

## A talk with Neil Smith

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*Q: Your work and interests in linguistics have been very varied, and they have touched upon many different areas, could you tell us a little bit about the development of your research interests in linguistics?*

N.S: It started when I was a student at Cambridge and I was bored with modern languages, which was mainly romance philology and the history of German literature before 1500. So I did a paper in linguistics and got hooked on traditional (pre-Chomskyan) linguistics so hooked that I then came to London to do a PhD. I was caught by the idea of doing field work on an unwritten language so I read books on anthropology till I found a nice one, namely the Nupe in Nigeria. Then I hitch-hiked out to Nigeria and spent a year sitting in a mud hut working on the language. And then I wrote out my thesis which was a grammar of Nupe. Although it was on Nupe it was a thesis in general linguistics, influenced mainly by Michael Halliday and his work - Halliday was in London at that time. As a result of that, I was given a job at the School of Oriental and African Studies (SOAS), where I was converted to generative grammar by one of my colleagues in the Department of Indian Studies. As a result of that I decided to go to MIT and spend a couple of years there, from 1966 to 1968, with the idea of working on generative grammar of West African languages. In particular, I wanted to look at the syntax of Ewe, but when I got there Chomsky was on sabbatical and I was adopted by Morris Halle, who was very kind to me and inspired me with an interest in phonology which I had not had before, at least generative phonology. As a result of that, I spent most of my time at MIT, at least for the first half, working on phonology. Then Chomsky came back and I worked on syntax. I also spent some time in California (at UCLA) working on African languages and linguistics, but my first really decent work as opposed to the dabbings in West African languages arose from that, because while I was in the States my first son was born and I started work on language acquisition in about 1968, when he started to speak.

Because I had spent so much time working on phonology it turned out to be a book on the acquisition of phonology rather than the acquisition of syntax. It was as a result of Halle's influence and the fact that I had been able to work at MIT that I could do that.

But I was only there for two years and when I got back I was working mainly on syntax and general theory, but I was doing research on phonology. And that left me interested in phonology for many years. It was probably another ten years before I gave up phonology and I haven't done much in that since. After that I suppose it was a continuation of my conversion to theory. When I started as a student I was only interested in facts, and description, and it was really at MIT that I began to be interested in explanations and theory. That developed with my work on the acquisition of phonology, and was encouraged when I got back mainly by Deirdre Wilson with whom I worked very closely. We wrote a book together<sup>1</sup> and that and other things I did on the philosophy of language was a development from description to explanation.

*Q: What did you find in theory that was more motivating than continuing with description?*

N.S.: I suppose the real reason was that every time you describe something you just add another little pebble on top of your heap, following the old fashioned idea that science is cumulative; you add a brick to the wall, everything is factual and so on. I think the problem with that view is extremely well-expressed by Popper in his *Conjectures and Refutations*: in order to be intellectually honest and in order to be able to make a serious contribution, you need to state the conditions under which you will give up your beliefs rather than simply add to them. That's a position that later became known as naive falsificationism, but for a long time I found it very appealing and I decided that, although I was quite good at manipulating data, it wasn't really the description that turned me on as much as what might constitute an explanation. It gradually dawned on me, much later than it should on most people and much later than it does on students these days, that if one wants an explanation one has to have a theory. And the only coherent theory that I came across was Chomskyan in one sense or another. At the same time, I got interested in pragmatics because of Deirdre Wilson and her work with Dan Sperber<sup>2</sup>. So, I set up a conference on *mutual knowledge* that resulted in a book that I edited, which sort of generalized my interest away from core linguistics to problems of communication. Again the idea was to see how on earth one could explain how communication took place. Although the book was called *Mutual Know-*

1. Smith, Neil and Deirdre Wilson. 1979. *Modern Linguistics. The Results of Chomsky's Revolution*. London: Penguin.

2. Sperber, Dan and Deirdre Wilson. 1986. *Relevance*. Oxford: Basil Blackwell.

ledge, there wasn't really a consensus view but the conclusion I drew from it was that mutual knowledge was a waste of time.

