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Introducing Special Issues in *Biolinguistics*

Kleanthes K. Grohmann & Cedric Boeckx

It is our pleasure to introduce a new 'feature' in *Biolinguistics*: the special issue. Other than the big inaugural issue (*Biolinguistics* 1, as representative of the entire year's volume, published in December 2007), it is the set goal of this journal to appear four times per year, roughly as the Winter issue 1 (at the latest at the end of March), the Spring issue 2 (by the end of June), the Summer issue 3 (by the end of September), and the Fall issue 4 (by the end of December). While we will make all possible efforts to bring these issues out just a little earlier than our own set 'deadline', the present issue should welcome you right on the mark: *Biolinguistics* 3.2–3 stands for the Spring/Summer double issue, and thus falls on the right edge of the set dates!

The present double issue is a themed, guest-edited special issue, so we will spell out a few general, and then some more specific, points on this new feature.

This double issue is a carefully selected sub-set of presentations given at a conference, and this is certainly one possibility for *Biolinguistics* special issues. As such, we appeal to all interested conference organizers to bear this in mind and get in touch with us for future collaboration. However, we will not publish more than one such special issue per volume, which means that in the future, we will be even more selective. Other potential special issues could be themed independently of relevant conferences, ideally through a call for papers. Topics should be in line with the general aims of the journal (see also Boeckx & Grohmann 2007) and might range from biolinguistic explanations in linguistic theory to evolution of/in language to genetic aspects of language, and many more. Again, we urge potential guest editors to contact us well in advance.

Whatever the background of a special issue, we will impose the usual selection criteria of *Biolinguistics*. This means that all submitted manuscripts will be peer-reviewed by (at least) two external referees. Our experience assembling the present special issue leads us to announce from the outset that future guest editors will also be asked to get a little bit more involved in the selection and editing process. We will communicate our policy on special issues to interested colleagues on request, of course, which includes setting a strict timeline in order for forthcoming issues to be planned ahead accordingly.

To introduce the present issue, we are extremely happy that the organizers of *BALE 2008*, the conference held at the University of York in July 2008 with the full title *Biolinguistics, Acquisition and Language Evolution* (at the time of writing, still available under http://www.york.ac.uk/conferences/bale2008/index.html), identified *Biolinguistics* as the right kind of platform to share the results of this stimulating event with the larger community. When we accepted our participation,

thereby introducing the first set of ground rules for *Biolinguistics* special issues, we made clear to the (then) York co-organizer, Nanna Haug Hilton, who became the special issue's guest editor (see her Guest Editorial on the next two pages) that we do expect high-quality submissions. In other words, even though a themed special issue may originate in a conference, this is no excuse to publish anything for the sake of publishing. We thus distance ourselves from the term 'conference proceedings' — at least, without the modifier 'highly selective'. In this case (and we see no reason for not doing so in the future), all manuscripts were sent to two anonymous reviewers with full disclosure of the purpose (i.e. a special issue from a conference) — and thus authorship (after all, the conference information is public, so it would be easy for any potential reviewer to extract this kind of information anyway). As a result, several submissions had to be rejected and we are pleased to see the current selection in print.

If readers still take issue with (parts of) this selection, we would like to encourage everyone, as with anything else published in *Biolinguistics*, to submit commentary and criticism in the form of Forum contributions, if not full-fledged articles.

With all of this out of the way, please enjoy this *Biolinguistics* special issue, generate ideas for future (special) issues, and check regularly the journal website at http://www.biolinguistics.eu for news and updates!

References

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Guest Editorial: Introduction to *BALE 2008*

Nanna Haug Hilton

The papers selected to appear in this volume of Biolinguistics were first presented at a conference held at the University of York in July 2008: *Biolinguistics, Acquisition and Language Evolution (BALE 2008)*. *BALE 2008* was a small but successful meeting that happened as a joint effort of the post-graduate research students in the Department of Language and Linguistic Science at the University of York.

BALE 2008 came about in the autumn of 2007, when we, 14 post-graduate students, were granted money from the university to organize a linguistics conference. The research interests and backgrounds of us doctoral students on the committee varied across a number of linguistic sub-disciplines. It soon became apparent, therefore, that finding a topic for the conference could prove problematic. After some debate, we concluded that instead of addressing a subject that a few of the students specialized in, the conference theme should be one that unites different linguistic disciplines. The topic that emerged deals with a question that, in our opinion, is at the core of all linguistic research: *What are the biological underpinnings of language, and what is the interaction between the innate knowledge of linguistic structure with the language input to which we are exposed?*

Thus, a main goal of *BALE 2008* was to be an interdisciplinary meeting, something we think is reflected by the diversity of papers selected for this issue. For the conference, we welcomed research papers that would lead to a wider understanding of the unique language ability of human beings. This meant that we accepted papers that were primarily based in linguistic research but that also tied its findings to other fields like anthropology, evolutionary biology, psychology, genetics, and computational modeling. This way, we hoped to present a conference dealing not only with the nature of our genetic endowment for language, but also the acquisition of language, the neurological and biological underpinnings of language, and the cultural and historical perspectives on evolution of language. We are very grateful to our excellent plenary speakers at *BALE 2008* who showcased this multidisciplinarity in their papers: Koji Fujita, Jim Hurford, Simon Kirby, and Juan Uriagereka.

The conference would not have come about without the enthusiastic help from the Department of Language and Linguistic Science at the University of York, and our thanks are directed, in particular, to Marilyn Vihman and George Tsoulas, who helped make *BALE 2008* such a success. Neither would *BALE 2008* have originated without the financial support from the University of York, which we are grateful for. We would also like to thank the anonymous reviewers for the initial paper selection for the conference, and Kleanthes Grohmann and Cedric

Boeckx for their editorial efforts in publishing the selection of papers here in this special issue of *Biolinguistics*. Finally, our heartfelt thanks go out to the presenters and attendees of *BALE 2008* for making those three days in July 2008 such a memorable conference for us.

On behalf of the other committee members for *BALE 2008*: Suzanne Bardeas, Maryam Gholizadeh, Phil Harrison, Alan Hsieh, Marianna Kaimaki, Koji Kawahara, Kaj Nyman, Jillian Oddie, Daniel Redinger, Rein Ove Sikveland, James Strang, Norman Yeo, and Xinfeng Zhang.

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A Prospect for Evolutionary Adequacy: Merge and the Evolution and Development of Human Language

Koji Fujita

Biolinguistic minimalism seeks a deeper explanation of the design, development and evolution of human language by reducing its core domain to the bare minimum including the set-formation operation Merge. In an attempt to open an avenue of research that may lead to an evolutionarily adequate theory of language, this article makes the following proposals: (i) Merge is the elementary combinatorial device that requires no more decomposition; (ii) the precursor to Merge may be found in the uniquely human capacity for hierarchical object manipulation; (iii) the uniqueness of the human lexicon may also be captured in terms of Merge. Empirical validations of these proposals should constitute one major topic for the biolinguistic program.

Keywords: action grammar; anti-lexicalism; evolutionary adequacy; FLN/ FLB; unbounded Merge

1. The Logical Problem of Language Evolution

Language is a biological/mental organ of extreme perfection and complication. Following the standard practice in biology, we can set up three distinct but interconnected levels of investigation for this uniquely human organ:

(1)   a.   Design
       b.   Development
       c.   Evolution

In classical generative grammar, the theoretical goals of descriptive adequacy and explanatory adequacy were neatly distinguished for the studies of language design and language development, respectively, while the topic of language evolution remained somewhat afield. Current biolinguistic program goes beyond this tradition and elevates language evolution as a central issue, for which we may define a new, higher theoretical goal of 'evolutionary adequacy'.[1]

Let us say that a theory of UG is evolutionarily adequate if it explains how it was possible for the human faculty of language (HFL) to emerge during our evolutionary history. With the advent of the minimalist program (MP), it is now well understood that UG is not so much the *explanans* for the logical problem of language acquisition (LPLA), as it is itself the *explanandum* in the context of evolutionary linguistics. The proclaimed species-specificity of UG, in tandem with the observed dysfunctional and maladaptive nature of its highly modularized principles as envisioned back in the GB era of the early 1980s, kept the topic of its origin and evolution as a kind of Pandora's box, a very delicate and almost intangible issue within the framework of the Neo-Darwinian orthodoxy and the Modern Synthesis. Without any trace of its possible precursors found in the whole biological world, and without strong evidence for its reproductive fitness, the emergence of HFL seemed to be the most unlikely biological event on earth.

How, then, was HFL able to come into being at all? This is the core essence of the new problem we have to face squarely in biolinguistic investigations, which may be dubbed the 'logical problem of language evolution (LPLE)' (Fujita 2007, 2009; see also Christiansen & Chater 2008, where the same term is used for a different purpose) or 'Darwin's problem' (Fujita 2002, Boeckx 2009, Hornstein 2009).[2] The LPLE stands as the clear indication that adaptation by natural (or sexual) selection cannot be the whole explanation of the evolution of HFL, and/or that UG need not be such a complex cognitive system comprising many domain-specific and non-adaptive grammatical principles interacting intricately. In short, the LPLE tempts us to consider a drastic reorganization of UG and biological evolution in general.

In such a context, the MP offers a very promising research strategy for the biological study of language evolution (the biolinguistic minimalism). In an important sense, the MP is an attempt to reduce the internal mechanism of UG/HFL to the bare minimum by shifting the focus of inquiry to domain-general physico-mathematical principles and constraints working on the evolution and

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[1]   Besides Fujita (2007, 2009), the term 'evolutionary adequacy' has already appeared in Longobardi (2004: 103), where it is proposed that explanatory adequacy and evolutionary adequacy are adequacy levels, respectively corresponding to the questions "What are the biologically possible human languages?" and "Why do we have precisely these biologically possible languages?". The usage of the term there is largely the same as in this article, though Longobardi seems to be more concerned with an explanation of historical/cultural variation of language by a parameter theory. I thank a reviewer for calling my attention to Longobardi's work.

[2]   The term 'Darwin's problem' may be something of a misnomer, however. This is because Darwin's major concerns were the apparently limitless biological variations that can be found in nature and their explanation by means of natural selection, whereas we are more interested in the uniformity of the biological organ in question, that is, HFL/UG, and how it may be accounted for in terms of natural/physical laws. Thus the problem may be more appropriately dubbed '(D'Arcy) Thompson's problem', for example.

development of any complex system in the natural world. To the extent that apparently language-specific properties can be derived from those 'third factors' (Chomsky 2005a), the genetically determined component of UG becomes smaller, and this has the effect of rendering the topic of language evolution more accessible. In its departure from classic genetic determinism and its emphasis on epigenetic processes under structural constraints through which immense phenotypic diversity will arise, the general idea behind the MP is in perfect harmony with the *evo–devo* paradigm in biology (see, among many others, Arthur 1997 and Hall & Olson 2003), which fact forces us to seriously reconsider topics such as modularity, autonomy, domain-specificity, evolvability, and the relation between evolution and development within the generative framework.

