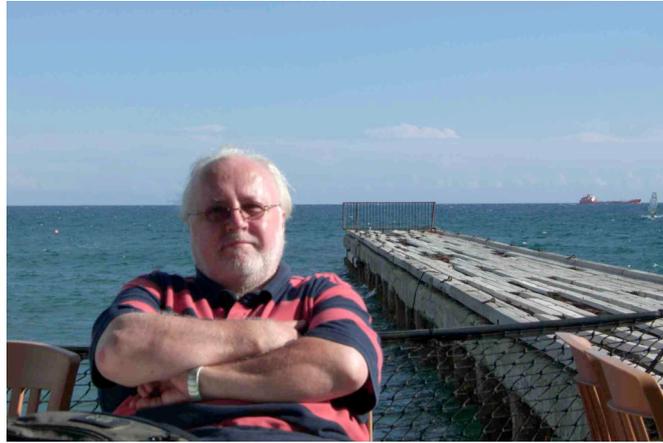


An Interview with Henk van Riemsdijk



by Kleanthes K. Grohmann

Platres & Zygi, Cyprus (14 October 2006)

*Henk, it's mid October 2006. Let's pretend it's already 2007 — the year this interview will be printed in the new online journal *Biolinguistics*. Before we talk about you, your visit to Cyprus, your work in linguistics, what comes to your mind when you hear these keywords: 2007, new online journal, and *biolinguistics*?*

One thing that, in retrospect, strikes me about *Syntactic Structures* [Chomsky 1957], the book that got me into generative grammar, is that it may well be the one publication by Chomsky that is least concerned, at least overtly, with mentalist issues. It seems only fitting that 2007, the 50th anniversary of *Syntactic Structures*, coincides with the first appearance of the new online journal *Biolinguistics*.

That's right. You mentioned this in the autobiographical introduction to your talk on Wednesday night [van Riemsdijk 2006c], when you said that you looked for some historical developments of generative grammar, in particular the possible genesis in Chomsky's writing of what we would now call the "biolinguistic" approach to language. Could you please briefly rehash this for a wider audience?

The following is a heavily shortened, edited, and amended version of the original interview. It basically covers the third part of the interview of which a fuller version can be obtained from <http://www.punksinscience.org/kleanthes/BL/HvR.htm> — but that is still an edited version (expletives and personal issues deleted); for legal reasons, the spoken corpus can *not* be made available to a wider public. The amendments to this interview are obvious: In the interest of the reader who may not be a specialist on all issues addressed, I added references to the literature as we mention them.



Well, what I said in the talk was that only two years after *Syntactic Structures*, he came up with the Skinner review [Chomsky 1959] and that was, as far as I am aware of, the first clear sign that Chomsky was adopting that frame of mind. I sort of said jokingly that in retrospect, if you tell Chomsky that he only came up with that stuff later, he would probably deny it and say that it had been clear to him right from the start — and, you know, who am I to say that he would be lying? All I'm saying is there was no real evidence in the writing that that was the main goal he was pursuing. It would actually be interesting at some point to ask him this question.

And you also mentioned that you looked for some specific keywords, like 'cognitive', 'biological', and you found them in LSLT [Chomsky 1955] — but only in the introduction to the published edition, which Chomsky actually wrote in 1975.

Yeah, that's right. [But bear in mind that] the publication of *Syntactic Structures*, which was like a brief summary of *LSLT*, was in part specifically designed to get his career at MIT going, and as we well know from that time, much of the research was financed through the military, and what the military were interested in was information theory. And so, clearly what he emphasized was the stuff about the Chomsky hierarchy [Chomsky 1956], things like that. I mean, if he were to say, "Well, I didn't want to endanger the success of linguistics at MIT by throwing in this psychologizing stuff that the real hard scientists are not necessarily gonna be very enchanted by" — that in psychology, in the dominant views on psychology at the time, were absolutely abhorrent — who am I to say again that that wasn't the best strategy?

In the last few years the term 'biolinguistics', has become one of those buzzwords, if you like — of course, the term has been around for long, much much longer [for a critical query in this direction, see, for example, <http://linguistlist.org/issues/18/18-1379.html#2>; see also the editorial to this volume, Boeckx & Grohmann 2007] — but what's your take on, let's say, the current use of the term, and possibly the work connected to it?

