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REVIEW ARTICLE

# Functionalism is/n't formalism: an interactive review of Darnell et al. (1999)[1]

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**Michael Darnell, Edith Moravcsik, Frederick J. Newmeyer, Michael Noonan & Kathleen M. Wheatley (eds.)**, *Functionalism and formalism in linguistics*, vol. I: *General papers* & vol. II: *Case studies* (Studies in Language Companion Series **41** & **42**). Amsterdam & Philadelphia: John Benjamins Publishing Company, 1999. Pp. iv + 514 (vol. I) & pp. iv + 407 (vol. II).

SETTING: The University of Arizona's idyllic desert campus. As in many colleges across the United States, 'formalist' linguistics is implicitly understood to be at cross-purposes with 'functionalist' linguistics. The Linguistics Department's only course on non-minimalist syntax is famously nicknamed 'Bad Guys'. Although the linguistics department forms a unified front, malcontent quietly simmers across campus as functionalist sociolinguists, discourse analysts, grammaticalization specialists and linguistic anthropologists outnumber formalists, though they roam within their own language-department fiefdoms. Politeness and cooperation reign among senior faculty linguists, who have realized that antagonism only hurts students and programs in all the language sciences. The junior faculty are more brash: they work hard, publish a lot, and speak loudly to get tenure as respected form/functionalists. They socialize together and joke about each other's positions, but don't talk very much serious shoptalk. Until now …

SCENE: AC, the junior formal syntactician, walks into the local campus coffee shop and sees NMD, a junior variationist sociophonetician, lounging in the corner reading the latest developments in probabilistic sociolinguistics. Unbeknownst to them, they have both recently read the same books, the

[1] The authors would like to thank Heidi Harley, Mike Hammond and an anonymous *JL* referee for their comments, and especially Maggie Tallerman for her boundless patience. The authors blame each other for any mistakes or misrepresentations that are in this review, especially ones that will get them in trouble with their own camp. We would like to note that although each paragraph is attributed to a particular author, we frequently wrote each other's parts. We leave it as a challenge to the reader to figure out which sections were actually written by whom.

two-volume *Functionalism and formalism in linguistics* by Darnell et al. The books share a coffee table with NMD's cafe macchiato.

AC: Hey Norma.

NMD: Hi Andrew, getting coffee? Want to gossip?

AC: Us gossip? Never! <mock look of surprise> Sure, I'll just grab a half-caf cap and be right back. <wanders off stage right>

NMD: Half-caf cap?? <to self> Hmph! A syntactician's drink if I ever heard of one. I wonder what he's been reading lately. Probably some contentless formalisms. Oh! Here he comes.

AC: <returns with steaming cap and glances at the coffee table> So, have you looked at the two-volume debate between the fuzzies and the good guys?

NMD: What do you mean 'good guys'? Are you talking about the *Functionalism and formalism* volumes? I suppose you think the formalists are the good guys?

AC: Of course, the book just proved everything I've been saying to you for the past three years! You fuzzies have it totally wrong.

NMD: <clears throat> I am not a fuzzy! I am a probabilistic linguist, interested in modeling and predicting language patterns. I am one of many kinds of functionalist, so don't lump us all together, we can have very divergent views! <NMD leans angrily toward AC and flicks a bit of macchiato foam, AC smirks, picks the foam off his shirt and licks it off his finger> Well, let me see, I think that in the first chapter Noonan[2] makes some excellent points about the shortcomings of formalist approaches when encountering data from usage and psychological evidence.

AC: Funny, I thought Noonan's paper was a manifesto with no specific proposals, as Abraham makes explicit in his commentary. I think it is pretty ironic that Noonan's paper criticizes formalists for not considering all the relevant data, when his paper doesn't contain a lick of data itself.

NMD: I don't agree. <smugly> I saw it as being the introductory chapter, outlining the main conceptual issues and specifying what it is that formalists routinely ignore, namely variation, diachrony, statistical patterning, and psycholinguistic processing.

AC: Do you really think that formalists don't believe in variation, change, or psycholinguistics? That seems like a straw man to me, or worse yet, a caricature of formalists. A number of papers in these books set us up with that unreasonable position (Payne, Durie, Axelrod, and Clamons et al.). None of us would deny the importance of such things. We simply believe that it is possible to characterize some parts of grammar independent of their usage; it's possible to talk about certain properties of grammar, without complicating things with considerations such as memory, etc. <NMD shifts

[2] In order not to interrupt the flow of this narrative, we refer to all papers in the volume by the last name of their authors. An appendix at the end of the review contains a complete list of the paper titles.

uncomfortably in her seat, clearly anxious to respond> Let me finish … I think that the study of diachrony, the study of processing, etc. are all important contributions to the study of language and grammar, and indeed they tell us many important things about the way grammar works. We simply believe that there is SOME grammar that can be talked about independently of usage, social context or performance. This is a point that Abraham makes very clear in his somewhat pessimistic discussion of the Noonan and Lasnik papers.