*Q: What does mutual knowledge stand for?*

N. S.: "Mutual knowledge" was a locution devised by Schiffer in the 1960's and the idea was that in order to communicate successfully you needed to establish mutual knowledge between speaker and hearer. Sperber and Wilson have essentially demolished that position and replaced it by a situation in which you optimize *relevance* rather than dealing in terms of *knowledge* which presupposes factuality or truth, and factuality is not necessarily important. That again was a diversification into greater interest in theory. And I suppose all the time what I try to do is unite as many different strands in linguistics as I can, so that I have an overall conspectus of the discipline, which includes phonology, syntax, and a little bit of semantics and pragmatics, as well as detailed analyses of any one part of it. I have always been a jack of all trades and tried to get my fingers into as many pies as possible and draw everything together, so that there is an overall theory that will account for everything that is going on.

*Q: You've mentioned so many different areas in the answer to the first question that I guess I will now ask you to expand on some of the different things that you've talked about. One of the first things you've mentioned is language acquisition and how you became interested in it. What is the present state of first language acquisition studies and what has been achieved?*

N.S.: It's sort of fun at the moment because so much seems to be going on simultaneously. There's Radford's recent book<sup>3</sup> and that work has been influential, but I think the real turning point came with the arrival of the *principles and parameters* framework<sup>4</sup> at the end of the 1970s and beginning of the 1980s in Chomsky's theory, where at last it began to seem possible to provide some sort of explanation of how it was that children could learn their first language. In Chomsky's early work he had postulated a language acquisition device which presupposed some kind of evaluation measure. There were all sorts of problems with the evaluation measure: how on earth the child could choose among competing descriptively adequate grammars, and if you had a descriptively adequate grammar why would you want to go and look at seventy more of them and then start choosing between them by whatever kind of metric. The details of the metric were never particularly well worked out, anyway.

3. Radford, Andrew. 1990. *Syntactic Theory and the Acquisition of English Syntax*. Oxford: Basil Blackwell. See the review section of this issue for a review of this book.

4. See the review section in this issue. The following books reviewed there are within this framework: Ouhalla (1991), Radford (1990), Botha (1989), Speas.

The developments in the principles and parameters framework seemed to reiterate the kind of work in biology especially in immunology. It is described quite nicely in an article by Piattelli-Palmarini<sup>5</sup> where he describes how work in immunology underwent a change from being what one might call instructive to being selective. That is, it used to be believed that the system had to be instructed by impinging external stimuli in order to react appropriately and develop the correct kind of antigen. Then gradually all those theories were replaced by a selective theory that presupposed that the organism had available right from the beginning all conceivable antigens and therefore all it had to do when some external stimulus impinged was select the correct antidote from an antecedently available reservoir. If you think about it, that is exactly similar to what happened with having an evaluation measure where you had to generate a particular response to the incoming stimuli, where you constructed a grammar and then when you got lots of them you chose among them. In principles and parameters you don't really have to go through that procedure, there are no rules of grammar anymore, what you have is universal principles, a number of which are parametrized and, of course, you have lexical entries, and opinions differ as to where the parametrization lies. In the case of language then what happens is that the child selects from an antecedently available set of grammars the actual grammar that he happens to be exposed to. That is an interesting transition in that it now makes it feasible to acquire language if there is essentially one supergrammar specified by Universal Grammar (UG) and the idiosyncratic differences across languages can then be selected from that available set perfectly straightforwardly. I think that development especially in syntax and gradually in phonology has had a beneficial influence on language acquisition research because people can begin to see how they can answer the questions as to how children can possibly solve particular problems - of negative data, problems of retreat, problems of tadpoles becoming frogs and so on. I think Chomsky's influence in that respect has been for the second time extremely beneficial to language acquisition research.