I think the first thing I want to say is that I'm glad that people talk about it — it shows a certain level of awareness, that this is what we're really doing all this for. The second thing is that it's actually kind of interesting that, just as we're talking more about biolinguistics, there is the "third factor" [Chomsky 2005] being emphasized in language acquisition, namely general principles of design. Now, general principles of design could be truly biological principles of design. But in actual fact, when you look at the examples that are discussed, they're really principles of design concerning the whole physical world. And what are the principles of design that people talk about? Economy and locality — and indeed these do seem to determine many properties of the physical world. So, one somewhat diabolical interpretation would be to say that, just as we are beginning to talk about biolinguistics, the main focus of attention shifts to non-biological principles. [LAUGHTER.]

This said, I think that would be too diabolical and in fact the wrong way of looking at it because what we're looking at is, of course, how laws of nature manifest themselves in biological systems. There already we face a reduction problem because we don't really know whether to talk about these things in

terms of, let's say, a physical theory, with physical primitives and a physical alphabet, or whether to talk about these things in terms of biological objects and biological principles, biological alphabets, and so on; axioms, if you wish. And then, when we try to interpret biological principles of design in the role that they play in grammar, we face yet another step of reduction of this sort because we don't really know how to talk about biological systems that are dealing with DNA, with biochemistry, with cell structure, and so on, and how to connect that sort of thing with the sort of notions that we work with in linguistics.

Such as economy or locality. The displacement property of human language.

Exactly. It's quite clear that a great deal of thinking is gonna be needed before we can do anything really significant. And I'm actually hoping that whatever is going on around the world in terms of activities on the biolinguistic front, including the new journal, is going to help set the stage for such work.

That's certainly also our hope, as editors, and one of the reasons why we have people like you on the Advisory Board to discuss some strategies in the future. Right now, for example, this year [2006], a new e-journal was started in the biological world, the journal Biological Theory [<http://www.mitpressjournals.org/toc/biot>] (even if backed by an established publisher), with which we hope to have some interaction for drawing papers, submissions, discussion, and so on. This ties in with what you just said. We don't really know yet how to talk about some of these things. Also, you and Riny [Huybregts] organized this conference at Tilburg University some years ago, on biolinguistic matters with all kinds of linguists, psychologists, biologists, neuroscientists, right?

Oh yes, with lots of people. The conference was called *The Genetics of Language* [<http://linguistlist.org/issues/12/12-682.html#1>], and what we actually tried to do was mainly to create a bridge between several, rather independent brands of, call it, biolinguistics. For one thing, the people who work on population genetics try to find some significant answers to questions like, "Is the Black Eve Hypothesis correct or not?" — that is, that language originates in one single spot, and migrated and diverged from there on — or whether we should adhere to a multiple-origin story. The other thing was biolinguistics in the stricter sense: "Is there anything we can say about the structure of the human brain in relation to the property of grammar?" And there was particular emphasis on SLI, specific language impairment. We had some of the major players of that subfield at the conference. If I'm honest, I would say that interaction between these different groups left something to be desired. But then, I think, it is easy to be over-optimistic in these matters, and I'm still hoping that the very fact that these people interacted and participated in the same conference is in itself already a significant step in the right direction. And in fact, Lyle Jenkins produced a book that was largely the result of that conference [Jenkins 2004].

That is correct. That a lot remains to be desired in these interactions is natural, and it happens time and time again. The Genetics of Language conference is one example and, from what I hear, something similar happened this year at the summer school in San Sebastian, where you had a bunch of geneticists, biologists, psychologists, Chomsky [Piattelli-Palmarini et al., in press; see also Chomsky 2007, in this volume] — that they were talking to, rather than with, one another. I wasn't there, so I don't really know.

Specifically, if someone came to you, a colleague, a junior colleague, maybe a senior col-league, and says, “Hey Henk, I saw your name on the advisory board of this new journal, Biolinguistics — what do you expect to be doing there, what do you hope for?”

The first part of the answer would simply be that I’m on the advisory board of a number of journals, and in general, I don’t do anything. I get asked to do reviews of manuscripts a lot, but there is very little correlation between the journals for which I’m asked to do reviewing and the journals that I’m on the board of. Now, at the same time, I don’t really like to lend my name to just anything, and the fact that my name is there minimally should be interpreted as meaning that I think it’s a good idea that such a journal exists. And the reason that I think it is such a good idea is that it’s a field that deserves attention, in which a lot of work should be done, and that has a chance of going somewhere. Now, I want to correct this to a certain extent, which is, that I seem to be suggesting that it’s a separate field. But of course, what this means is that it just emphasizes that what we do as linguists is work that is ultimately to be taken to be part of the biolinguistic enterprise.