The idea that developmental processes, rather than genetic information *per se*, is responsible for the observed language variations was already obvious in the formative years of the Principles-and-Parameters approach, the basic tenet of which it is that a slight change in a parametric value will bring about tremendous cross-linguistic differences that are apparently limitless but only within a tightly restricted range. Adopting the *evo-devo* perspective for language evolution and language development suggests that we can proceed much further and take the universality of human language also as a phenotype, not directly encoded in the human genome as such. If this should prove to be the case, then ultimately there will be nothing left to be ascribed to UG — the final stage of the minimalist inquiries but for the time being too remote a goal even to speak of.

The term LPLE is in part intended to point to the parallel nature of the problems surrounding language evolution and language acquisition. The latter issue has been traditionally associated with the 'poverty of the stimulus' (PoS) argument, the observation that what is circumstantially available to the learner (primary linguistic data, PLD) alone is not sufficient to make language acquisition possible at all. Likewise, given the qualitative difference (insurmountable gap) between HFL and non-human primate and non-primate communication systems, language evolution seems to present a kind of 'poverty of the precursors' (PoP) argument, that is, what our common ancestors had already had (pre-existing capacities) before the formation of HFL was not sufficient to allow its emergence only in the human lineage. For the LPLA, UG has been assumed to bridge the gap between PLD and the attained steady state of FL (I-language). For the LPLE, UG is obviously of no help since it is the end product, not a pre-existing condition, of language evolution.[3]

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[3]   A reviewer suggests that something like 'poverty of selective pressures' (PoSP) would be a better evolutionary counterpart to PoS, stressing that it is selective pressures as a 'species-external' factor that are on a par with PLD as an 'individual-external' factor. While I fully appreciate the merit of the reviewer's alternative, which makes every sense especially in light of Yang's (2002) variational model of language acquisition that sees linguistic input from the environment as a selectional pressure on competing grammars in the learner's brain, my contention here is that PoP presents a qualitatively different, more general problem than PoSP: While a pluralist solution (which claims that natural selection is just one among many driving forces of evolution) is readily available for PoSP even within Darwinian theorizing, such is not the case with PoP, which calls for a serious reconsideration of an adaptationist program.

The same reviewer also questions the compatibility of the PoP argument with the exaptive scenario of language evolution adopted in this article. Let us just note that while

Human language is essentially a system for connecting meaning and sound (including the alternative externalization by means of signing and, sporadically, writing) via syntactic structure. The phonetic and semantic interpretation of a linguistic expression (sentence) is to some extent determined by its syntactic computation and the resulting hierarchical phrase structure. Structure dependency in this general sense sharply distinguishes human language from animal communication. Structure building in the syntax module is at the same time sending instructions to the external systems of sensory-motor (SM) and conceptual–intentional (C–I) capacities. These three systems constitute equally autonomous components of HFL.

That human language has this kind of modular architecture may be a good indication that language first came into being through a process known as exaptation. It is natural to assume that the evolution of these three systems predated the emergence of language, that they had, if any, separate original functions whose connection with language was thin, and that language suddenly appeared as a result of their integration within the human brain, dated about 50,000-200,000 years ago, when the pre-existing SM and C–I capacities were mediated by the so far isolated computational system of syntax.

The view that the emergence of human language was a sudden and unexpected event is often misunderstood and criticized by opponents who believe that evolution is always gradual. As a matter of fact, such an instantaneous model of language evolution, quite like the also controversial generativist instantaneous model of language acquisition, is a form of abstraction and idealization, one that purports to make otherwise too complex an issue less so for the purpose of investigation. Chomsky (2004: 395) states: "Plainly, the faculty of language was not instantaneously inserted into a mind/brain with the rest of its architecture fully intact. But we are now asking how well it is designed on that counterfactual assumption. How much does the abstraction distort a vastly more complex reality?" Not very much, perhaps.

The now famous distinction between HFL in the narrow sense (FLN) and in the broad sense (FLB), advocated by Hauser *et al.* (2002) and defended by Fitch *et al.* (2005), is of immense import when we seek to attain evolutionary adequacy by first identifying what more is necessary, other than the pre-existing capacities, for human language to come into existence. By definition, FLN is that part of HFL which is unique to the humans and human language, and FLB includes all other components which are more or less shared among other species or among other human cognitive faculties. We can assume that the core of the LPLE lies in the origins and evolution of FLN and its integration into the rest of FLB.

What constitutes FLN, then? Hauser *et al.* suggest: (i) the recursive computational operation of human syntax which gives rise to the property of discrete infinity, and (ii) the two interface systems connecting syntax to the C–I- and SM-faculties, respectively. In minimalist theorizing, the former has been recognized as the combinatorial operation of unbound Merge, the sole structure-building device of HFL. Following the criticism by Pinker & Jackendoff (2005) and

language as a system of distinct subcomponents does not seem to have any precursors, those subcomponents, taken in isolation, may well be exaptations of pre-existing capacities.

Jackendoff & Pinker (2005), we may tentatively add the lexical system as yet another component of FLN, for the obvious reason that in its productivity and profligacy the human lexicon is unique to the species.

(2)   *Ingredients of FLN (tentative)*
    a.   Recursion (unbounded Merge)
    b.   Interfaces (C–I and SM)
    c.   Lexicon

"To create is to recombine," said the French biologist François Jacob, whose insight applies to the process of the evolution of language, too. HFL is an evolutionary novelty resulting from a recombination of FLN and FLB, the origins and evolution of the former being unclear for the time being.

Importantly, if the three components of FLN listed in (2) are phylogenetically unrelated capacities and their evolutionary origins have to be sought independently of each other, the LPLE has to remain as hard as before. If, on the other hand, it is shown that they can be traced back to some common precursor, or alternatively that one of them serves as the precursor to the other two, then the LPLE becomes easier to approach. Biolinguistic minimalism suggests that this kind of reductive thinking is a possibility we should not discard immediately.

As a first step toward this reduction, I will argue in the remainder of this article that (2a) may be (part of) what made (2b) and (2c) possible at all, and that (2a) may find its precursor in the uniquely human capacity for hierarchical object manipulation, the Subassembly strategy of Greenfield's (1991, 1998) grammar of action (Action Grammar). For that purpose, I will first clarify the true nature of the elementary syntactic operation Merge (section 2), next point out the striking formal resemblance between Merge and Action Grammar (section 3), and then suggest that Merge is crucially responsible for the formation of the C–I-interface and the lexicon (section 4).

As is often the case with theoretical studies of language evolution, much of the following arguments have to remain speculative. It is worth noting, however, that descriptive linguistic research sometimes sheds light on the core property of HFL and consequently on its origins and evolution. Evolutionary adequacy on one hand and explanatory and descriptive adequacy on the other are not two separate goals but are tightly interconnected and should be pursued in parallel.

2.   The True Nature of Merge

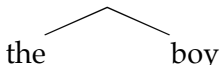In its simplest form, Merge is a set-formation operation that takes two objects and combines them into an (unordered) set.

(3)   Merge $(\alpha, \beta) = \{\alpha, \beta\}$

Merge is then a binary and symmetric operation. For heuristic purposes alone, it may be safely assumed that the recursive, unbounded application of Merge is the only generative device of HFL. To the extent that this assumption is tenable, we

can begin our discussion of language evolution by focusing on the possible evolutionary scenario(s) of Merge. In this context, it seems fair to ask whether Merge *per se* is the most elementary operation or whether it is a complex operation that can be further decomposed into more fundamental operations. This is because, if the latter supposition turns out true, the target of the evolutionary explanation is not Merge but rather those fundamentals, Merge being a later innovation (biological or cultural) resulting from their (re)combination.

Decomposition of Merge into smaller units is a rather natural minimalist move, and such attempts are not new in the literature, including works by Fukui (2006, to appear), Boeckx (2009), and Hornstein (2009). These authors share the view that labeling, which gives rise to endocentricity, one prominent property of human language, and which therefore seems to be an indispensable component of Merge, should be conceived of as a distinct syntactic operation detachable from the core part of Merge (Merge in the narrow sense, say core Merge). Fukui calls the labeling operation 'Embed', while Boeckx gives it the different name of 'Copy'. Hornstein also proposes to derive the effect of Merge from the combination of concatenation (our core Merge) and labeling. Rather surprisingly, however, their proposals in fact serve to confirm that no additional operation other than (core) Merge is necessary to label a set formed by a prior application of Merge, as I will now show. For expository purposes here I use Fukui's Merge+ Embed system for discussion.[4]
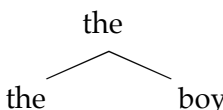
As a simple exemplar, suppose we build *the boy* by Merging *the* and *boy*. In accordance with Fukui's framework, we first combine the two lexical items into an unordered set by means of core Merge.

(4) a. Merge (the, boy) = {the, boy}

b.

       the   boy

At this point, there is as yet no label for this new object, and its structure lacks endocentricity. Fukui argues that the Merge operation in (4a) has the effect of defining the Base Set (BS) in (5), to which further syntactic operations may apply.

(5) BS = {the, boy}

It is to this BS that Embed now applies, to form a labeled structure, as in (6).

(6) a. Embed (the, {the, boy}) = the $\cup$ {the, boy} = {the, {the, boy}}

b.   the

    the   boy

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4   To be more precise, Fukui's approach differs from both Boeckx's and Hornstein's, in that instead of decomposing Merge into smaller operations, it adds Embed as a distinct new operation available for syntactic computation while Merge is kept intact. I thank Naoki Fukui (p.c.) and a reviewer for reminding me of this distinction.

In general terms, Embed takes BS and one of its members, combines them, and forms a set union of the two. In other words, it is an operation that embeds a given object in a larger set that contains it as a member. In (6), BS = {the, boy} is embedded in the larger set {the, {the, boy}}, in which *the* acts as the label of the resulting phrase structure, now yielding endocentricity (and symmetry is broken, so to speak). Recursion is equally relevant to Merge and Embed. Both operations may or may not apply recursively, and the type of recursive structure we usually associate with natural language results from a successive application of Merge followed by Embed.

For Fukui, the distinction between Merge and Embed is crucially relevant for evolutionary studies, too, because he assumes that while Merge is probably not specific to human language, it is Embed that is truly unique to it. If so, the key to solving the LPLE may be further narrowed down to the origin of Embed and its combination with the independently existing Merge. While I fully appreciate the merit of such reasoning, my suspicion is that there is a way of making a better sense of Fukui's proposal. That is, contrary to his conclusion, I take it reasonable to think of Embed as nothing more than another sub-class of Merge (on a par with Move as Internal Merge).