NMD: Of course I don't think that anyone CAN deny the importance of variation and change. And I agree with you that both sides of the fence consistently caricature each other here, as they have for the last forty years. Now, I am not sold on the idea that just because it is possible to characterize language purely in terms of its formal properties one should do it that way. It yields an incomplete theory. I realize that, like Heisenberg's uncertainty principle, one may characterize either the position or the speed of a particle, but not both. I still believe that our eventual goal should be to come up with a model that can account for all the information we have about particles. Formalists seem profoundly uninterested in this project.

AC: That's simply not true; it's simply the case that we try to address the smaller question of mental grammars first. Why is it that you functionalists always use metaphors to argue for your positions? George Lakoff would be proud. But here, I have a metaphor for you. Heidi[3] told me this one the other day: Think about the state of genetics in the 19th century. Gregor Mendel was able to characterize the formal properties of genetics without knowing anything about DNA or RNA, yet the work he did was an important contribution to our understanding of how genetics works. Further, Mendel never made the claim that his approach was a characterization of the chemical processes underlying it. Formalists should be doing the same thing, that is, modelling grammar without trying to talk about the grey matter that underlies it (although, I admit, we sometimes get carried away by our own analogies and talk as if we were). Turning the metaphor to the question of performance, imagine a situation where Mendel bred two tall pea plants together, and unexpectedly got a short plant. Then he realizes that this is because he forgot to water it. You wouldn't want to throw out all his research on inherited traits because of this, nor would you want to say that plant watering is an integral or even an explanatory part of genetic theory. Isn't that what functionalists are saying about language?

NMD: Cute, Andrew. Actually, I want to counter your comment with the idea that if a functionalist were looking at the short pea-plant case, we would never throw out the theory based on one single example – we just don't operate on single, armchair cases. Get out of your ivory tower, Andrew! One

[3] Heidi Harley (p.c.).

particular counterexample is not enough to overturn any hypothesis, and I don't understand the dogged refusal by formalists to collect statistically robust data (Hammond's and Miglio's papers in these volumes being exceptions to this). But, just so you know, many of the researchers working on statistical modelling of language phenomena also do not see themselves as making claims about mental processes. In fact, most variationists would tell you that it doesn't actually matter if we are modelling rules or constraints, just as long as we are able to accurately predict behaviour. Let me give you an argument from Pierrehumbert's review of Hayes and Bybee:

> [Even allowing for the radical] possibility that constraints cannot be imputed as such to the minds of speakers, [but rather] represent external scientific generalizations about the patterning that results from speech processing, … it still seems appropriate to speak of phonological 'constraints' in much the same way that one might view the laws of statistical thermodynamics as 'constraining' the physical universe. Whatever dynamics in the brain may indirectly result in phonological laws such as the laws of syllable structure, it is still the case that these pervasive laws constrain our models. (Pierrehumbert: 289)

AC: You already sound like a formalist. I'm not aiming for a neurological account either.

NMD: Maybe we are not so far apart after all. <shudders> Come to think of it, a neurologist would find many formalists and functionalists hopelessly misguided along the same lines.

AC: Too true. I guess what we mean when we fight about psychological reality would be laughable to a neuroscientist. But, I don't stay up at night worrying about it. <NMD and AC sigh, then look at each other and high-five>

NMD: You know, we have a lot more in common than we would ever admit, and there are beginning steps out there to unify our enterprises. The work in phonology, as you can see clearly from the contributions by Hammond, Hayes, Bybee, Pierrehumbert, and Nathan, is often both statistical and formal. In fact, the strong development of the relatively new area of laboratory phonology synthesizes these trends. This is possibly because phonology is inescapably grounded in phonetics.

AC: I agree entirely, the link between the phonetics and phonology is more concrete and tangible. Perhaps they should serve as a model for the rest of us. The worlds of morphology, syntax, and semantics are a little less clear. I'm not entirely sure what syntax should be grounded in. Parsing? Discourse? General cognitive principles? I know all of these have been posited.

NMD: Why not all of them?

AC: So, you are suggesting that the morphology and syntax modules might be epiphenomenal and really fall out of other properties of a single usage system?

NMD: Sure, let's go whole hog and ask why we need to posit multiple explanatory entities at all? The usage system is enough and we don't need innateness. You know, Occam's Razor and all that.