Excellent. That leads me straightaway to the next question. How — in the context of using the term ‘biolinguistics’ or anything like that — is biolinguistics today different from biolinguistics back then? Like in the ‘70s, when, for example, Lyle Jenkins started, or tried to start — I don’t know whether you know that — a journal which was to be called Biolinguistics, and he got support from a number of Nobel-prize winners in the natural, biological sciences [see Jenkins 2000 and this volume’s editorial, Boeckx & Grohmann 2007]. You had the Harvard and Royaumont conferences in 1974, 1975, organized by Massimo [Piattelli-Palmarini 1974, 1980]. So that era and the notion of biolinguistics versus today — what is different?

Well, first of all, I would say that in the realm of results, results that can be called ‘biolinguistic’ in a specific sense, there isn’t that much to report. I would say that developments in SLI are at least interesting [e.g., Leonard 2004, Wexler 2004], and there are some clear indications now that there are genes that are in some way, that we largely don’t understand, involved in matters that pertain to language in the brain.

A second remark that I think should be made is that it’s easy to mistake the boom in studies in neuro-imaging as constituting a very significant step ahead. I’m not an expert, and I by no means read everything, but from what I have seen, there really isn’t that much by way of significant progress to report. It appears that the level of resolution at which we currently are — but even if it goes much further — is still a long step away from telling us anything about the sort of interesting properties that generative grammar has found as being extremely unique to human language. Take recursion as the first thing that comes to mind — these two subfields just don’t touch each other, not by a long stretch, and in fact I think we shouldn’t necessarily be too optimistic that this is just a matter of time. It may just be that imaging of this type is simply not gonna tell us anything about things like binding, or subadjacency, or recursion, or Merge, or whatever.

Well, there are some current attempts — I’m not sure how you see that, given that I come from Maryland, intellectually speaking: Dynamic systems and the biological world, Juan

Uriagereka's attempts of tying in the Fibonacci sequence in an interesting way as a property of language [Uriagereka 1998; see also David Medeiros' work cited below and Soschen 2007].

Yes, I recognize that there are a bunch of things floating around, and in all modesty I would include my own *XX-work among the same [read: "star double-X"; van Riemsdijk 2006c, in press]

And grafts [van Riemsdijk 2006d; see van Riemsdijk 2001, 2006a, 2006b]...

And grafts, well —

Maybe not.

No, I mean the fact that I am using a botanical metaphor for that really has nothing to do with the biolinguistic perspective. Not any more than the fact that we have been calling syntactic structures "trees" for many decades now, so I would leave that out.

*Let's talk about *XX then.*

First of all, it is something I perceive as recurring, popping up all the time, and it's something that is absolutely not unfamiliar when we look at the physical world. Now, as we know, perceptions may be very deceptive, so it's easy to be deceived, but for the time being I think there might be something significant going on, and I would consider Piattelli-Palmarini & Uriagereka's [2004] work on equating feature checking with anti-immune reactions as being in the same spirit.

Again, from the Genetics of Language conference.

Exactly. And yes, taken all together, there may be half a dozen proposals floating around that go beyond mere hand-waving. But frankly, they don't really go that far beyond hand-waving, if we're honest — and I would certainly include my own work on *XX [van Riemsdijk, in press]. But that doesn't mean that it shouldn't be done, because these are the first attempts to try and sort things out in certain ways, and we should just think about these things and keep communicating with each other about these things as a way to get more out of it. I have to confess that, for example, throwing out these sayings about recurring sequences in DNA [van Riemsdijk 2006c] — well, I feel quite embarrassed because I've picked this up from one or two articles that I found in the literature.

Basically, only junk DNA can have identical sequences...

... while recurring sequences occurring in significant DNA always need to have buffers — that's how I read the articles that I have read. But at the same time I have to say, those are articles that I understand only 15% of. Those are difficult, specialized fields, and I don't have a glimmer of a hope to really understand what's going on. Also, I don't really know for sure that those researchers that write this up are taken seriously in that field, so it is quite clear that the only way in which we might possibly get a little bit further, is by starting to talk with these people, but, of course, that, in turn, would imply that we manage to explain to them the sorts of phenomena in linguistics that we think might be interpreted along these lines. And, you know, then the big chasm shows up, which is similar

in a sense to this chasm about the resolution of imaging techniques and what we really want to be looking for. We're not entirely sure that this chasm can ever be bridged, but that's what science is for: I mean, you can only try and hope, and maybe something will happen.