*Q: Could we say that it has given it a methodology?*

N.S.: Right, it's given it a methodology and it's given it a framework which makes it look like a feasible enterprise rather than a miracle. In some of his earlier work, Chomsky drew the distinction between problems and mysteries. Problems were those things which one might rationally address and solve. Mysteries were really fascinating but phenomena which one had really no hope ever of understanding, for instance, the mystery of free will. I think the principles and parameters framework has probably made language acquisition

5. Piattelli-Palmarini, M. 1989. "Evolution, Selection and Cognition: from "Learning" to Parameter-Setting in Biology and in the Study of Language". *Cognition* 31. 1-44.

study change from being some kind of mystery to being a problem that one might conceivably solve. And both in syntax and phonology there is a huge amount of work now attempting to put into practice the program that Chomsky devised in the late 1970s and early 1980s. There are further - maybe more technical, but quite fascinating - developments in first language acquisition arising from the details of syntactic theory. In particular, if you think of what's happened in post-Barriers work where X-bar syntax has been generalized to functional categories. Radford's work and a lot of other work has concentrated on the development of functional categories and there's the interesting hypothesis that what characterizes children's speech is the absence of functional categories, and that simple claim, given that we now have a suitably complex and well articulated theory of what the functional categories are and how they might even constitute some sort of natural class, makes empirical predictions across a huge range of constructions. For instance, if there are no functional categories and if functional categories are responsible for things like directionality constraints then it would follow that little children have no directionality constraints and they ought to have basically freer word order. Now, again and again one looks at child language data and one sees that there is much more free word order than there is in the adult language - unless, of course the adult language has totally free word order. But if you look at French and English and Spanish and Irish, lots of languages, children's language always seems to display greater freedom of word order than the adult language, and now we have an automatic explanation for that without having to make up some other kind of story. Ianthi Tsimpli's recently completed London PhD thesis<sup>6</sup> is an elegant demonstration of these points.

*Q: You are saying that research in first language acquisition from a generative perspective (specifically within the principles and parameters framework) is valid and desirable.*

*Is other work in first language acquisition useful?*

N.S.: I think we're now back to the dichotomy we went into before with regard to theory and description. I think that what is lovely about current work in first language acquisition is that it really is imbued with a theory which, for independent reasons, seems reasonably good and has begun to make predictions across a wide enough range for people to: a) take it seriously as a syntactic theory or as a general theory of language; and b) to have implications across a really wide domain. Now, there's clearly need and room for all sorts of studies on first language acquisition in what one might think of as a theory-neutral framework. There is the *chilides database*, for instance, and if you want to find out the

6. Tsimpli, Ianthi M. 1992. *Functional Categories and Maturation: The Prefunctional Stage of Language Acquisition*. PhD thesis. University College London.

incidence of free word order in Catalan, for example, you can look it up to see if there are any studies in the language. If so they can be encoded and that information can be accessed and used for further interpretation. But if one wants explanation then one has to go beyond this. I don't know of any other theory of first language acquisition that is remotely plausible or that has anything like these results. Now, of course, that doesn't mean there is agreement, there was radical disagreement at the Boston Conference<sup>7</sup> a few months ago. There were almost stand up fights between those who thought functional categories were available from the beginning and those who swore that functional categories matured later on. Nonetheless, there are empirical issues bearing on this, and it should be possible to come to a conclusion. But one can only ask these questions once one has a theory of functional categories. I mean, previously people would say things like "children's speech is vaguely telegraphic "or" they miss out the function words", but there wasn't a theory of function words and nobody knew what function words were.

*Q: Can second language acquisition be useful for the studies on first language acquisition? Are the two processes parallel?*

N.S.: I think they can be. And I think it is a matter of theoretical disagreement at the moment, as to whether the parallels are interesting. The position that I. Tsimpli and I developed in the *Working Papers*<sup>8</sup> recently implies that the parallels are not nearly as great as most people think they are. What we argued for in that paper was that parameter resetting in L2 is not a possible strategy and we did it for various reasons, largely, for instance, because of the discontinuities between L1 acquisition and L2 acquisition; that is, L1 acquisition appears to be deterministic whereas L2 acquisition clearly isn't, you have to work awfully hard and all sort of special inputs have to be taken into consideration. The "end state", the "steady state" that one ends up with after L1 acquisition is pretty uniform, it is close to identical across all the members of the speech community, indeed across the species, but the "steady state" one ends up with in L2 acquisition is clearly not uniform in the same way and is not usually remotely as perfect. That suggested to us that it would be an interesting hypothesis to suggest that, whereas L1 acquisition is correctly characterised in terms of parameter setting - where parameters in the version we're interested in are associated with lexical items rather than with principles of UG as they used to be for Chomsky - with L2 acquisition, parameter setting is no longer an option; that is, you can't reset parameters, all you can do is still