AC: OK. I have a BUNCH of things to say about that idea. Take Lasnik's position paper, which tries to address the question of external or internal explanation on empirical grounds. Considering functionalist and formalist accounts of locality restrictions, he compares a purely formal account of subjacency to a functional one based on parsing and memory considerations. Under the processing account, subjacency effects are not predicted in *wh*-in-situ languages, since there is no distance between the extracted element and the gap it resumes – they're in the same position. This isn't upheld by the facts of Chinese (Huang 1982). Lasnik points out the important distinction between functional considerations that may cause the evolution of a grammatical construction and the actual implementation of it in the mind. Functional pressures may well have given rise, evolutionarily, to the subjacency constraint. But the fact that it holds in languages where those functional pressures aren't present shows that it also has a formal implementation. Hayes takes a similar approach; he argues that functional pressures come to create optimality constraints, but the constraints themselves are independent implementations.

NMD: As you can imagine, I have some issues with the idea of innate formal structure. Hurford's is the only paper in these volumes that addresses the issue of what may actually be encoded in the genome. He evaluates some literature in the simulation-and-artificial-agents field, one study involving the critical period of language acquisition in simulated agents and the second a learning process by a neural network. Hurford found that in the population of artificial agents, it was advantageous to acquire language early, but retaining the ability to acquire language conferred no evolutionary advantage. In the second study, the neural network learned language better if it started training with short strings, and gradually moved on to longer ones. Hurford proposes incorporating these two explanations to arrive at an evolutionary model for the adaptive advantage of cognitive immaturity.

AC: I'm not so sure what the implications of this study are for formalist models of language acquisition. I wish that had been explained better.

NMD: Yes, that was lacking in the article considering its appearance in these volumes. The work done in the functionalist camp, including extensive work by connectionists, simply assumes that we don't operate or learn by so-called formal principles, but by generalizations over actual speech tokens. This necessarily entails that grammatical knowledge emerges from usage. In his paper, MacWhinney takes the emergent grammar approach that is well known in discourse circles a bit further by using neural networks and connectionist modelling. He claims that just as no rules are needed to control the shape of a bee honeycomb (it emerges from the most efficient way to pack bee-neighbours into the smallest space), so the structures of language that linguists observe as rules and syllables are emergent from more basic properties of neural functioning. MacWhinney's model of language emergence, extensively tested computationally through sample simulations and artificial agents, has several assumptions. The cognitively basic structures in which complex systems are grounded include feature maps, masking, argument frames and rehearsal loops. For MacWhinney it's only the relatively recent evolutionary overlay of functional neural circuits between areas such as the frontal attentional area and the temporal auditory area that has led to specifically human advances in the capacity for language use.

AC: <dismayed> Does this mean that he is saying that there is no language organ (which, by the way, we take to be only a metaphor for the SET of neurological/psychological constructs that are specialized for language)?

NMD: Actually, there is no human-specific language organ for MacWhinney; mammalian cognition reigns supreme. Which is fine by me, but why stop at mammals? One might argue that sophistication in cognitive systems lies along a continuum. I must say that, despite my agreement with his general strategy, I take issue with both Hurford and MacWhinney working on artificial agents and computer simulations of language acquisition and on that basis making specific claims about the evolution of language and the nature of networks in the brain itself. At least Hurford responsibly observes that the simulations of 'working memory' in neural networks are FAR removed from what psychologists call 'working memory' in their investigations with human subjects. With the MacWhinney article, I was left wishing that there had been more discussion of the potential difference between human and simulated agents' capacities, as well as more reference to MacWhinney's own work on brain function and evolutionary biology. Just as we were saying before, I think linguists get carried away with their own metaphors and take the phrase 'neural networks' and their simulations to be actual physical descriptions of how the brain works. But, overall, I am very pleased with MacWhinney's explanation of how all structural facts emerge from input. It seems plausible as a model.

AC: Sure, we acquire language-particular grammatical information from the input, but there are some cases, primarily involving the lack of negative evidence, that you simply can't explain that way. How does a Chinese speaker learn that she has subjacency constraints? There is nothing in the input for her to arrive at this generalization. Even Hurford's functionalist position acknowledges the importance of some innate component. Nina Hyams has an interesting observation in the conclusion to her contribution. She notes that while a MacWhinney-style model can account for overgeneralization and the well-known retreat from it, the competition model cannot account for the cases where overgeneralization in child language CONSISTENTLY fails to emerge – even where such a strategy would be probable on statistical grounds. She presents two cases. Children fail to overgeneralize *wanna*-contraction to subjects, and fail to overgeneralize the properties of finite clauses to non-finite ones, even though such properties are robust in the input. In models where you have both a formal grammatical module and usage-based modules, this follows directly: the absence of certain kinds of overgeneralization is due to the absolute restrictions placed on the syntax by universal grammar. In connectionist or statistical models there is no obvious explanation.