Well, I would say that Cedric [Boeckx] and I have a glimmer of a hope that the journal Biolinguistics may contribute a little bit towards that by — hopefully, eventually, once we have some kind of reputation — approaching geneticists, for example, and fill them in on what we are trying to do and ask whether this makes any sense to them. Or what you just said, “Ok, look, I read this one paper, I understand 10, 15%, this is roughly what I get out of it. Would you agree?” — and then they might tell you, “Well, that's exactly what it is” or that it's not what it really is instead, or that these people shouldn't be taken seriously, and so on.

Exactly, yes.

But you start this kind of dialogue, and hopefully by targeting a wider group of researchers in different fields, this might become common practice, if at all possible.

One would definitely hope so.

Now, you could, of course, argue that the relevance of grafting for the biolinguistic approach [van Riemsdijk 2001, 2006a, 2006b] — but then again you could say maybe that it's just a core syntactic approach — is to take an operation, Merge, seriously, in all its consequences.

Yes.

Yes, but then I guess it's just biolinguistic, biological, as much as Merge is.

Oh yes, absolutely, absolutely, yes. That is, of course, true.

I recall a paper by this guy from Arizona, David Medeiros, who's taken the Uriagereka-line on the Fibonacci sequence seriously and applied it to X-bar structure, and basically derives a system very similar to the X-bar system [Medeiros 2006; see also Carnie & Medeiros 2005 and Carnie et al. 2005]. Something that I tried to do in my dissertation, my book, from a different perspective, by getting away from this bare phrase structure in the sense that we get linear structure [Grohmann 2000, 2003, also Grohmann 2001 and updated versions, but see Atkinson 2007 for a critique] — something you argued for also in Wednesday's talk, that we really have to make available these positions: specifier, head complement [van Riemsdijk 2006c, in press]. These are the serious relations that we have in our phrase structure and nothing else, and you see this over and over again. We should be able to say something about it, derive it. However, it's not so clear to me that bare phrase structure necessarily does that [Chomsky 1994 et seq.]. But then again, of course, bare phrase structure does a lot of other things very nicely, such as taking Merge to successively build our structure.

Absolutely, yes.

Let me move on a little. You could say that GB was the explanandum of the P&P theory of English [cf. Chomsky & Lasnik 1993]. Most minimalists, Chomsky included, at least up to a certain degree, would place minimalism in the wider research agenda — that what we're trying to do is to come up with a theory, or with a set of rules, that describe the

principles and parameters of natural language. I'm not so sure how much our knowledge of principles of UG and parameters in the grammar have developed. Do you still see today's research program influenced by, or should it even be part of, the general P&P approach?

You know, that's a pretty big question.

Well, it was supposed to be the last question.

Yes. Let me perhaps begin by saying that there are a number of aspects of this rather diverse thing called "minimalism" that I definitely think are interesting and worthwhile. That's certainly true of Merge. You know, being very selfish here, I think Merge is a great step forward.

Merge follows from grafts. [LAUGHTER.]

Second, the idea that all conditions can be reduced to interface properties, is certainly worth pursuing.

The strong minimalist thesis [Chomsky 1995, 2000, 2001, 2004].

It's the strong thesis. I am not sure how optimistic I am about success, but again, let's say, it's worth pursuing. Now, there are other respects in which I tend to feel that going from P&P to minimalism has possibly been a step back. I think that the idea that Move can be successfully and explanatorily regulated by features and feature checking of whatever comes after that, has largely failed. It was an interesting idea, essentially taking Vergnaud's idea from that infamous letter in the late '70s seriously [Vergnaud 1977] and trying to extend it to all of Move α . But the thing was — Actually, let me go even one step further. It was thought to be an almost incontrovertible case showing that it was possible to motivate certain types of Move α and to do it by means of features. Now, even there I think there is serious cause for doubt, and, you know, the main reason that I think that that might have been wrong to start with is that it ignores the notion of default case. In fact that's what we see when we look around the languages of the world, when a noun phrase is in a position where it cannot get Case —

Hanging topic, left dislocation, fragments.