7. The Sixteenth Annual Boston University Conference on Language Development. October 18-20, 1991.

8. Tsimpli, Ianthi and Neil Smith .1991. "Second Language Learning: Evidence From a Polyglot Savant". *University College Working Papers in Linguistics* 3. 171-183.

have access to principles of UG but only non-parametrized ones. Then you have various other possibilities like errors due to general learning strategies - if there are general learning strategies, transfer errors from your first language, maybe inductive errors of one kind or another, etc. Now, if that position is right - and there's a huge literature saying that what goes on in L2 acquisition is precisely parameter resetting -, but if they're all wrong and if we are right, then L2 acquisition can't say nearly as much about L1 acquisition as it could in the other system, simply because by hypothesis now - and hopefully by argument as well - the parallels are not nearly as great as they were supposed to be.

*Q: Which data do you work on to test your hypotheses?*

N.S.: We've looked at various bits of data. The nicest data that we've looked at recently is work with an "idiot savant", a man called Christopher (C). This is a man who is institutionalized because he is mentally retarded such that on non-verbal IQ tests, he scores between 40 and 75. He's unable to cut his nails, he cannot indulge in normal social interaction, he's awkward, he loses his way when he goes from place to place. When he came and stayed in my home for two days, even at the end he was still puzzled as to how to get upstairs. But he can speak 20 languages, with varying degrees of fluency. He knows English, French, German, Spanish, Italian, Dutch, Modern Greek, and some Hindi reasonably well, and then another dozen languages like Welsh, Turkish, Finnish, Norwegian, Swedish, Danish, Polish and Russian not quite so well. As part of our investigation of this man, we've been teaching him two new languages (Berber and an invented language) under controlled conditions, so that we can control the input. The original idea when I thought of doing this was: wouldn't it be nice if he were really learning these languages as though they were first languages? And that when you therefore set a parameter you might get some kind of parametric cascade and you could see what parameters were related to what particular sets of phenomena, and we could have answers to all these questions such as "Is the pro-drop parameter" one parameter or two parameters, because of inversion problems and so on?

*Q: So when you started working on this, you thought that there were more parallels?*

N.S.: At that time, when we were setting this up, I thought there were far more parallels than there turned out to be, but then when we started teaching C two languages it transpired that the kind of mistakes he was making in these - and in fact in other languages especially in Modern Greek, which we've looked at in great detail, but also French, German, Spanish and Italian - were typically characteristic of L2 learners. They were not the kind of mistakes typically characteristic of L1 learners, so you get transfer errors all the time, you get