NMD: Actually, I'm glad you bring up Hyams. I found her article interesting because the modular account that she proposes seems quite functionalist in spirit. Hyams believes that the functional heads T(ense) and D(et) are pronominal in nature. And she follows Grodzinsky & Reinhart (1993) in arguing that pragmatic principles such as Rule I[4] do not operate in young children. I know you think Rule I is a formalist rule but as far as I can tell it interacts with discourse context and language performance.

AC: Sure, that's what I've been saying: the syntax, an independent module, interacts with the discourse. Notice that I'm not using the dreaded 'autonomous' word, which seems to be the source of a lot of functionalist confusion.

NMD: Anyway, Reinhart & Grodzinsky's Rule I states that all other things being equal, a pronoun is to be interpreted as anaphoric. The underdevelopment of Rule I is used by Hyams to explain why children exhibit optionality in their use of functional elements. Root infinitives, null subjects, and bare nouns in children are the result of this pragmatic insufficiency. Adults, on the other hand, exhibit direct discourse construal and contextual recoverability of these pronominal functional heads. I like this idea a lot: just as children need to acquire object permanence and other facts of contextual recoverability in the world, they also acquire the interpretation of pronouns. Bever (1970) has some similar arguments about the development of sentence interpretation, where children use the same kinds of strategies to interpret objects in the world and to interpret discourse. Note also that this is the same idea of cognitive immaturity that appeared in the Hurford paper. One thing that I thought was lacking in the whole volume was cross-referencing to similarities in arguments in the chapters. The reader is just left to make the connections herself.

AC: The interconnections between the articles were quite extensive, enough to make one wish that, for instance, Hurford had seen Hyams' article. This could have strengthened his argument a bit. But wait – I'm forgetting about something that I wanted to come back to about statistics but on a different track – I still take issue with your idea that any empirical hypothesis must be subjected to statistical scrutiny via the investigation of relevant independent variables. Let me give you an example. Imagine I started doing fieldwork on a language, and all I initially heard – perhaps because of a

[4] NP A cannot corefer with NP B if replacing A with C, C a variable bound by B, yields an indistinguishable interpretation.

usage-based politeness constraint against directly naming objects – were intransitive or passive sentences. I might, in my naiveté, hypothesize that this language lacked accusative case marking. Then suppose, after a period of time, I became initiated into the culture, was allowed to hear taboos, and heard a single instance or a few instances of an accusative case. There, my hypothesis is proved wrong, on a single piece of data. I don't need statistics to tell me this. A single token or a few tokens will do it, even when my ivory tower is out in the field.

NMD: Actually, a single token should not do it because it could be a speech error or, worse, from this example you might infer that accusative case marking was associated with taboo speech. That is why you need ethnographic methods and knowledge of performance backing up your statistical regularities. A long-term study would allow you to make sure that you were exposed to many contexts. By the way, <sweetly> you could sit in on my statistics class if you have time next semester.

AC: Oh joy! That's what I need before I come up for tenure. <grin> OK, granted that a single token may be an error or an exemplar of something other than accusative case. But I'm still not convinced that I need to run an ANOVA on the data to figure this out. I'm a trained linguist, I have experience with these things. More importantly, if I'm clever in my phrasing, I can simply ask my consultant if it is an instance of politeness, a mistake or really an instance of an accusative case. As a more concrete example, I don't need a statistical analysis to tell me that Irish usually uses VSO order in simple, discourse-neutral contexts.

NMD: OK, sure. That may be obvious to you as a speaker and trained observer of Irish with a lot of analytical experience. Let me ask you a question. When Irish deviates from simple VSO order, what is the order of importance of the factors associated with the deviation? Can you rank them? Can you predict not only the contexts in which the order changes but also the range of variation?

AC: The points you raise are empirical, and ones that I haven't investigated, but they aren't unsolvable. Let me construct a scenario for Irish. Imagine there are two factors that affect word order, let's say, phonological weight (which triggers rightward movement) and topicality (triggering, say, clefting or left dislocation). These two factors might plausibly compete in a situation where there was a topic that is also heavy. In order to investigate this, I'd need to tap the judgments of native speakers, giving them appropriate contexts, to see what they prefer. But I don't think this would answer the question you are asking. I'm talking about how these things are encoded grammatically, and you are talking about how they are used. Are we talking at cross-purposes here?

NMD: Oh probably! Perhaps the following question makes it clearer: does everyone in the community have the same rank order of factors affecting VSO? Actually, let me rephrase this even further, since I don't believe you can address my ranking question with your assumptions. Does everyone in your community encode grammar in the same way?