Let us begin by noting the striking formal parallelism between Merge, Move and Embed:

(7)   Given $\beta = \{\beta 1, \beta 2\}$,

   a.   Merge: $(\alpha, \beta) \Rightarrow \{\alpha, \beta\}$, where α is external to β.
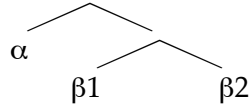
   b.   Move: $(\alpha, \beta) \Rightarrow \{\alpha, \beta\}$, where α is internal to a member of β, here $\beta 2$.
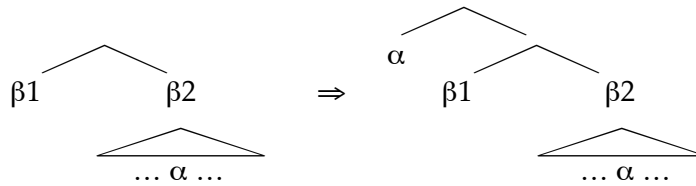
   c.   Embed: $(\alpha, \beta) \Rightarrow \{\alpha, \beta\}$, where α is a member of β, here $\beta 2$.
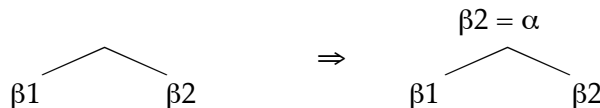
These three computational devices share the property of being a binary set-forming operation, the sole difference lying in where α is chosen from. In particular, for Move, it is to be found inside $\beta 1$ or $\beta 2$, while for Embed, it is $\beta 1$ or $\beta 2$. Thus Embed can be understood as a strictly localized version of Move, in the sense that its search domain is minimized. This way of reformulating Embed as a kind of Move (and therefore of Merge) is in harmony with the independently

suggested similarity between chains (product of Move) and projections (product of Embed) (Uriagereka 1998 and Boeckx 2008).[5]

We are therefore inclined to suspect that just as Move naturally follows from Merge, so does Embed, too. In short, Embed as well as Move comes for free once we have Merge, and consequently there is no need to speculate on their distinct evolutionary origins in addition to that of Merge.

There remains one obvious discrepancy between Merge and Embed, of course, and that is whether the operations are symmetric or not. By definition, Merge has been understood to be symmetric, given {A, B} = {B, A}, whereas labeling by Embed is obviously asymmetric, given {A, {A, B}} ≠ {B, {A, B}}. This discrepancy becomes only apparent when we take into consideration the trigger of Merge in each application and not just the resultant unordered set. In (6), for example, it is the selectional feature (or the edge feature) of *the* that gets *boy* Merged to it: In familiar terms, *boy* is the complement of the head *the* and not vice versa. We can safely assimilate this asymmetric relation to the attractor–attractee relation (Chomsky 1995) or the more recent probe-goal relation (Chomsky 2001) involved in the application of Move. And it is precisely on the basis of the choice of the attractor/probe in the preceding Merge operation that the subsequent Embed operation correctly picks out the label, the one object in which to embed the other.

To exemplify, assume we have arrived at the derivational point where the structure (8a) is formed by Merge, where *v*P contains DP (as a subject or an object):

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[5]   Incidentally, one might wonder whether there can be something like 'non-local Embed', which will have the effect of choosing the label of a phrase from inside its immediate constituents. Such an operation is formally indistinguishable from Move in the present proposal, and the resulting structure will be exocentric in nature. One relevant case that comes to my mind immediately is the oft-discussed internally headed relative clauses (IHRCs). Here is an example from Japanese:

(i)   [Taro-ga   ronbun-wo toukou-sita]   no-ga   kyakka-s-are-ta.
      Taro-NOM   paper-ACC   submission-did   NMNL.NOM   rejection-do-PASS-PAST
      'The paper which Taro submitted was rejected.'

In (i), the intended head noun *ronbun* 'paper' occupies the canonical object position inside the relative clause while the clause itself functions as the matrix subject DP, as if headed by *ronbun*. This phenomenon will receive a simple explanation if non-local Embed is at work here, which picks up *ronbun* as the label of the relative clause. Provided that non-local Embed is non-distinct from Move, this proposal may be taken as a simple reformulation of the movement-based analysis of IHRCs without actual constituent movement. Because of the complex nature of the potential problems such a new analysis will necessarily face, I refrain from pursuing it any further here.

   Let us also note that endocentricity does not seem to be an essential property of linguistic structure in general. Morphological root compounding has been known for its exocentricity. In Japanese, for example, both *takai* 'high' and *hikui* 'low' are adjectives, but when combined together in the form of *takai-hikui*, this compound behaves as a noun.

(ii)   Tatemono-no takai-hikui-ga   juuyoo-da.
       building-GEN   high-low-NOM   important-is
       'The height of the buildings matters.'

(8)   a.   {T, *v*P}   ⇒
       b.   {T, {T, *v*P}}   ⇒
       c.   {DP, {T, {T, *v*P}}}   ⇒
       d.   {T, {DP, {T, {T, *v*P}}}}

To the extent that the Merge operation in (8a) is triggered by the selectional feature on the part of T, the next step is necessarily to Embed (8a) in T, yielding (8b). Subsequent application of Move to the *v*P-internal DP, again triggered by the EPP feature of T, will form (8c), and because of this, the next step is again to Embed (8c) in T, as in (8d). Note that the order of (8b) and (8c) cannot be reversed to incorrectly build #{T, {DP, {T, *v*P}}}, because minimal search always prefers Embed to Move where applicable. The Merge–to–Embed–to–Move–to–Embed sequence depicted in (8a-d) is itself a nice illustration of Merge applying recursively, given that both Embed and Move are subtypes of Merge. In short, labeling by Embed, and the resulting endocentricity of linguistic structure, are natural consequences of recursive Merge. It can now safely be concluded that Merge needs no further decomposing, and it is the most elementary computational operation of HFL. As a consequence, the origin and evolution of Merge remains at the core of the LFLE.

3.   From Action Grammar to Merge

For evolutionary studies of human language, reducing the human syntax to the most elementary operation Merge has the great advantage of making it possible to compare Merge with other human and non-human capacities that bear formal resemblance to it in search of the precursor(s) to Merge and the human syntax. This kind of comparative studies were certainly out of the question in earlier days of generative grammar when people spoke of phrase structure rules and X-bar theory to explain the hierarchical phrase structure of human language together with its endocentric nature. No one would ever dream of discovering a homologue or an analogue of X-bar schemata in the non-linguistic behaviors of non-human primates, for example. Biolinguistic minimalism has brought the viability of such a comparative method to the attention of interested researchers for the first time in the long history of generative grammar, reminding them that they can (and must) approach the issue of language evolution in the same way as evolutionary biologists explore biological evolution in general.

So what other capacities may be comparable to Merge? There have already been a lot of proposals in the literature, ranging from navigation and foraging to music and songs, gestures, manual dexterity, and social intelligence (including theory of mind (ToM), reciprocal altruism, Machiavellian intelligence etc.).[6] Unfortunately, our current (lack of) knowledge does not allow us to tell which of these proposals has more or less plausibility than others, and it appears that each of them has its own problems. Take the idea of ToM as a precursor to recursion,

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[6]   See Corballis (2002) for some discussion on the possible link between ToM and linguistic recursiveness.

for example. Although the view that some kind of mind reading is involved in syntactic recursion may sound convincing in light of the iterated complementation as in *I know that you know that I know that…*, the correlation must remain rather illusionary, largely (i) because there are instances of syntactic recursion that have nothing to do with mind reading (*Theory A proves that Theory B proves that… Theory Z is wrong*), and (ii) because mind reading can be expressed linguistically without clausal complementation, as in *He likes her idea.*

Such being the case, instead of examining these alternatives any further I will here add just one more conceivable (in my view highly plausible) candidate, by referring to and bringing to the fore the now classic developmental studies by cognitive psychologist Patricia Greenfield.

Greenfield (1991, 1998) builds on her earlier work (Greenfield *et al.* 1972) and argues that young children's developing skills in hierarchically organized object manipulation, as typically exhibited in cup nesting skills and tool use (such as using a spoon), precede their language development and serve as a pre-adaptation for it, and furthermore that a similar situation may hold true of the evolution of language in the species. In connecting the ontogeny and phylogeny of human language via their common precursor, her pioneering studies were precedent to current *evo–devo* approach to language evolution.

Greenfield observes that there are three distinct developmental stages in children's 'Action Grammar', from the simplest Pairing strategy via the Pot strategy to the most complex Subassembly strategy. It is extremely interesting to note that these strategies neatly correspond to the development of linguistic structure, in particular to the different modes of the application of Merge. In terms of nesting cups, these three combinatorial methods can be represented as follows (Greenfield *et al.* 1972 and Greenfield 1991; see also Maynard Smith & Szathmáry 1995):

Strategy 1: Pairing Method

(A)   (B)   (B (A))
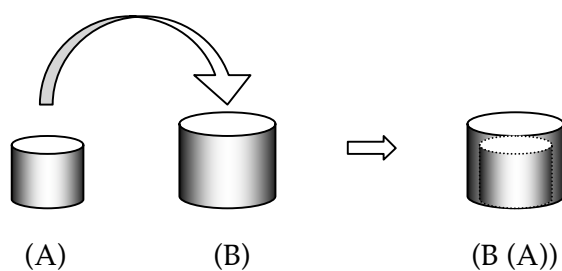
Strategy 2: Pot Method

(A)   (B)   (C)   (A)   (C (B))   (C (B (A)))
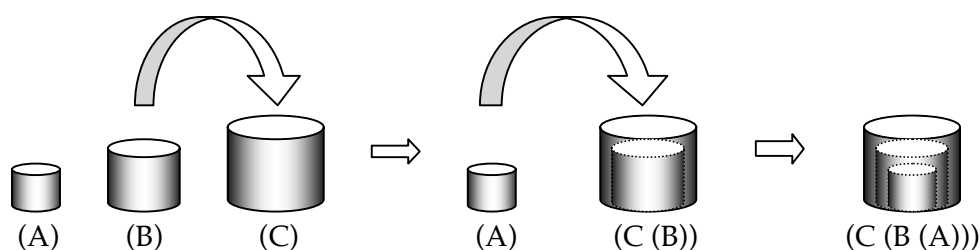
Strategy 3: Subassembly Method

Figure 1: Three Stages of Action Grammar

In the Pairing method, we just combine the two cups into one object by putting the smaller cup (A) into the larger one (B). In the Pot method, this same procedure applies twice (or more), combining three (or more) cups into one object, first by putting the middle-sized cup (B) into the largest (C), then by putting the smallest (A) into (C), which now contains (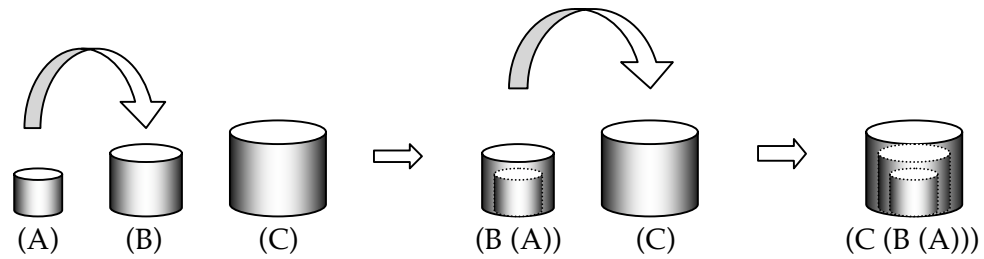B), too. The third strategy is the crucial one. This Subassembly method may at first appear not very different from the second strategy, but in fact their gap is immense. In this case, we first put (A) into (B), and then we take the complex object consisting of (A) and (B) as a subunit for further operation, putting this subassembly into (C). Anticipating the comparison of Action Grammar to Merge below, it may be noted here that the Pot method requires just one constant attractor, whereas the Subassembly method has to switch attractors at each step.