general learning strategies, you get analogies of a kind you don't find in L1 acquisition. That gradually persuaded us - maybe the others didn't need persuading, I needed persuading - that L2 acquisition was different in kind to L1 acquisition, at least for the majority of people - there's a residue of very skillful expert L2 learners who may retain something of their L1 ability, but I doubt it, I mean, Christopher is extremely good at learning languages and we had an example this week. On Friday, Ianthi and I went to see Christopher, who lives in the North of England, with a Dutch woman who is representing Dutch television - because they want us to talk on Dutch television next month. Now, Christopher knew a little bit of Dutch and we got him to translate a passage a long time ago, which indicated that he had some rudimentary knowledge, but it wasn't particularly good. On Wednesday of last week, the director of the institute where he lives gave him two days off work and a copy of *Teach Yourself Dutch*. He spent the intervening time just reading it and going through all the exercises and so when we arrived he was able to converse in reasonably fluent Dutch. It had all sorts of mistakes, but it was remarkable a) that someone with his mental problems could concentrate to that extent, and b) that he could absorb the material so fast. He clearly does have a phenomenal ability to absorb vocabulary items. But when you look at the syntax, his syntactic errors are nearly all transfer errors from L1, his English mother tongue, or they are general inductive generalizations of a kind that one would expect in any L2 learner, rather than the kinds of mistakes that little children make. For instance, in learning Berber he adopted a general strategy to put all clitics after the verb, whereas in fact in Berber clitics quite often are in second position, if there is a focus marker or a future maker or negation, and in any of those situations, the clitic is attracted and climbs to the superordinate functional category. Now, although he'd had lots of examples of these, he didn't induce that generalization, he induced a generalization which happened to be false, but which was very sensible: namely, "clitic goes after the verb". I don't know of any L1 acquisition data for Berber syntax but that is not a typical kind of child mistake. So we think that there is evidence that L2 acquisition is different in kind to L1 acquisition and certainly there is evidence from Christopher. I. Tsimpli and A. Roussou have got a paper in our *Working Papers*<sup>9</sup> indicating the same conclusion from experiments carried out on adult speakers of Greek learning English as their second language. So, again, there is converging evidence that parameter resetting doesn't take place. So, that's a long-winded way of saying that the parallels between the two are not nearly as close as we might like, but, obviously, if you are going to characterise the notion of transfer error even, then you need to have a theory of what the grammar is so that you know what a transfer error could be. So you can still get evidence for the theory

9. Tsimpli, Ianthi and Anna Roussou. 1991. "Parameter Resetting in L2?" *University College Working Papers in Linguistics* 3.149-170.

of grammar from L2 acquisition and you can still get evidence or useful insights into how you might structure L2 instruction.

*Q: Having talked about all these different areas, do you envisage a general theory of language compatible with the generative approach to grammar on the basis of existing theories?*

N.S.: What I think is significant in a number of recent developments is the way that at least three different strands have come together. The three strands that appear to me as being most important are: the work in generative grammar - especially the syntax of Chomsky, and in fact one can think of the whole of generative grammar including phonology as being uniform in this respect, despite what people like Halle and Bromberger<sup>10</sup> say; the second strand is the work of Sperber and Wilson<sup>11</sup> in pragmatics which is one of the most significant developments in areas peripheral to core grammar of the past ten years - . It represents the growth of a fully explicit and sophisticated theory of pragmatics, the theory of utterance interpretation, and I think Sperber and Wilson's *Relevance theory* has now enabled us to redraw the lines of demarcation in a number of areas so that we no longer try falsely to give syntactic explanations for facts that are not syntactic but are semantic, as happened in the sixties. Now we've discovered that we can give explanations which are actually pragmatic rather even than semantic, and some of my own work has been devoted to demonstrating that you can indeed provide pragmatic explanations for phenomena which simplify the syntax or the semantics. One particular area that I've looked at with my son is on the analysis of conditionals<sup>12</sup>. There are traditional problems in the semantics of conditionals, in particular in the so-called paradoxes of material implication. One of the things we tried to show was that those paradoxes dissolve once you have a pragmatic input rather than simply a semantic or syntactic one. Now, that pragmatic analysis is totally or largely parasitic on the existence of a generative grammar of a Chomskyan kind, but what we want to do is say that certain phenomena of language ought not to be handled by a generative grammar, not even a generative grammar that includes a level of LF where you have particular logical or quantificational statements, but that some phenomena of language will fall out from a theory of pragmatics, a theory of utterance interpretation rather than a theory of utterance meaning.

10. Bromberger, Sylvain and Morris Halle. 1989. "Why Phonology is Different". *Linguistic Inquiry* 20. 51-70.

11. See note 2 and the article by Sperber and Wilson in this volume.

12. Smith, Neil and Arnahl Smith . 1988. "A Relevance Theoretic Account of Conditionals". Hyman, L. & C. Li (eds.) *Language, Speech and Mind: Studies in Honour of Victoria A. Fromkin*, pp. 322-352. London: Routledge.