AC: Well, I'm not making any claims about the community of Irish speakers. I'm making a claim about the mental grammar of my consultant. Variation is important for us, but primarily as a window into the particular grammars of speakers. It isn't at all clear to me that asking questions about language use by a community precludes the study of language knowledge of each individual. Your question regarding the aggregate distributions in the community of speakers is a different issue entirely from what we are looking at.

NMD: Precisely my frustration. You run up and down claiming that what functionalists do is simply ask different questions, as though we were not all investigating language.

AC: Well, it's true that we're not asking the same questions. I think the point that Nettle makes about the formalist/functionalist debate in biology is crucial here: '[f]ormal theory can proceed in answering its own particular questions without initially making much reference to function' (Nettle: 459).

NMD: Ah, but you are forgetting what he says on the very next page: 'Just as functionalists must acknowledge the reality of structure, so formalists must acknowledge that the specification of structure is neither the only proper activity for linguists nor an ultimate end in itself' (Nettle: 460). But seriously, shouldn't functionalists and formalists engage in an attempt to unify the kinds of questions that are asked?

AC: Sure, but it doesn't mean that we can't split things up into discrete areas of investigation, and THEN study the interaction among them. The study of formal syntax in the 1970s and 1980s was done this way because we recognized the importance of studying the internal mechanism of, say, passives, before looking at how those internal mechanisms interact with external matters, like when they are used or even what they mean. This syntactico-centrism has changed in my relatively short academic life. A lot of work now goes into the syntax–semantics, syntax–pragmatics, sentence–processing interfaces. Today, things like topicality and focus are well-studied phenomena, even among us formalists, <smirk> you can tap the intuitions of speakers about the felicity of such sentences, and even look at discourse transcripts. There is nothing wrong with doing that.

NMD: Now I have a problem. Discourse transcripts are precisely examples of language in use. You contradict yourself.

AC: No, we use transcripts in a different way from functionalists. For functionalists they are often the limit and total extent of the data source. For many formalists today, they serve as a first pass, from which we can then generate tools for eliciting judgments from speakers. The work of Sperber and Wilson (notably Sperber & Wilson 1995) or Vallduví (notably Vallduví 1992) are wonderful examples of how pragmatic information can be integrated with formalist theorizing.

NMD: Actually, let me tell you about one of my favorite papers in the volume, Miglio's diachronic paper on the evolution of *ser* in Spanish. I loved it even though it was badly typeset. Miglio takes the rare step of analysing the same phenomenon from both the functionalist and formalist viewpoints, and showing that in both, the reanalysis of the Middle Spanish *ser*+past participle passive hinges on the ambiguity of the past participle as a verbal and adjectival form. The explanation then turns out to be different depending on the theory chosen, but descriptively adequate in either case. She uses historical texts, and helpfully points out that diachrony has been relatively neglected in formalist accounts precisely because the analyst cannot go back and question the native speakers. Formalist accounts assume that language change is catastrophic and fast, the result of parameter resetting and imperfect learning on the part of successive generations of speakers. But if you document the changes through statistical distributions, we are able to establish that language change is neither rapid nor catastrophic. In fact, many forms that are in the process of shifting remain at low levels of frequency, competing with homonymous alternations, and often relegated to specific discourse uses. The Spanish examples that Miglio documents took from the 12th to the 16th century to stabilize. Not exactly blazing along.

AC: <looking coyly across the table> Oh, those are just the laggards whose parameters didn't get reset. Or even worse, maybe some of those Luddites just refused to reset their parameters at all.

NMD: Anyway … Miglio employs the economy principle 'Procrastinate' (higher cost connected to overt movement) to argue that *ser*+past participle would be interpreted whenever possible as an adjective, and speakers would opt to interpret the subject in its base-generated position [Spec,IP]. I agree that both the formalist and the functionalist explanations are viable but still don't understand how formalists can explain that it would take so many centuries for the change to be carried through to completion.

AC: You know, the issue of diachrony is a thorny one for me. When I read functionalist papers which explicitly deny the existence of the diachronic/synchronic distinction (such as the Payne and the Axelrod contributions), I get really nervous. My first reading of this claim is that historical information is explicitly stored in the mind of the speaker. This is what I seemed to understand from Axelrod's description of the shift in meaning and function of classifier markers in Koyukon. This can't possibly be right, is it?

NMD: Hmm, let me see if we can find some clarification in Croft's article … let me find the quote. <rummages for book> It's on page 101: 'there is no a priori reason why a functional factor that historically motivated a particular construction at some point in the past is still operating in speaker's [sic] minds as they use their grammars today'. I agree with this but in many respects we can show that the gradual advance of historical processes in language is reflected in performance facts for particular items. One acquires these from the distributions of frequencies in the input. As Bybee would have it, 'every diachronic change contributes to a synchronic state' (page 216). Synchronic variation is thus not only the result of diachronic processes but of the social distribution of its forms – some prestigious, some stigmatized – and is actually the engine for future developments in the system.

