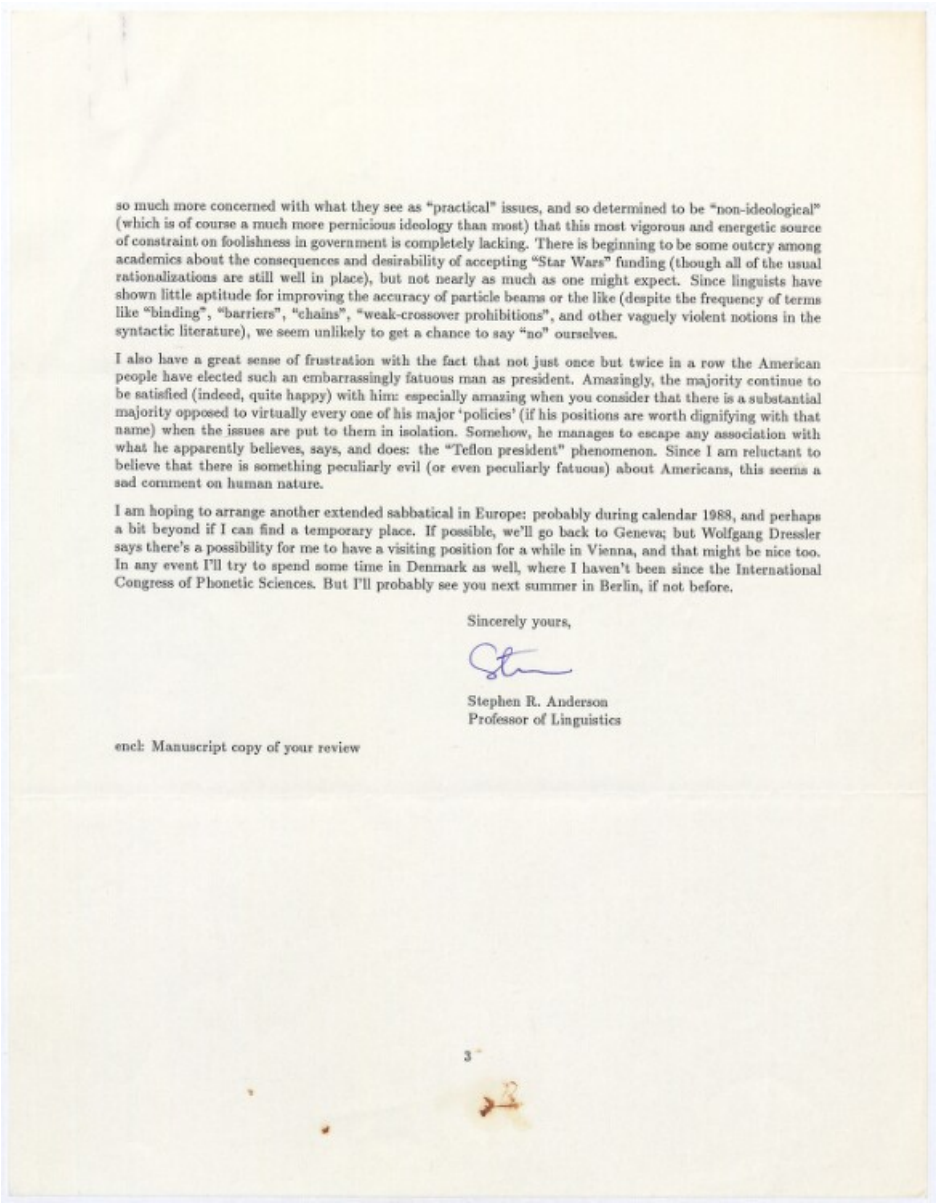
The other points you raise are not, I think, really matters of disagreement between us. The relative importance of the sentence in American vs. European linguistics does not, I think, impugn the claim that a shift in emphasis to the properties of systems of rules represents a very considerable change of focus. And as far as the appeal to non-segmental properties in the work of people besides Trubetzkoy, I certainly tried to stress that element in my chapter on Hjelmslev. Many other people have of course presented theories of the syllable *per se*, but I think there are few places where an extensive worked-out analysis of other 'prosodic' properties (as opposed to a mere programmatic statement of principle) is as closely tied to the theory of syllable structure as in Trubetzkoy.

Anyway, I haven't any complaints to make about the treatment my book receives at your hand: on the contrary, I'm decidedly grateful for the nice words. Of course, I probably couldn't have written it if I hadn't had your own work to crib from; and I probably wouldn't have gotten to the point of thinking seriously about the historical issues involved if not for the experience of reading the early drafts of your book years ago in Copenhagen. So on all counts, thank you.

I hope that you are well, and that the illness you refer to (which seems to have reduced your efforts to only one working day per day!) was not serious. In any event, the amount of energy you have always had is truly remarkable, and I can imagine that with fewer constraints of a bureaucratic nature to hold you down, it is necessary to do something to keep you from doing absolutely everything!

I was sorry to miss seeing Jørgen Rischel when he was here recently: I was at a morphology conference in Hungary at the time, but my wife Janine (whom I believe you have not met?) was able to go to Vicky's for brunch with him one Sunday. Please give him my regards.

Upon re-reading your letter, I wish I could say that our country has begun to behave more sanely since last November, but I am sadly afraid it is rather the reverse. The present generation of students, for one thing, is



so much more concerned with what they see as "practical" issues, and so determined to be "non-ideological" (which is of course a much more pernicious ideology than most) that this most vigorous and energetic source of constraint on foolishness in government is completely lacking. There is beginning to be some outcry among academics about the consequences and desirability of accepting "Star Wars" funding (though all of the usual rationalizations are still well in place), but not nearly as much as one might expect. Since linguists have shown little aptitude for improving the accuracy of particle beams or the like (despite the frequency of terms like "binding", "barriers", "chains", "weak-crossover prohibitions", and other vaguely violent notions in the syntactic literature), we seem unlikely to get a chance to say "no" ourselves.

I also have a great sense of frustration with the fact that not just once but twice in a row the American people have elected such an embarrassingly fatuous man as president. Amazingly, the majority continue to be satisfied (indeed, quite happy) with him: especially amazing when you consider that there is a substantial majority opposed to virtually every one of his major 'policies' (if his positions are worth dignifying with that name) when the issues are put to them in isolation. Somehow, he manages to escape any association with what he apparently believes, says, and does: the "Teflon president" phenomenon. Since I am reluctant to believe that there is something peculiarly evil (or even peculiarly fatuous) about Americans, this seems a sad comment on human nature.

I am hoping to arrange another extended sabbatical in Europe: probably during calendar 1988, and perhaps a bit beyond if I can find a temporary place. If possible, we'll go back to Geneva; but Wolfgang Dressler says there's a possibility for me to have a visiting position for a while in Vienna, and that might be nice too. In any event I'll try to spend some time in Denmark as well, where I haven't been since the International Congress of Phonetic Sciences. But I'll probably see you next summer in Berlin, if not before.

Sincerely yours,

Stephen R. Anderson
Professor of Linguistics

encl: Manuscript copy of your review

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