Studies in comparative cognitive ethology inform us that the Subassembly strategy is almost uniquely human (the only exception seems to be chimpanzees trained linguistically in captivity), while the Pairing and Pot strategies are shared among other primates and non-primates equally. For example, Tokimoto & Okanoya (2004) demonstrate that even Degus (*Octodon degu*) have the capacity for hierarchically organizing objects by using the Pot strategy. Thus it seems natural to suspect that the uniquely human Subassembly strategy in Action Grammar plays some important role in the formation of human language, in particular of syntax.

Greenfield's (1991) contention was that Action Grammar corresponds to phoneme combination in word formation, but in response to criticism by other researchers (including Tomasello 1991 and Swan 1998), Greenfield (1998) agrees to return to the initial insight of Greenfield *et al.* (1972) and admits that the proper object of combination is not the phoneme but the word. This makes it possible to project the three strategies of Action Grammar directly onto different modes of application for Merge.

For example, consider building the VP structures (9a) and (9b) (here the *v*P projection is omitted for simplicity).

(9)   a.   John saw Mary.   b.   The boy saw Mary.

(10) a.   Merge (saw, Mary) = {saw, Mary}
              Merge (John, {saw, Mary}) = {John, {saw, Mary}}

      b.   Merge (saw, Mary) = {saw, Mary}
              Merge (the, boy) = {the, boy}
              Merge ({the, boy}, {saw, Mary}) = {{the, boy}, {saw, Mary}}

In both derivations, Merging (saw, Mary) into {saw, Mary} is analogous to the simple object combination by the Pairing method. Here *Mary* is attracted by *saw*, as if the smaller cup goes into the larger cup. Note that Action Grammar and Merge share the property of being symmetric in principle but asymmetric in practice. That *saw* contains the selectional feature that attracts *Mary*, and not vice versa, determines that *saw* but not *Mary* counts as 'the larger cup'.[7]

The next step crucially differentiates the two derivations. In (9a), with *saw* attracting *John*, Merge applies to (John, {saw, Mary}) to yield {John, {saw, Mary}}, as in (10a). Here *saw* remains the constant attractor, and the operation now counts as an instance of the Pot strategy. In (9b), however, something different must take place, as depicted in (10b). In order for the subject DP *the boy* to be properly attracted by *saw*, this DP must first be constructed by an independent application of Merge: *the* functions as the attractor, triggering Merge to form {the, boy}. In other words, this DP acts as a subassembly in the whole derivation. Obviously, the derivation of (9b) corresponds to the Subassembly strategy.[8]

Throughout the history of generative grammar, certain nodes or phrases have been known to block extraction from within, and have been subject to different forms of formulation under the rubrics of islands, barriers, phases, and so on. The notion of a phase is particularly interesting in this connection, as it functions as a subassembly unit in the derivational process. The Phase Impenetrability Condition (PIC, Chomsky 2001), whatever its precise definition may be, is very presumably a reflection of the fact that a derivational subunit, once completed, cannot be probed into by later operations. On the face of it, PIC is a highly language-specific principle that appears to defy a deeper explanation,

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[7]   Di Sciullo & Isac (2008) argue that the asymmetry of Merge can be captured in terms of the proper inclusion relation that holds between the relevant feature bundles of the two objects undergoing Merge. Without going into the details of their analysis, we can say that their intuition is fully compatible with the present observation. Here *saw* is 'larger' or 'heavier' than *Mary* because of the selectional feature carried only by the former.

[8]   A reviewer objects that the analogy drawn here between Subassembly-type Action Grammar and Merge is quite arbitrary and that it would be equally valid to assume that the Pot strategy more closely reflects the full human syntax, by reversing the attractor–attractee (or Probe–Goal, in his or her words) relation. More specifically, this reviewer asks why the largest cup has to be chosen as the Probe and not as the Goal. A simple answer would be to point to the fact that in Move (Internal Merge), the Probe is (part of) the category being moved into (like the larger cup) and the Goal (part of) the category being moved (the smaller cup), rather than vice versa. To claim that the moved cup acts as the Probe in Action Grammar would require further justification beyond this simple formal correspondence.

      This same reviewer also questions the validity of nesting cups as an analogue of syntax on grounds that the former always gives rise to a total inclusion/dominance relation, whereas syntactic structure exhibits such a relation only sporadically. This type of objection is based on the failure to notice that Action Grammar is observed in a large variety of object combining behaviors, not restricted to cup nesting actions. For example, consider the nut-hammer relation in nut cracking, which can hardly be assimilated to an inclusion relation.

but when seen this way, it may turn out that PIC can be given a natural place in the evolution of HFL.

That the distinction made here between Subassembly-type Merge (henceforth, Sub-Merge) and Pot-type Merge (Pot-Merge) reflects some important aspect of syntax is supported by an important observation made by Roeper & Snyder (2005) with respect to the cross-linguistic variation in root compounding patterns. In English, a compound like *child book club* is structurally and semantically ambiguous: Both the right-branching structure (11a) and the left-branching structure (11b) are permitted.

(11)   a.   b.

      child   club

          book   club   child   book

In Swedish, however, the corresponding compound *barn bok klub* is not ambiguous and only the right-branching structure (12a) is possible.

(12)   a.   b. *

      barn   klub

          bok   klub   barn   bok
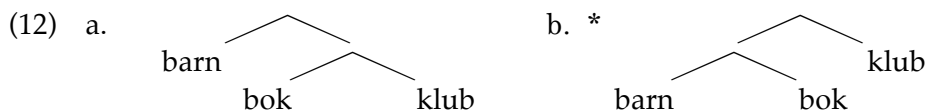
Roeper & Snyder (2005) offer their own elaborate account of this discrepancy between the two languages, but here let us just note that only the left-branching structure requires Sub-Merge to apply. (11a) and (12a) need only one attractor, but (11b) and (12b) need two.[9]

The fact that there is at least one language that utilizes Pot-Merge but not Sub-Merge for compounding, can be understood as an indication that the latter type is computationally more complex, probably echoing the species-specificity of the Subassembly strategy in Action Grammar: After all, it is the last strategy to emerge in child development. Importantly, that Swedish bans Sub-Merge in compounding does not mean at all that the language lacks it altogether: Otherwise, even a simple sentence like (9b) would be excluded. All we can infer from above is (i) that Sub-Merge is a universal option of the human syntax, and (ii) that each language may have a different range of its actual application. While I admit that when stated this way, the universality of Sub-Merge becomes virtually indisputable, this may be the right way to look at things first before we jump at the opposite conclusion.

I emphasize this point because the same consideration is highly relevant in assessing the controversial Pirahã data. Everett (2005, 2007) has famously demonstrated that this Amazonian language lacks clausal complementation, relativization and other hallmarks of embedded structure, and suggested that the language is without recursion in general. The phenomena he brings to our attention are each very interesting in their own right, and I totally agree with him that cultural factors have considerable influence on the grammar of a language.

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[9]   If modifiers are also attracted by a head, then selectional features cannot be the only relevant attracting force.

But that may be where we should stop for the moment. I see no deep conflict between his Pirahã data and the generativist claim that recursion or embedding is an innate and universal property of human language. In any case, for something to be part of UG does not require that it be observed in every particular language, extant or extinct.

The above illustrations may have given the reader the (wrong) impression that Sub-Merge is something very special, a trick to be resorted to under very limited conditions. As a matter of fact, instances of Sub-Merge can be found in every bit of linguistic expressions and it is indeed what makes HFL worthy of the name. Consider, for example, the derivation of *Mary saw the boy*. In this case after the object DP is first built in (13b), Sub-Merge applies and attracts this whole subunit to *saw* as in (13c). (13d) is a case of Pot-Merge.

(13)   a.   Mary saw the boy.
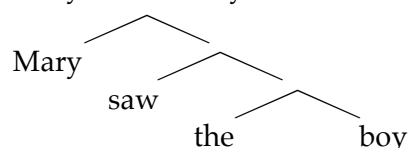
 b.   Merge (the, boy) = {the, boy}
 c.   Merge (saw, {the, boy}) = {saw, {the, boy}}
 d.   Merge (Mary, {saw, {the, boy}}) = {Mary, {saw, {the, boy}}}

In general, every head–complement merger must take place in the form of Sub-Merge, unless of course the complement is also a zero-level lexical item. Sub-Merge thus seems to be at the core of phrasal syntax, as a default option. This last point may be relevant in searching for the reason why its application is sometimes restricted in root compounding, as in the Swedish data quoted above. For example, it can be assumed that in root compounding, to the extent that it belongs to the domain of non-phrasal syntax, Sub-Merge counts as extraneous, and that some grammars prefer to deter its application if it is only for the purpose of compound formation.

The discussion so far has amply demonstrated the formal parallelism between the elementary syntactic operation and manual object manipulation. This alone, of course, is not evidence for the evolutionary and developmental link between grammar and action, nor does it show that Action Grammar is the precursor to Merge, a possibility that needs to be explored in a multidisciplinary endeavor by researchers from every relevant field of cognitive sciences. The point I would like to make is that, by reducing the surface complexity of the human syntax to its bare minimum in the form of Merge and its recursive application, generative grammar now takes a prominent role in such an enterprise, a situation that was not easy to envision before the advent of biolinguistic minimalism.[10]

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[10]   To claim that Merge is evolutionarily linked to Action Grammar or any other cognitive capacity that is not species- or domain-specific does not entail that Merge is not part of FLN, contrary to what a reviewer seems to believe. Merge as the definitive syntactic operation of human language is both species- and domain-specific, but its origin may still be found in some pre-existing domain-general faculty. The alternative possibility that there is nothing in FLN remains, of course, which is well worth pursuing as an ultimate minimalist hypothesis.