AC: I think we may actually be talking about the same thing, then. The results of diachronic change are encoded in the child's mind, due to the temporal limits of the input. This is where formalists stop. Functionalists offer diachrony as an explanation for the shape of these changes. I think most formalists would be entirely comfortable with this, as long as it is understood that the mechanism in the child's mind does not directly encode information it can't have access to. This seems to relate to the issue of typology and possible languages. Kirby's contribution seemed to me to be the most straightforward in this regard. He argues that two processing pressures (accessibility and parallel function) that might explain accessibility in relative clauses do not equally have typological implications. Only accessibility actually seems to play a role in implicational universals. As such, he claims that both functional pressure and innate formal structure are needed to encode typological variation and the range of possible language types.

NMD: Well, that's similar to the general conclusion from Hoff-Ginsberg's paper. She carefully set up some studies that tested, from the perspective of child language acquisition, the relative merits (ability to account for data) of a purely innatist vs. a purely functionalist perspective, and she found that she had to reject this forced choice. There was an interesting disconnect between children's syntactic development and their conversational development, since the development of syntactic abilities proceeded somewhat independently of the development of communicative functions (leading to a rejection of pure functionalism). And, intriguingly, birth order was an important factor in determining whether children would be more advanced syntactically (firstborns are more advanced) or conversationally (later-borns win in this case).

AC: That's fascinating, but what about Purnell's Categorial Grammar/ acquisition paper? I thought it was a pretty resounding rejection of functionalist claims for language acquisition.

NMD: Purnell rejects the functionalist argument that holophrastic phrases in children are unanalysable from their structure because they rely on arbitrary adult expansion. So, for instance, the utterance of a child going into an empty room and saying 'ball' can be expanded endlessly by analysts into 'where is my ball?', 'the ball is gone', etc. Functionalists would argue that the single word is not the surface representation of an underlying structure, but that intonation and gesture help the child to achieve the communicative intent. Purnell, on the other hand, argues that the intonation that the child places on the utterance renders it analysable, at least in terms of topic and comment. That's well and good for me, and although I wish she had used more data, I am ready to accept her innovative claim that holophrastic phrases should be thoroughly studied, taking into account possible structural cues in intonation. However, this still does not convince me that the intonational categories were innate to begin with, but only that they may be acquired prior to the ability to combine words productively.

AC: Maybe we'll have to agree to disagree on some of the thornier basic assumptions. Do you think there is any chance that, despite this, the two camps will ever be able to work together? Or will we be able to pass each other in the street without throwing coffee at each other?

NMD: Well, you and I are far too fabulous to lower ourselves to petty bickering, of course. <grin> But I was pleased with the way some of the papers in the book tried to integrate formalist and functionalist thinking. I've already mentioned Miglio's interesting paper. Croft's contribution is also very conciliatory: although he doesn't propose a unification of the views, he points out that important empirical results from both camps are to be found in the literature.

AC: Yeah, his paper makes an interesting contrast to Anderson's, which seems to dismiss the entire functionalist enterprise as a waste of time. He's too radical even for me.

NMD: See, I knew you'd see the light …

AC: I'm still not a convert, so don't go counting me in just yet with your little ANOVAs or whatever. Newmeyer's paper, which is really a short summary of his 1998 book, also explains the ways in which the two approaches are not necessarily incompatible, if we can get past the animosity. Nettle's article takes a similar tone: 'The more substantive point of disagreement between functional and formal approaches seems to stem from an assumption that the two approaches are in conflict. Thus, to give a functional explanation for a phenomenon is assumed to entail a denial that that phenomenon is underlain by the kind of specialised cognitive machinery the formalists posit. This need not be so, as a glance at biological practice reveals' (Nettle: 458).

NMD: There were other papers that I thought brought the two sides together too. Liu brings a new perspective to the literature on the Chinese *ba* constructions. She presents an aspectual analysis of *ba*, where either the object NP is specific or the predicate denotes a bounded event, properties related by homomorphism. She compares this to a functionalist transitivity account and shows that the two are entirely compatible.

AC: Kaiser's paper takes a similar tack: she formally encodes the discourse properties of the Japanese post-verbal construction using Vallduví's information structure. I like the contrast between this paper and Ariel's paper on discourse markers in Hebrew, which notes that there are both 'linguistic' and 'extralinguistic' (which I take to mean 'syntactic' and 'extrasyntactic') factors contributing to the distribution and interpretation of Hebrew *harey*. Similarly, Wilbur analyses brow raises and observes that both functional correlations and formal descriptions are appropriate.