Yes, exactly. Why, in English, the way of making up for lack of case is to insert *of*, — that's why a noun with an object gets *of* (as in *the destruction of the city*), so if direct object case is absorbed in passives, why can't you say *It is killed of John*? But none of that obtains and to my knowledge, nobody has ever given a satisfactory answer to that. So, this is just by way of an illustration of the fact that even the best-case scenario for triggered movements is on very shaky grounds and 90% of it is pretty much the worst-case scenario.

That was the motivation for your Triggers conference a couple years ago [<http://www.linguistlist.org/issues/13/13-705.html#1>]; see also Breitbarth & van Riemsdijk 2004]?

Yes, yes. We were very egalitarian-minded, so we invited people from all camps and even more people submitted abstracts and so on — but yes, the outcome was definitely very mixed, very mixed. Of course, you can always say it's too early to evaluate and things like that, but I am tempted to say that that was a wrong

move. Well, I'm not necessarily saying that going back to the old system would be the right move, but I sort of felt that, assuming that Move was really Move α , and to work on a system of constraints that would make sure that overgeneration was taken care of, was a perfectly worthwhile way of thinking about things. And there's nothing in principle that makes it incompatible with minimalism, it seems to me, because after all, we may be able to interpret or re-interpret the constraints that are needed to get Move α to work well in terms of interface properties.

Right. Now, one way of constraining Move α in GB was the modular structure — modular on two levels: First you had various GB modules (Case Theory, PRO Theorem, Theta Theory) that interacted in particular ways; second, you had written into the architecture several levels of control (call them levels of representation). So one core minimalist line of attack would be to ask: "Do we really need all this apparatus, can we reduce it?" [Hornstein 2001; see also Hornstein et al. 2005]. Now, to the extent that what you just said at the end, that you don't really see why GB (or why Move α from GB) could not really be reconciled with minimalism, you could say, "Well, in that technical implementation it can't be, because we don't want these things" (the modular structure and superfluous levels of representation). Or you could go the other way around and say, "No, we don't want them if they are really superfluous, but maybe we really need these four levels" — as Juan Uriagereka argues currently, that we do need some kind of D-structure component [Uriagereka 1999]. So, maybe these levels of representation need not be all interface-driven — or you could say that D-structure is actually an interface level, namely between the lexicon and the derivation. Or you might want to say, maybe you don't like the modular structure of GB, but there is some beauty about that, too — that it's nice to have these different levels, they hang together, they connect.

Absolutely.

So it's not so clear that we should have to throw them out altogether. The other thing is when you said Move α was nice because it was unconstrained — you had to find ways to constrain it and Move, triggered movement for feature-checking purposes, does not seem to do the trick. However, in the very latest version of Phase Theory [Chomsky, in press], of the phase-based approach to syntax, that's pretty much what Chomsky says. He calls it now EPP. Now, the EPP does not have any intrinsic meaning, it doesn't have any meaning related to the '82 EPP [Chomsky 1982], that subject positions must be filled. It just means that we have a p-feature somewhere, an edge feature for various projections — but really he dissociates movement from checking again.

That's true.

We didn't have feature checking mechanisms in GB, but if we had them, given that everything should be done by means of government and c-command and m-command, it's perfectly conceivable to think of something like Agree to work in a GB framework, but then you just have to find some other explanation for movement. For Chomsky [2005], it's the optimal solution.

Yes. I think what you and I both seem to be saying is that one shouldn't exaggerate the discontinuity between GB or P&P on the one hand [Chomsky 1981, 1986, Chomsky & Lasnik 1993] and minimalism on the other hand [Chomsky 1995, 2000, in press]. Actually I think of myself as rather eclectic in this pers-

pective, I just pick stuff left and right, which I happen to think is our best bet at being right. Another thing which actually predates the minimalist discussions is the status of parameters. I mean, at some point it became fashionable to say: “Well, parameters are sort of designed to do language-particular stuff” — but especially as more micro-syntactic stuff came up [see Kayne 2000 for discussion and further references, for example], it became clear that these big, overall sweeping parameters didn’t really work that way, and that they were much more fine-grained and so on.

So it didn’t seem a bad idea to say that if that’s sort of language-particular stuff, why don’t we put it where the language-particular stuff belongs, namely in the lexicon. Ok, fine, but the other thing that was always said about it was that, we know that people learn words the hard way — sort of, you know, they have to pick them up from experience — and parameters clearly have to be learned because they need to be set, so that’s a good reason why we should put them in the lexicon. And as for parameters that are not tied to the lexicon — I mean, despite things like the subset principle and so on, it’s not entirely clear how they are learned.