Before we proceed, let us note that the crucial property of Merge, its unboundedness, has been left untouched so far. Apparently, Action Grammar applies in a bounded manner, and even if it turns out to be the precursor to Merge, how the shift from boundedness to unboundedness took place calls for an independent explanation. In this respect, Chomsky's (2007b: 23) following comment must be carefully considered. He states: "[F]or both evolution and development, there seems to be little reason to suppose that there were precursors to unbounded Merge." In his view, Merge is unbounded from the beginning. It is to everyone's knowledge that young children go through the developing stages of one word and two word utterances before they exhibit the full expressive power of unbounded Merge. This is primarily due to their limitation in language-independent cognitive and physical capacities, and does not argue against the innateness of unbounded Merge.

In evolution, however, it seems more natural to suppose a transitional process from bounded to unbounded Merge, a transition made possible by various factors including the enhancement of working memory in the enlarged brain. If Action Grammar is linked to the evolution of Merge, then it is likely to be the precursor to bounded Merge, so it seems. Note, however, that the distinction between bounded Merge and unbounded Merge is largely for theoretical purposes. In actual practice, Merge is of course bounded for familiar reasons: Life is short, and no one would ever produce a sentence that could only be generated by applying Merge 2^{100} times! But if Merge is unbounded only in theory, the same can be said of Action Grammar, too. With an infinite number of cups and infinite time (and strength) to do the nesting, the Pot and Subassembly strategies could be repeated endlessly in theory. The distance from Action Grammar to unbounded Merge may not be too remote.

4.   Anti-Lexicalism and Evolutionary Adequacy

With so much discussion on Merge in mind, let us turn now to the other two components of FLN listed in (2): The two interfaces and the lexical system. Given that Merge is an indispensable ingredient of FLN, one possible research direction along the minimalist guideline is to ask to what extent these additional systems can be related to, or even reduced to, this elementary combinatorial operation. With ample abstraction, it can be taken for granted that any system that creates something must be equipped with a Merge-like device: Recall Jacob's remark that creation is recombination. In this section, I will suggest that at least some fragments of the C–I-interface can be trivialized if we take seriously the prospect of *anti-lexicalism*, which may at the same time allow us to largely dismiss the problem concerning the evolution of the human lexicon.

By anti-lexicalism, I mean the following general picture:

In fact, Fitch *et al.* (2005: 203) state: "If future empirical progress demonstrates that FLN represents an empty set, so be it."

(14)   *Basic claims of anti-lexicalism*
   a.   Words are generated by syntax.
   b.   The lexicon can be decomposed into FLN (Merge) and FLB (sound and meaning).
   c.   Consequently, there is virtually no lexicon.[11]

There can be many different implementations of anti-lexicalist theorizing, the Distributed Morphology (DM) framework (Halle & Maranz 1993 and Marantz 1997, among others) and the studies on 'l(exical)-syntax' (Hale & Keyser 1993, 1998 and many other works) being two representatives. Other important works include Baker (2003), van Hout & Roeper (1998), and Roeper & van Hout (1999). Here I will not commit myself to any particular theory but only keep to the general idea that words and sentences are equally outputs of syntactic computation; in other words, syntax (recursive Merge) is the sole generative engine of HFL and the uniquely human generative lexicon is part of syntax.

In my view, anti-lexicalism can offer a profound account of language evolution, because obviously one cannot assume the existence of a rich lexicon from the start. In evolutionary contexts, words were not what had already been given to humans, but they had to be created through a process of synthesis and analysis, which is essentially what syntax does. This point was clear to Bronowski (1977: 120), for example, when he discussed human and animal languages and stated: "It cannot be true literally that 'In the beginning was the word': On the contrary, in the beginning was the sentence."

Take a simple case of forming a nominal from a verb, say *destruction* from *destroy*. Following the practice of DM for concreteness, once *destroy* is decomposed into the verbalizing element (v) and the category-neutral root ($\sqrt{}$), replacing v with the nominalizing element (n) in the combination $v+\sqrt{}$ will give rise to the nominal form $n+\sqrt{}$, to be realized as *destruction*. This sort of extraction and recombination must have underlain the formation of a rudimentary vocabulary from segmentation of distinct alarm calls (to be assimilated to unstructured sentences) used by some species of birds and monkeys for different types of predators. It seems reasonable not to posit the whole lexicon as a totally distinct component belonging to FLN, but rather to decompose it into the generative component and the list of surface morpho-phonological forms associated with conceptual–semantic properties, with the former falling under the proper domain of recursive Merge.

This kind of unitary approach to words and sentences by means of basic syntactic computation may find its roots in the traditional idea of lexical decomposition dating back to Generative Semantics in the early 1960s, now partially

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[11]   That is, as an independent module of grammar responsible for word formation. Needless to say, there has to be a universal pool of features in the human brain, different combinations of which will ultimately yield a different set of lexical items or words (sound-meaning pairings) available in particular I-languages. These are a residue of the lexicon that may safely be assumed to be part of FLB. Hauser *et al.* (2002: 1576) discuss some "key aspects of words" that may be "distinctively human" — including the astonishing "scale and mode of acquisition" by children, and the absence of "straightforward word-thing relationship." Whether these uniquely human properties must be stated as such as part of FLN or they may be better explained by other equally unique capacities, is another matter, of course.

reincarnated by the split VP structure generally assumed in minimalist syntax. As a representative case, compare the two versions of VP structure proposed for double object verbs like *give* and *show*:

(15) a.   Mary gave John a book.

   b.   *v*P   c.   VP

        Mary   gave   John   a book

             v
                John
                      V   a book
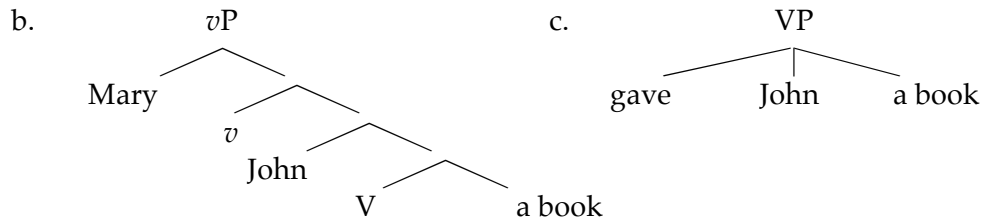
(15b) is the familiar split VP format, while the flat structure in (15c) is adopted, in particular, in the Simpler Syntax framework (Culicover & Jackendoff 2005). The structure (15c) is said to be simpler than (15b) because, as Culicover & Jackendoff (2006) explicitly define it, the structural complexity is determined on the basis of sub-constituents and invisible structure. The problem is that they assume a non-derivational, representational model of phrase structure, which radically differs from our strictly derivational model, and therefore that any straightforward evaluation in terms of simplicity between (15b) and (15c) is in fact impossible. The claim here is that, seen from the derivational viewpoint, it is certainly (15b) that is simpler, since its derivation involves only binary Merge, whereas (15c) would require a more complex operation of tertiary Merge (or quaternary Merge, to derive *gave John a book in the park*, for example). The purported simplicity of (15c) is valid only on highly theory-internal grounds.

Our major concern here is, which of them is more helpful in mapping syntax to the C–I-system, that is, which makes the topic of C–I-interface 'simpler'. Recall that the split VP structure like (15b) is an embodiment of the general conception of lexical decomposition, one important interpretation of which being that the fundamental part of conceptual/semantic structure is directly encoded in syntax. Compare the putative conceptual structure (16) with (15b).

(16)   [Mary CAUSE [John HAVE a book]]

The abstract causative function CAUSE corresponds to the small verb *v* in (15b), and HAVE to the large verb V. The actual word *gave* will be the morpho-phonological realization of the *v*–V amalgam (plus the past tense value) formed by syntactic Merge (head movement) and/or morphological merger. This virtually isomorphic relation between syntactic structure and (core) conceptual structure I take to be the essential foundation on which the C–I-interface is established.[12]

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[12]   We may want to structurally encode the distinction between Agent subject and non-agentive Causer subject, the necessity of which is shown by oft-cited examples like *The exam gave John a headache*. In terms of conceptual structure, the distinction can be made by using another function DO as an abstract agentive verb, while reserving CAUSE for the non-agentive interpretation, as in:

   (i)   [x DO [x CAUSE [y HAVE z]]]

To a certain degree, it can be said that syntactic structure building by recursive Merge is at the same time a parallel hierarchical conceptual structure formation by Merging semantic atoms successively (say, conceptual Merge). This proposal, by no means, is intended to suggest that syntactic structure and semantic structure are the same, as was once claimed falsely by Generative Semantics. On the contrary, full semantic interpretation requires much more information than syntactic structure provides (in particular where the compositionality principle fails to capture the vastly multifaceted and flexible syntax-semantics relations), and syntax and semantics remain two autonomous modules as before.

Seeing the C–I-interface in the suggested way allows us, however, to reconsider the origin and evolution of the connection between these two modules in a more gradual manner than is often hinted at within the generative camp. Instead of taking them as derivatives of unrelated origins, we may speculate that they come from a single root and their mutual autonomy is a matter of later cladistics, or even that either one of them is an exaptation of the other, co-opted for new functions. In short, it is advisable to take into consideration Darwin's concept of 'descent with modification' in discussing the evolutionary scenario of the C–I-interface, and in making this point clear to us, biolinguistic minimalism is successful in bringing theoretical linguistic research into the broader context of evolutionary biology.

Turning back to the comparison of (15b) and (15c), it may be noted that while the layered structure (15b) is optimized for the C–I-interface, the flat structure in (15c) is optimized for the SM-interface instead. That is, (15c) looks closer to the surface linear sequence and is able to make the linearization task trivial. In an important sense, then, this structure can be assimilated to the Surface Structure representation of classic generative grammar, and (15b) to the Deep Structure representation of core thematic relations. It seems to me that this contrast is tightly connected to the supposed primary (evolutionarily older) function of language. The flat structure is more adapted for external functions in-cluding communication, while the layered structure is fitter for internal functions (thought, planning, etc.).

Both inside and outside linguistics, researchers fall into two groups: Those who take the original function of language to be that of communication and those who take it to be that of thought. By arguing for the layered structure, I am adopting the view here that it was thought and not communication that was initially facilitated by the emergence of language: That was enough to make those who happened to obtain this capacity reproductively more successful, and communicative functions along with many others were later accommodated

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In a series of earlier works (Fujita 1996 and references cited therein), I have proposed a three-layered VP structure roughly of the form (ii), to provide a structure-based account of various syntactic and semantic peculiarities of double object and dative object verbs, middle and ergative verbs, and psychological verbs.

(ii)   [Agent V1 [Causer V2 [V3 Theme …]]]

Here the mapping between (i) and (ii) becomes more straightforward, rendering the C–I-interface even more accessible accordingly.

through exaptation. A simple thought experiment clearly shows that such must have been the actual situation: Supposing that you were the first individual in the population to obtain language, perhaps by some point mutation, how could you put this new faculty to communicative use when there was no one else who shared it with you?