NMD: Maggie Tallerman's paper is interesting in this regard. She looks at the phenomenon of initial consonant mutation in Welsh and makes the observation that soft mutation is triggered in 'non-canonical' orderings. This is grammatically encoded. But she notes that the grammatical construction may very well have a functional motivation: this special marking allows the parser to recognize the heads of phrases when they are not in their expected initial position. Functional motivation is not the grammar, it simply explains why the grammar might have evolved the way it does. Tallerman doesn't deny the importance of functional considerations, she simply separates them from their formal implementation. What did you think of Hengeveld's comparison of Dik-style functionalist operators and minimalist functional categories?

AC: I think he is absolutely right that the similarities in the two approaches converge on a very similar result. He ends with a criticism of minimalism for encoding (semantically vacuous) agreement with the same tools it uses for encoding temporal and discourse operators (i.e. functional categories). In this, he seems to have presaged Chomsky's recent motivation for the elimination of AgrPs from the grammar. Are there any papers that you thought unified approaches to phonology?

NMD: Hammond's paper addressed the functionalist question of the list-rule fallacy from a formalist (optimality theoretic) perspective, arguing that we can eliminate one construct in favor of the other.

AC: Correct me if I'm wrong, but I think he didn't implement the correction to the fallacy the same way functionalists do. Didn't he try to derive lists from rules (in the form of constraints) and get rid of parts of the lexicon? Isn't that the reverse of most of the Construction and Cognitive Grammar literature? I was worried about the fact that Hammond still needed some lexicon, in the form of a list of form–meaning pairs.

NMD: Well, I wouldn't call this the reverse, but many strands of functionalism, especially cognitive grammarians such as Langacker (1987), seek to do away with both rules and lists, and incorporate the notion of schemas, which reduce organizational exigencies to semantic structures, phonological structures, and symbolic links between the two. Kibre's paper also addresses the list–rule fallacy to explore issues in the regular and irregular morphophonology of Turkish. Kibre tentatively proposes a new formal model grounded in hierarchical levels of representation, familiar to us from generative models, while assuming basically a connectionist theory of subsymbolic processing. He implements some formalist ideas in a functionalist framework. Makes for a nice crossover.

AC: The topic of Optimality Theory as a crossover theory was brought up by Hayes, Nathan, and Nakamura. One case stood out for me, though, negatively: Nakamura's system of encoding ergative splits seemed overly complex to me and the author didn't cite some of the significant formal works on the topic, such as the work of Eloise Jelinek (e.g. Jelinek 1993). I was pretty disappointed by this paper.

NMD: I was puzzled by the paper by Clamons, Mulkern, Sanders & Stenson. Their contention was that Oromo agreement phenomena, which appear to be discourse-sensitive, couldn't be dealt with in a formal grammar. It seemed to me that they were attacking formalism as if syntax were truly autonomous in a radical sense, but from what I've heard from you, there appears to be no reason that the syntax/morphology couldn't interact with discourse.

AC: I felt the same thing was true of Comrie & Polinsky's paper on the (lack of) marking on head nouns in Tsez relative clauses. They conclude that the pragmatics plays a big role in interpretation here. I think that is certainly correct, but it doesn't mean that there isn't a syntax.

NMD: Sure, as long as you concede that pragmatics is part of the grammar.

AC: Anyway … speaking as a formalist, I normally take grammar to refer to syntax, morphology, and phonology. But in a broader sense, if you mean: do these areas interact with pragmatics and other parts of usage, then yes. In fact, I thought that Meinunger's paper was a very interesting contribution and was potentially applicable to Clamons et al.'s data.

NMD: You mean how he applies a Diesing-style mapping approach to agreement phenomena triggered by topics (cf. Diesing 1992)? Topicality, which is grammatically encoded by case and agreement, is mapped to the pragmatics interface by an algorithm.

AC: As you know, these volumes are supposed to be the proceedings of a conference on formalism and functionalism. The organizers/editors chose word order and ergative case marking as test issues. Unfortunately, many of the conference speakers (Pesetsky, Marantz, and Dubois) didn't submit their contributions. The gaps in the contributions make the test-issue part of the book seem very disjointed. Davison's summary of ergativity is interesting and tries to draw together the similarities of two approaches, but lacks the context of the original papers. Hale's paper is an interesting contrast here. He rebuts Payne's idea that 'all truth, of whatever sort, must ultimately cohere' (Payne: 144), using Navajo binding to show that parts of the grammar can work against each other. I was particularly annoyed by Payne's paper, which – as we've discussed before – attacks a straw man, whereby syntactic explanations are the only acceptable explanations for syntactic phenomena. That might have been true in the 1960s, but it isn't any more.