Probably biological.

Well, frankly, that fails to convince me. I mean, the primary data that are needed to fix something like the Headedness Parameter, is perfectly straightforward, and I have always failed to see the rationale for trying to get rid of the Headedness Parameter. Maybe if it’s possible for us to learn that the English word *cheese* is a noun that means what it does, then why can’t we learn some little property that says, “In this language heads are final”? Big deal. You know, maybe it’s like a big silent word that sits in the lexicon and that we happen not to use to insert into syntactic structures, but that we use as a reference library. I don’t see anything wrong with that in principle. And, of course, that gets us into the whole LCA business [Kayne 1994] and so on. If we look at what a fairly simple thing like the Headedness Parameter buys us and all the trouble that it avoids for us, well, I think I’ll settle for that any time.

Sounds good. Here’s a potential shortcoming of the minimalist approach in this respect — any minimalist approach, from the beginning within the P&P theory — that might be formulated: On the P&P approach — you just mentioned it, that when it was developed, we had some core principles, whatever they are, and it’s true that, if it’s not exactly enough to have the big parameters, people will look for smaller ones. If it’s this type of approach looking at the specification of these parameters — take Clark & Roberts’s [1993] work on parameters, for example — then we haven’t really made that much progress in terms of how we should visualize these purported principles and parameters. Along comes minimalism, which was quite a different approach — without explicitly saying, “Ah, principles and parameters.” No, it says that we can build in parametric variation — strong features, weak features. But does that really tell us anything deeper? How does that fit in — given that Chomsky and others, you and me included, spent a lot of time on architecture, architecture and design. But what is the architecture and design of these principles and parameters? Is it the switch-board analogy [which Chomsky attributes to Jim Higginbotham in this volume; Chomsky 2007]? Is it the Baker-style way, that you can go to parameter 3 only if parameter 2 is switched on [cf. Baker 2001]? If parameter 2

is switched off, then you never get to parameter 3, so it's irrelevant for, let's say, 95% of the languages. I don't know of too much literature that deals exactly with these kinds of questions — apart from the learnability angle, Clark & Roberts and others — I don't know of too much work that tells us something more about design and architecture of principles, and of parameters, regardless of what they are. And I don't think people have really tackled that question in the last 15 years.

Well, I think you are exactly right. Basically, I don't think that in the P&P period people were so lazy. I mean, lots of people were working on parameters, and one of the main things that was happening was that with increased attention for dialects and for more related languages, that is, with increased interest for micro-syntax, people started to realize that these parameters really are about much more fine-grained structure. Well, that was about where we arrived at, when minimalism, you know, hit us around the ears, and that put an end to that. That is, thankfully, research on micro-syntax is still going on, but you don't see these people talking about parameters much anymore.

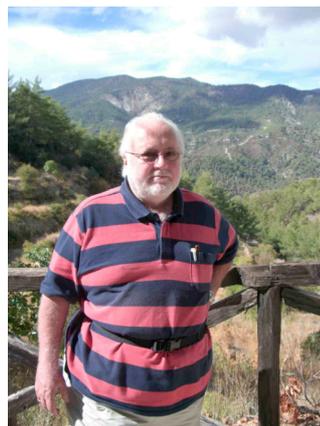
Do you think they would have continued? You could say, "Oh no, minimalism came at the right time" — because at the time, we were not ready yet to spell out our assumptions about the structure of principles and parameters clearer. Bear in mind that all I just said, that was never spelled out by anyone, of course, and Chomsky didn't say, "Hey, let's do this new minimalism thing now because we are not ready yet to continue with the old line of research on principles and parameters."

Yes, I think it's likely that someone would have continued talking about parameters.

So, is the future perhaps Principles-and-Parameters and biolinguistics?

We'll see.

Thank you very much for a stimulating conversation and a wonderful day.



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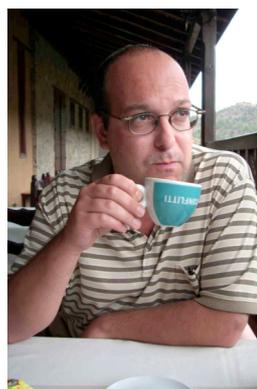
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