In short, if something like anti-lexicalism is on the right track and actual words are generated by syntax, we can minimize the proper domain of FLN by saying (i) that both the C–I-interface and the lexicon are subserved by Merge, and therefore (ii) that only recursive, unbounded Merge constitutes the genuine part of FLN. The list in (2) may safely be updated as in (17).[13]

(17)   *Ingredients of FLN (less tentative)*
     a.   recursion (unbounded Merge)
     b.   nothing else

I have said nothing so far about the status of the SM-interface from the perspective of anti-lexicalism. To the extent that linearization is a matter of deriving order from unordered hierarchical structure (for example, by mapping the derivationally determined c-command relations onto linear ordering; cf. Epstein *et al.*'s 1998 reformulation of Kayne's 1994 Linear Correspondence Axiom), the SM-interface is also crucially dependent on the recursive application of Merge. Also, derivational and inflectional morphology reflects the hierarchical relations of relevant heads to a considerable extent. Whether the same can be said about other aspects of morpho-phonological interpretation remains largely unclear, but pursuing such a possibility will be one major issue in the generative studies of language evolution. My conviction is that anti-lexicalism offers a rich avenue of research towards an evolutionarily adequate theory of HFL.

Note incidentally that the success or failure of anti-lexicalism is not directly related to the controversial issue of whether proto-language was analytic (holophrastic) or synthetic. According to the former (Wray 2000, Arbib 2003), proto-language started with sentence-like holistic units which were later analyzed and segmented into what looked more like our modern words. The latter view (e.g., Bickerton 2003, 2007, Tallerman 2007) holds that proto-language initially had only individual words which were only to be combined in a meaningful way thanks to the later emergence of syntax.

Although anti-lexicalism may at first seem to be in harmony with the holophrastic view (since it asserts that words cannot exist in the absence of syntax), it is intended to capture the richness and productivity of the lexicon of full human language which should be qualitatively different from that of proto-language. The word-like elements of proto-language (proto-words) may have existed as a primitive conceptual unit before the advent of Merge, but it certainly

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[13]   A tacit claim made here is that Merge *per se* is not part of FLN but only its recursive nature is. Likewise, Agree can be seen as belonging to FLB, since it is a form of general pattern recognition as can easily be found in animal cognition or even in molecular biological phenomena like immune reaction. A reviewer suggests that Agree may also be an instance of internal Merge (applying to values of features), theoretical consequences of which deserve a careful examination.

took Merge to elevate them to the full-fledged modern words with their internal composition. What anti-lexicalism suggests is that the great leap from the proto-lexicon to the full human lexicon, so to speak, could not have occurred in the absence of the capacities for synthesis and analysis afforded by syntax.

The above scenario of lexical evolution may receive support from the observation on the parallel development of lexico-syntactic knowledge in young children. Tomasello (1992) famously proposed the Verb Island Hypothesis (VIH), according to which during the first two years of life children use verbs in an item-based manner, without any general knowledge of argument structure or categorization. Where in adult grammar two verbs belong to the same subclass and behave syntactically in the same way, for example as transitive verbs that take Agent and Theme, children treat them as two distinct, unrelated entities, each with its own organization and appearing in different frames. Abstract generalizations concerning categories, schemas and thematic roles emerge in children at later stages of development only gradually, according to Tomasello.

The VIH has subsequently been critically reexamined by other researchers and effectively rebutted, for example, by Ninio (2006), who concludes that "no verb is an island" (p. 59). Ninio instead proposes a lexicalist analysis of children's early syntactic knowledge. According to this latter view, at the earliest stage item-specific lexical rules regulating the syntactic combination patterns of each verb are sufficient to allow for the later development of full syntactic knowledge.

Both Tomasello's and Ninio's empiricist positions seem to be incompatible with the observation that young children begin to learn verbs by fully utilizing lexical decomposition very early. Viau (2006), for example, points out on the basis of data from CHIDLES a tight correlation between their acquisition of double object and dative object verbs on one hand, and that of the atomic elements like HAVE, GO, and CAUSE that these verbs can be decomposed into.

(18)   a.   DP1 gives DP2 DP3
       b.   [DP1 CAUSE [DP2 HAVE DP3]]
       c.   DP1 gives DP3 to DP2
       d.   [DP1 CAUSE [DP3 GO-TO DP2]]

Omitting the details, the mean ages of their acquisition can be shown sequentially as in (19), adapted from Viau (2006):

(19)   CAUSE (2;0.4) ≥ HAVE (2;0.7) ≥ Double Obj Verbs (2;1.6) >
       GO (2;4.0) ≥ Dative Obj verbs (2;4.9)

It is obvious from this result that children acquire double object verbs immediately after they acquire CAUSE and HAVE, while the acquisition of dative object verbs occurs significantly later, only after the acquisition of GO. My interpretation of this interesting fact is as follows: (i) By the time children reach the stage of two word utterances, they have fully activated recursive Merge, and (ii) this same capacity enables them to construct new verbs (including the well-documented overgeneration such as *Daddy giggle me*) by Merging basic conceptual units they already have. Tomasello's VIH, if it is correct at all, is appli-

cable only to the earliest stage of development before Merge comes into force, and the insular behaviors of the limited verbs at that stage, without systematic organization, is analogous to the supposed property of proto-verbs (primitive verb-like elements of proto-language).

Although we know that Ernst Haeckel's theory literally does not hold biologically and that ontogeny does NOT recapitulate phylogeny, I think one practical application of the *evo–devo* approach to language evolution is to adopt the working hypothesis that language development in the individual proceeds more or less analogously to language evolution in the species. Anti-lexicalism proposes to treat the full human lexicon, but not the primitive proto-lexicon, in terms of syntactic computation by recursive Merge. Where those island-like atomic units, to which Merge applies to form words, first came from is another issue, and they very likely belong to the domain of FLB.

On this latter issue, Emonds (2004) has already pointed out the apparent paradox that syntax is uniquely human but some of those features that drive syntactic derivation, notably ɸ-features, are not uniquely human. The paradox is lost once we notice that language evolution, just like all other instances of biological evolution, is a process of recruiting some old traits for a new combination. The lexicon, just like HFL itself, is a product of such reorganization. Although not necessarily the only possibility, anti-lexicalism seems to be one promising framework in which we can seek a theory of HFL that may attain a certain level of evolutionary adequacy.

Notice finally that to the extent that simple words are syntactically complex objects, it follows that Sub-Merge (Subassembly-type Merge) is always involved even in the derivation of two word utterances. This is so since to Merge *milk* and *cup* to form *milk cup*, for example, each of the two nouns must first be formed by Merge. In addition to corroborating the central role of Sub-Merge in the evolution and development of human language, this fact forces us to take a new look at the Swedish compound data (12), because it must be the case that (12a) is also formed by Sub-Merge. One can imagine quick solutions, such as allowing for (literally) root compounding of categorially unspecified roots, as in {MILK, CUP}, before this new object is specified as N and realized as *milk cup*. In the absence of any direct empirical evidence for (or against) such a move (but see fn. 5), I will not discuss this possibility any further. Compounding continues to be a highly important phenomenon in understanding the nature, development and evolution of Merge (see Roeper 2007 for a close examination of Merge and compounding as they develop in children's minds).

5.   Final Remarks

As theoretical linguists, we certainly understand more about the internal machinery of human language than researchers in other fields, and it is in this respect that we make our own contribution to the multidisciplinary study of language evolution. In fact, it makes almost no sense to try to approach the topic without first establishing a working model of HFL, and in this sense generative biolinguistics constitutes the most productive, if not ultimately correct, frame-

work for research.

Against this theoretical linguistic approach, it is sometimes objected that a full understanding of language evolution must take into consideration various 'external' or environmental factors affecting the way language evolved in one way or another, most notably the human 'life history'. For instance, humans have prolonged infancy, and one may want to suggest that language evolved in order to firmly maintain the mother-child bonding as a means of communication.

This explanation has every kind of weakness a typical adaptationist scenario does. First and foremost, communicative utility, whether it is for social bonds, courtship or competition, is not enhanced by the formal device of unbounded Merge, and therefore its evolutionary explanation must be sought elsewhere. Second, in order to be usable at all as a communicative tool between mother and child, HFL must have already existed in their brains. Natural selection only chooses among existing variations, some of which happen to be fitter than others, and whatever is to be chosen must be present before selectional pressures work. It is the 'arrival' of the fittest, not its survival, that truly matters in biological evolution. Specifying the benefit and advantage conferred on us by the existence of language is no explanation of its initial arrival.

Generative biolinguistics, by contrast, succeeds in asking the right kind of questions, if not in answering them, by decomposing language into distinct components or modules. It explores the nature, origins and evolution of each of these components before they were interconnected to form what later became HFL, without confusing their respective original adaptive functions (not linguistic at all) with their current utilities in the organization of language.

This point should be kept in mind when we discuss the 'communicative function' of Internal Merge (Move), too. It is sometimes suggested that the dual function of language as a tool for thought and communication is served by External Merge and Internal Merge, respectively (Chomsky 2005a). External Merge establishes θ-relations and argument structure, whereas Internal Merge defines discourse-related information structure. While these descriptions may correctly characterize the functional motivations for applying these operations, they are not to be understood as explaining their origins and evolution, for the obvious reason that the capacity for them had to exist in the human brain before they were co-opted in the suggested dual manner.

Last but not least, thinking about the origins and evolution of language, especially from the viewpoint of exaptation and recombination of preexisting faculties, leads to a serious reconsideration of the proclaimed species-specificity and domain-specificity of language. Taken in isolation, each of the capacities that jointly constitute our language faculty, including Merge, is not strictly specific either way at the levels of genes and neural substrates, for evolution and development alike. The pursuit of evolutionary adequacy invites us to seek an integration of generative grammar into the multidisciplinarity of, say, evolutionary developmental linguistics (*evo–devo* linguistics; see Locke 2009 for one such proposal). The future of generative biolinguistics largely depends on the success of such a unification.

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The Non-Biological Evolution of Grammar: *Wh*-Question Formation in Germanic

Jacqueline van Kampen

The *wh*-marking of questions in child English is as early as the appearance of the *wh*-questions themselves. The *wh*-marking of questions in child Dutch (and the other Germanic languages) is delayed until the acquisition of articles and free anaphoric pronouns. An acquisition procedure is proposed that succeeds to set first a typological difference, V2 for Dutch and $SV_{fin}O$ for English. The different setting of the typological parameters determines the *wh*-development in subsequent acquisition steps. The learnability approach relativizes Chomsky's poverty of the stimulus, but affirms his position that language is 'perfect' in the sense of being learnable as a cultural construct without the assumption of innate grammar-specific *a prioris*.