NMD: I heard a rumour that the latest revelation from Chomsky is all about this.

AC: It's true, Chomsky's become a functionalist! In a recent paper (Chomsky 2001), he claims that the 'grammatical' (syntactic) component is relatively pure, and maximally simple too. All the action is outside syntax. All the language variation should follow from external factors, including – quite radically – natural laws of the universe.

NMD: <rolls her eyes, and glances at her watch> Oh no, it's almost time for the Anthropology Department faculty meeting.

AC: Yup, I have to go teach at 1:00. You know, although we clearly still disagree about a bunch of things – such as how language-specific a lot of the machinery we use is and the exact nature of our genetic endowment for language – I think we are closer on a lot of issues than I originally thought.

NMD: Yeah, it seems like a lot of the 'debate' about formalism and functionalism revolves around people characterizing each other's positions in extreme terms, setting up unrealistic characterizations that couldn't possibly be true.

AC: You mean, like the way I characterized the diachronic claims of functionalists in terms of children actually knowing the specifics of historical change the way linguists do (instead of just as the results), or the way you characterized formalists in terms of an absolutely autonomous syntax?

NMD: Yeah, exactly. After reading Hyams' article, I see that the parameters idea is meant primarily as an analogy for actual learning heuristics.

AC: I don't think that any but the most radical formalists really believe parameters are switches. I at least think of them as innately specified limitations on search space that the child uses for determining properties of the grammar. But let's not get back into the question of innateness.

NMD: Yeah, we'd never get out of here! You are certainly not as crazy as I thought you were.

AC: <bowing graciously> And you aren't nearly as fuzzy as I thought you were. <NMD bats AC affectionately on the arm> Gosh, can you imagine what the tenured senior faculty in our respective departments would think if they knew that we actually AGREED on some stuff? You probably wouldn't be invited to the cool functionalist cocktail parties anymore …

NMD: And you'll have your MIT degree revoked for heresy! <both break into howls of laughter>

AC: We'd better swear NEVER to tell anyone we've spoken about this!

<NMD and AC then beat a hasty exit for fear that they might have been seen cavorting with the enemy by someone from their own departments>

APPENDIX

## List of articles

Abraham, W., Discussant paper referring to the 'Syntax position papers' by Howard Lasnik & Mickey Noonan; vol. 1, 55–86.

Anderson, S. R., A formalist's reading of some functionalist work in syntax; vol. 1, 111–135.

Nathan, G. S., What functionalists can learn from formalists in phonology; vol. 1, 305–327.

Nettle, D., Functionalism and its difficulties in biology and linguistics; vol. 1, 445–467.

Newmeyer, F. J., Some remarks on the functionalist–formalist controversy in linguistics; vol. 1, 469–486.

Noonan, M., Non-structuralist syntax; vol. 1, 11–31.

Payne, D., What counts as explanation? A functionalist approach to word order; vol. 1, 137–165.

Pierrehumbert, J., Formalizing functionalism; vol. 1, 287–304.

Purnell, E., The holophrastic hypothesis revisited: structural and functional approaches; vol. 2, 365–382.

Tallerman, M., Welsh soft mutation and marked word order; vol. 2, 277–294.

Wilbur, R., A functional journey with a formal ending: what do brow raises do in American Sign Language? vol. 2, 295–314.

## REFERENCES

Bever, T. G. (1970). The cognitive basis for linguistic structures. In Hayes, J. R. (ed.), *Cognition and language development*. New York: Wiley & Sons. 279–263.

Chomsky, N. (2001). Beyond explanatory adequacy. Ms., MIT.

Diesing, M. (1992). *Indefinites*. Cambridge, MA: MIT Press.

Grodzinsky, Y. & Reinhart, T. (1993). The innateness of binding and coreference. *Linguistic Inquiry* **24**. 69–102.

Huang, C.-T. J. (1982). Logical relations in Chinese and the theory of grammar. Ph.D. dissertation, MIT.

Jelinek, E. (1993). Ergative splits and argument type. *MIT Working Papers* **18**. 15–42.

Langacker, R. (1987). *Foundations of Cognitive Grammar*, vol. 1: *Theoretical prerequisites*. Stanford, CA: Stanford University Press.

Newmeyer, F. J. (1998). *Language form and language function*. Cambridge, MA: MIT Press.

Sperber, D. & Wilson, D. (1995). *Relevance: communication and cognition* (2nd edn.). Oxford: Blackwell.

Vallduví, E. (1992). *The informational component*. New York: Garland.

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