I said that there were three strands: there's the Chomskyan strand for grammar, there's the Sperber-Wilson strand for pragmatics, and utterance interpretation, but I think that overarching those is the work of Jerry Fodor. In a number of seminal books he's developed a position which is, I think, best summed up in his 1983 book *The Modularity of Mind*, where what he wants to say is that human cognition falls into two camps, if you like. On the one hand there's the central system which performs an integrating function and deals mainly with what one might think of as the fixation of belief - how we know what we know and how we come to know what we know - and that central system is served by a number of input systems - systems for vision, for audition, one corresponding to each of the senses- . In addition there's one further input system for Fodor, language is an input system so each of these systems serves as input to the central system, say, if you like, to provide the grist for the central mill to grind. One of his major contributions is to point out that input systems - both vision and language - have a number of properties in common: they work extremely fast, they are mandatory - you can't refuse to understand something in your own language just as you can't refuse to see that picture there, or a cow, or whatever- . Most importantly, they are informationally encapsulated. This means that the fact that you have knowledge of a particular kind stored in your central system has no effect on the immediate workings of the input system. The obvious example is visual illusions like the Müller-Lyer illusion. You have two lines of identical length but with arrow heads at each end pointing in and pointing out, and the one with the arrow heads pointing out looks longer than the one with the arrow heads pointing in. You can measure them so that you know they're the same length, but you look at it and it still looks as though one of them is longer than the other. That's the essence of informational encapsulation in that your real knowledge has no effect on your visual perception. Similarly, even if you're primed by the context so that you're talking about spiders, if you hear the word "bug", momentarily you access both the word "bug" meaning something like a spider but also the word "bug" meaning "electronic bugging device". Dave Swinney has done some interesting experiments on this, pointing out that for a few centi-seconds you actually have both of these meanings available. Again, you have informational encapsulation in the language system. Now, it seems to me that why this fits together and why it's relevant is that Fodor provides an overall theory of human cognition divided into a central system and input systems, which makes sense of a wide range of phenomena including some phenomena of visual perception, auditory perception and so on. If he is right that language is like an input system - which Chomsky actually disagrees with but not in, I think, very radical ways- , then you have language as an input system, and language has been best characterized by Chomsky's work. So you have the theory of language - you can take Chomsky's theory - and plug it in to Fodor's model. When it comes to vision, you can take David Marr's theory. Sperber and Wilson

fit in in the following way: for Fodor the input systems are amenable to investigation and we can have interesting theories about them and we can study them. But he says, basically, that the central system is impossible to study. We have no access to the central system, and he's very pessimistic about it and says in general, the greater the abstractness of the system the less likely we are to say anything about it. But it seems that Sperber and Wilson's work is precisely a theory of the central system. Sperber and Wilson's work is not just a theory of utterance interpretation, it's a theory of cognition, of the central system, and the "theory of relevance" applies not just to verbal interpretation but also to interpretation of visual stimuli and everything else as well. So if you take the three strands together, you've got Fodor providing some kind of general theory of mind - with input systems which are modular, and the central system -; you've got Chomsky providing probably the most insightful and best analysis of any human cognitive endeavour this century - and Chomsky's theory is itself modular, so that you've got little modules inside the module of language -; and to complement that you've got Sperber and Wilson providing an initial attempt to give some sort of characterisation of the central system itself in terms of relevance. So I see those as ultimately leading to an overview of human cognition which includes language as a special case. This means that we can have a general theory of language, but only now. You know, 30 years ago, Chomsky talked about competence and performance, and he developed a theory of competence, and we are now getting a theory of performance and seeing how both of them fit into a theory of human cognition generally.