Keywords: acquisition of *wh*-questions; child Dutch/English; cultural evolution; learnability; lexicalism

1. The Acquisition of *Wh*-Questions

1.1. *Outline of the Article*

I will first draw the attention to an acquisition problem that has been noticed before. *Wh*-elements in Germanic V2 languages do not appear in child language questions before the acquisition of the V2 rule and the subsequent acquisition of articles and free anaphors. By contrast, the *wh*-elements in $SV_{fin}O$ English appear as early as the constituent questions themselves. Both types of languages ($SV_{fin}O$ English and V2 Dutch) use clause-initial *wh*-elements in the same way. There is no difference in the *wh*-parameter. The acquisition difference must be due to the different typological background. The presentation of that problem constitutes the first part of this article. The second part will sketch an acquisition procedure that derives the phenomenon from the basic typological difference.

In the third part, I will argue that typological alternatives (parameters) are just those grammatical properties that are the first to be derived from input. The

simple reason is that the learner applies a systematic input reduction based on ignorance. The residues of that reduction single out the major typological properties. Once set, they determine the further developmental track towards the target grammar. This reminds of evolution. Preceding stages determine the way in which the subsequent stages adapt to the environment. Environment in the case of first language acquisition is the adult input language that the child's system gradually adapts to. The fact that typological properties are derived from input, rather than being *a priori* parametric alternatives, does not prevent them from characterizing major alternatives in language design. To the contrary, the fact that they are the first to be acquired causes them to influence the further course of acquisition. It rather seems that the reason for language types to be there and to remain so is that they enable an acquisition strategy. It is not claimed here that the language type enters the acquisition procedure as a bunch of typologically representative patterns that are further elaborated upon as in Tomasello (2003). Rather, I will argue, *contra* Construction Grammar, that each acquisition step, including the ones towards a certain language type, develops a category that is stored in the lexicon and that is characterized by its combinatorial properties. No phrase is used by the child unless all its lexical elements have a provisional categorial label that specifies its elementary combinatorial property. The somewhat odd forms of early child language can be derived and explained from the principle 'Establish such a grammatical category first'.

The evolution of a minimalist grammar in language acquisition needs no more than two types of elementary acquisition steps, both based on a locality frame (Roberts 2001). One type of acquisition steps serves a *Merge* construction and its categories, and the other one a *Move* construction and its categories. Both steps derive a category and its combinatorial property from its most simple and local pattern. The intricacy of grammars follows from a combinatorial effect which needs neither be innate nor learned. It is just implied by previous acquisition steps. The successive grammatical categories show standardized semantic oppositions, for example <±definite> for reference marking or <±aspect> for predication. These oppositions reconstruct part of the pragmatic understanding into grammatical oppositions.

1.2.   A Paradoxical Fact

Some properties of the target grammar are acquired before others. Initially, some children make more headway in matters of grammar than others, but in the end they all succeed and more importantly, they all succeed along the same line of partial acquisition steps that is implied by the target language. The order of acquisition steps gives an important indication how a first grammar is acquired (see also Brown 1973: 427). The empirical case presented here is the acquisition of root *wh*-questions in child Dutch (and other Germanic V2 languages) as opposed to the same procedure in child English (SV$_{fin}$O language).

The order of acquisition steps in the two languages is strikingly different. When acquiring *wh*-questions, English children use *wh*-pronouns from the start. The first *wh*-questions, though, as in (1), lack a finite verb (Klima & Bellugi 1966). The English child introduces the finite verb in a later acquisition step. See the

adult examples in (2) that appear later in the speech of the English child.

(1) a.   What that?
     b.   Where bear go?
     c.   How I get in?

(2) a.   What is that?
     b.   Where does bear go?
     c.   How will I get in?

Children acquiring a V2 language like Dutch, German, and Swedish, rather start their *wh*-questions with the finite verb in clause-initial position, and they avoid the *wh*-pronoun. See the early child language examples in (3).

(3)   *Child Dutch*   *Child Swedish*   *Child German*
     a.   Is dat nou?   Är det den?   Ist das denn?
            is that then   *is that then*   *is that then*
            'What is that?'   'What is that?'   'What is that?'

     b.   Moet dat nou toe?   Är den andra bilen?   Sitz du denn?
            must that now at   *is the other car*   *sit   you then*
            'Where must that go?'   'Where is the other car?' 'Where do you sit?'

     c.   Gaat deze nou open?   Öppnar man då?   Geht das denn?
            goes this now open   *opens   one   then*   *goes   that then*
            'How does one open it?' 'How does one open it?' 'How does it go?'

Dutch, German and Swedish children introduce the *wh*-pronoun in the first position in a later acquisition step (Tracy 1994 for German, Santelmann 1995 for Swedish, van Kampen 1997 for Dutch). See the adult examples in (4), that appear later in the speech of the Dutch child.

(4)   *Dutch*   *Swedish*   *German*
     a.   Wat is dat?   Vad   är det?   Was ist das?
            what is that   *where is that*   *what is that*
            'What is that?'   'What is that?'   'What is that?'

     b.   Waar moet dat naartoe? Var   är den andra bilen? Wo   sitzt du?
            where must that at   *where is the other car* *where sit   you*
            'Where must that go?'   'Where is the other car?' 'Where do you sit?'

     c.   Hoe gaat dit open?   Hur öppnar man?   Wie geht das?
            how goes this open   *how opens   one*   *how goes that*
            'How does one open it?' 'How does one open it?' 'How does it work?'

What causes the order preferences in child English (1) and child Dutch (3)? The acquisition difference cannot be due to a mere frequency difference in the input. All Dutch *wh*-questions start with a *wh*-element, as in English. I will argue that the difference in acquisition order can be explained as the solution to system-internal problems. Thereby, it will support my contention that grammar evolves

as a learnable non-biological construct. The order difference indicates that the acquisition device is attentive to the typological properties of the core grammar. The first question is how the child detects such typological properties, in the present case Dutch, as a V2 language, versus English, as a $SV_{fin}O$ language.

Let me formulate the kind of answer that I will develop. The child cannot attend to all data at once and she does not even try to. She applies a massive data reduction instead, and she subsequently builds a grammar for the residue only. That residue determines what new facts can be accommodated. The reduction procedure needs no innate, biologically pre-wired, knowledge. It is based on ignorance. Assuming that, a different acquisition path for *wh*-questions in English versus Dutch is still unexpected, since both languages have parallel constructions for their non-subject root questions. See the examples in (5):

(5)   a.   What   have   you   bought?   *English*
      a′.   Wat   heb   je   gekocht?   *Dutch*
      b.   Where   can   I   buy   a sandwich?   *English*
      b′.   Waar   kan   ik   een sandwich kopen?   *Dutch*

The constructions in (5) begin with a *wh*-phrase followed by an inversion of finite verb and subject. English and Dutch use the same shifts with the same categories. They move the *wh*-element to [Spec,CP] and the finite verb to C^0.[1]

(6)   a.   Move a <+*wh*> element to [Spec,CP]
      b.   Move a <+fin> element to C^o

Both languages get their root questions by the same two movement types. English, a 'residual V2' language, differs from other Germanic languages by allowing subject–verb inversion for a small group of functional verbs only (modal and auxiliary verbs, so-called 'Auxes'). The other Germanic languages ('regular V2') allow inversion for any finite verb, and moreover they allow it for questions as well as topicalizations. The subject–verb inversion indicates for both systems that the initial notion 'topic' turns into the notion 'subject'. 'Subject' is definable as a clause-internal argument in real grammar. It combines with a predicate category, whereas 'topic is definable as a pragmatic distinction in proto-grammar. It prefers the initial position and names the aboutness of the utterance (cf. Krifka 2007). One would expect that the primary learners of non-English are better prepared than the learners of English to acquire *wh*-words and inversion. The examples presented in (1) and (3) show that this is not the case. Dutch, German, as well as Swedish children start to use V2 and subject inversion early, especially for modals and copulas, but they delay the introduction of *wh*-words. English children, by contrast, introduce *wh*-words early and rather delay the residual V2. Different primary systems (V2 Germanic, residual-V2 English) apparently invite different data-selections for *wh*-questions. This difference in acquisition paths between the two languages is intriguing, since the grammatical target forms themselves seem identical, cf. (5).

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[1]   English subject *wh*-questions are left out. A questioned subject does not move in English ('vacuous movement', Chomsky 1986: 48), and fits into the general $SV_{fin}O$ pattern.

1.3. *The Longitudinal Picture*

The claims about the different order of acquisition steps in English and Dutch are not based on impressions. For each acquisition step and each child one may construct a longitudinal graph. Once scattered data begin an irreversible rise towards the adult norm, the child gets the pattern. I will assume that the child has reached the acquisition point when the graph is around the 85–90% conform to the adult norm (Brown 1973: 305). I have constructed longitudinal graphs of the development of *wh*-pronouns and finite verb movement to C^0 for American-English Sarah (Brown corpus) and for Dutch Sarah (van Kampen corpus). See Evers & van Kampen (2001: 23–28) for a detailed account of the data. The findings are based on the language development of two children, but the picture is confirmed by a longitudinal study of other children. The acquisition speed of children may differ, but the order of the steps is fixed and typologically determined. Typological features are simply those that are acquired first (van Kampen, in press).

English only applies a movement of the finite verb to the C-position in root questions for the restricted set of Auxes. This so-called 'residual V2' (Rizzi 1990) is acquired late. It obviously is a difficult thing to get and the children delay it until the second half of their third year, which is late in child language. By contrast, the English *wh*-pronoun appears one-and-a-half years earlier, which is early in child language. Even more important is the fact that the use of the *wh*-pronoun is instantaneous. There is no period in which the English learning child omits the *wh*-pronoun.[2] See Graph A in Figure 1.[3]
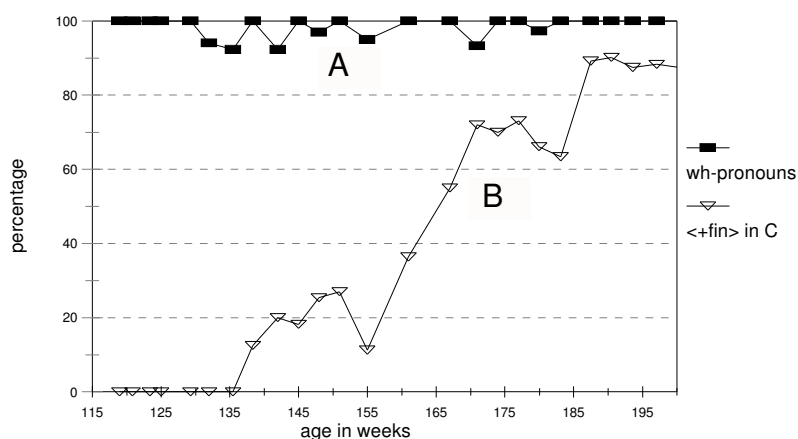

Figure 1: English: A<+*wh*> → B <+fin> in C° (Sarah, Brown corpus)
Graph A <+*wh*> in front: at 2;3 (instantaneously)
Graph B <+fin/+aux> to C^0: 2;3–3;7 (its rise takes more than a year)

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[2]   Graph A in Figure 1 shows that child English sometimes drops the *wh*-pronouns, but as an exception only.
[3]   Repetitions and imitations were left out.