*Q: Since you've already talked about syntax and pragmatics and how they've been developed, why don't you tell us about phonology? What do you think the present state of the theory of phonology is?*

N.S.: I think phonology is currently polarized. I feel that *The Sound Pattern of English* (SPE) is still an overwhelming landmark and it's still important. When I wrote *The Acquisition of Phonology* (CUP 1973), it was straight SPE type phonology. Now, in syntax since then there's been a radical change because of the development of principles and parameters. The people who invented generative phonology, Chomsky and Halle, have really continued with it, Chomsky's given up phonology he says quite categorically, he took a policy decision because he didn't have time for that and for politics. Halle has kept on and has published a great deal, much of it very interesting, very seminal, over the years, but a couple of years ago he and Bromberger<sup>13</sup> - I mentioned this in passing a while ago - published a paper called "Why Phonology is Different", which I think is a counter-revolutionary tract attempting to demonstrate that

13. See note 9.

the sorts of arguments that led people to develop principles and parameters in syntax don't carry over to phonology, and that phonology still has to use the same kind of old - fashioned machinery that it did in the 1960's , things like rule ordering. It seems to me that rule-ordering in particular, which they concentrate on, to some extent, is a priori a disaster. If you have a system with rule - ordering it's unlearnable. And given the interest in language acquisition then it seems to me a disaster if you say that your theory requires rule-ordering. On the other hand, there are people in phonology, in particular in what's called "government phonology" associated with Kaye, Lowenstram and Vergnaud<sup>14</sup>, who think that the principles and parameters paradigm carries over to phonology. Their work seems to me on exactly the right lines, even though it's still extremely sketchy, a lot of it ill-thought out, very minimal in the actual factual discoveries or insights it's come up with. But the general meta-theory seems to me to be exactly in the right direction. Now, that dichotomy is not always perceived in the way it is because all phonology - even the sort of stuff in the mainstream developing from SPE - has changed fairly radically and it's changed I think in particular in maybe three ways. One is that the unilinearity of SPE has now given way to multilinearity or non-linearity and so we have autosegmental phonology where you have a number of different tiers of representation taking place simultaneously. Whereas in SPE you would have a sequence of segments where the vowels would be specified for both tongue height and pitch height, nowadays you have two quite independent autosegmental tiers where you have segmental properties on one tier and tonal properties on another tier, and the arguments for this seem to be overwhelming, I think everyone has accepted them, and it's a radical change from when I was working on *The Acquisition of Phonology* (AP) for instance. Similarly, everybody now, I think, accepts that you need some kind of hierarchical structure. That really came in with metrical phonology and again when I did the AP I made various remarks about the need for the syllable and how it would be nice if we had some kind of construct of that kind. In SPE there was no syllable, there was just the feature [ syllabic]. But nowadays everybody has hierarchical structure and metrical phonology has developed it most. Similarly, everybody pretty well now acknowledges the importance of the lexicon so lexical phonology has taken it to its natural extreme, but all current theories of phonology embody claims about the role of the lexicon and usually divide their phonological statements into those that take place in the lexicon and those that don't. So those are three fairly radical departures all of which really characterise everyone working in current phonology. The things that really keep them apart are things like the nature of the primitives, whether it's elements or distinctive features - which strikes me

14. Kaye, Jonathan, Jean Lowenstram and Jean-Roger Vergnaud.1990. "Constituent Structure and Government in Phonology". *Phonology* 7. 193-231.

as not particularly important - , the question of rule-ordering - which strikes me as VERY important for language acquisition reasons -, but most of all the status of rules as opposed to principles. All the other kinds of phonology except government phonology still tend to be preoccupied with rules. Rules for Chomsky, for instance in syntax, are now purely epiphenomenal, they have no primitive status, it's only the principles and the individual lexical entries that have real ontological status in the grammar. That's the case also for Kaye et al's "government phonology", they don't want rules any more either, if you have a rule it must be wrong. They want principles and the instantiation of those principles in particular geometrically defined sequences will give you all the output you want, and therefore they don't have to postulate individual rules. That seems to me to be the direction that we ought to go in. But very little progress has been made yet which is why it's not widely known except in London, and not particularly popular but it seems that that's the way things ought to go, and to adopt the kind of strategy that mainstream post-SPE people do -like the Bromberger and Halle paper I mentioned - seems to me theoretically undesirable a priori.