The Dutch acquisition path is completely different. Since Dutch is a V2 language, the finite verb always moves to the C-position. The Dutch children begin with the V2 rule around their second birthday, and it may take them some 4–5 months to establish the V2 rule. During that half year, questions are posed by the child, but the use of *wh*-pronouns is avoided. It is only after the establishment of the V2 rule that the *wh*-pronouns come in. When the *wh*-pronouns come in, they are not acquired instantaneously. It takes again some 4–5 months for Dutch Sarah before all constituent questions appear with a *wh*-element. See the graphs in Figure 2.
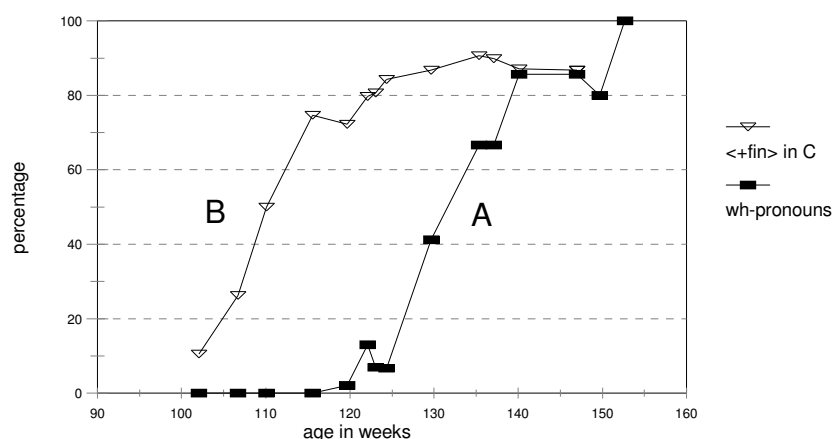
Figure 2: Dutch: B <+fin> in C^0 → A <+*wh*> (Sarah, van Kampen corpus)
               Graph B <+fin> to C^0:   between 2;0–2;5 (rise graph 4–5 months)
               Graph A<+*wh*> fronted: between 2;0–2;8 (rise graph 4–5 months)

When comparing the instantaneous English graph A for *wh*-pronouns in Figure 1 and the developmental Dutch graph A for *wh*-pronouns in Figure 2, one may notice how outspoken the English/Dutch differences are. In a nice counterbalance see the graphs B for V<+fin> movement in Figure 1 and Figure 2. English residual V2 (graph B in Figure 1) is slow and delayed when compared to Dutch V2 (graph B in Figure 2). It takes American-English Sarah a full year. The acquisition of V2, graph B, for Dutch Sarah is around week 125. Shortly after that point, the Dutch graph for *wh*-pronouns begins to rise. The point I want to make here is the A/B acquisition order, not the timing differences between the two Sarahs. Some children make more headway in matters of grammar than others, but that is not interesting. The relevant point is elsewhere. The order of acquisition steps is the same for all children given a target language. That order betrays the child's decoding procedure.

The question why residual V2 is slow as compared to full V2 gets even sharper if one looks at the finite verbs that establish the V2 type in early child Dutch. These are all the very Auxes (modals, copula; and in addition for Dutch the aspectual *gaan* 'go') English applies residual V2 movement to. Dutch children start with finite denotational verbs only later (de Haan 1987; graphs from Evers & van Kampen 2001). Graph B in Figure 2 can therefore be refined as in Figure 3.
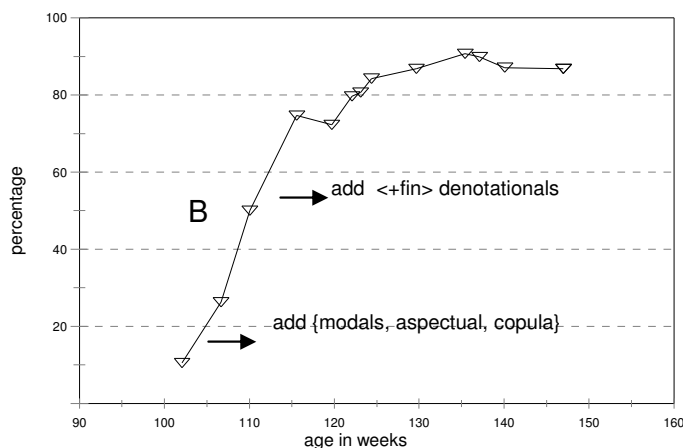
Figure 3: Dutch Sarah: Rise of <+fin> marked predicates in C⁰

The graph for Dutch in Figure 3 reflects <+fin> for early *wh*-questions, but also <+fin> for declaratives: *(papa) moet doen* '(daddy) must do', *k-ga even kleuren* ('I go just color' = I will color), *dit is beer* ('this is bear').

I will now argue that the English SV$_{fin}$O type leads child language towards a topic-oriented proto-grammar, whereas the Dutch V2 type leads towards a clause-operator proto-grammar. That difference in proto-grammar dictates the difference in the <+*wh*> acquisition order.

2.   The Child's Strategy

2.1.   *Input Reduction*

The central idea is that the child begins with a massive reduction of the input. It should be possible to predict the reduction stages given an adult target grammar. The learning strategy consists of constructing intermediate grammars that overcome the reduction in a stepwise fashion (cf. Dresher 1999 for phonology). The reductions are part of a decoding procedure: leave out temporarily all elements that you cannot sufficiently identify yet. Initially, the child starts with learning single word-signs. Subsequently, the child combines two words to binary structures. The initial strategy is formulated in (7).

(7)   *Input-Reduction Filter*
      a.   Leave out all that you do not recognize.
      b.   Restrict yourself to single binary combinations of pragmatically interpretable items.

The input-reduction filter formulated in (7) is based on the grammatical ignorance of the acquisition procedure, not on innate knowledge that informs the acquisition procedure which material to leave out where. The child is now bound to leave out all grammatical markings as not interpretable. The residue then consists of words that are either (i) denotational words that are interpretable in

the pragmatic situation or (ii) pragmatic deictic and illocution elements, like demonstratives and modals. The first grammar arises when two pragmatically interpretable words are combined in a binary construct. This initial proto-grammar without grammatical markings or categories appears in the schema in (8) as G_0. The target grammar appears as G_n. The acquisition series of intermediate grammars G_i elaborates on a corresponding picture in Chomsky (1975: 119f.).

(8) G_0 ---- $G_i \Rightarrow G_{i+1}$ ---- G_n

The transitions in the series are discrete. Each transition step adds a functional feature F_i and stores it as a property of a lexical item or a property of a category of lexical items. Longitudinal graphs show how an addition is optional first, becomes more frequent and then turns into a grammatical obligation. As long as the possible constructional contexts are still limited, no more than one single grammatical feature is learned at a time together with its distribution. This recapitulates the Single Value Constraint in formal learnability (Berwick & Weinberg 1984: 208, Berwick 1985: 108, Clark 1992: 90, Gibson & Wexler 1994). A more careful analysis of acquisition steps may show how certain grammatical features cannot be acquired before others have been established. To offer a trivial example, agreement on the finite verb cannot be acquired before the category <+D> has the features for person and number. See van Kampen (2005, 2006b) for quantitative support of this claim. The acquisition procedure re-traces a category-ial learnability hierarchy that is imposed by the system.

Each new acquisition step is a pattern recognition, defined an 'evidence frame' in (9) (Evers & van Kampen 2001, 2008). From a somewhat more abstract way of looking at the acquisition steps the language acquisition procedure needs two types of evidence frames in parallel with the generative devices 'Merge' and 'Move'.

(9)   a.   Adding a new category/grammatical feature to a reduced pattern.
             (Merge)
       b.   Moving an additionally marked category within the reduced pattern.
             (Move)

Hopefully, the acquisition procedure will only need these two types of maximally simple pattern-recognition ('treelets'; Fodor 1998, Sakas & Fodor 2001) to derive grammar from input. This is not meant as a procedure for rote-learning of grammatical distinctions. The cognitive distinction is recognized and then as such automatized as a grammatical reaction.

Adding a new category/grammatical feature F_i to a reduced pattern by Merge is illustrated in (10) for the English auxiliary *is*.

(10)   *Treelet for <+fin>/<+aux> Merge*

```
               ZP
             /    \
          XP        ZP
         bear      /   \
               Fᵢ       ZP        ⎧ ZP selection
                      walking     ⎪ ZP adjacency
                                  ⎨ ZP stress
                                  ⎪ > 2/3 frequency
                                  ⎩ Fᵢ = is  [Fᵢ --- –ing opposes to a lexical frame
                                                      without –ing]
```
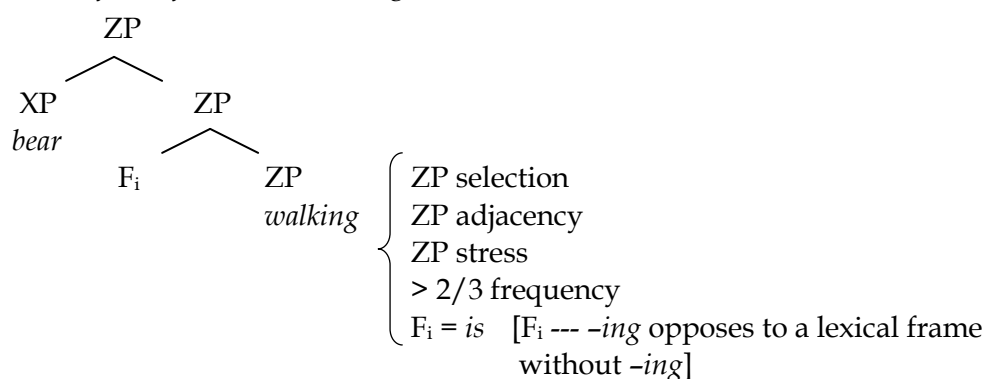
The child must already have pragmatically understood that *walking* was about the "person *bear* actually moving around." Adding the grammatical marking turns the 'comment' into a grammatically identifiable predicate. The grammatical marking (F_i) and its function is input identifiable. It need not be selected from an *a priori* set, but is acquired on 'robust evidence'. The addition becomes obligatory when the evidence frame supports the feature for >65%. The rest {bear loves walking; bear walking along found the honey; etc.} is disregarded by the acquisition procedure.[4]

It is claimed here that the lexicon inspires the underlying structure (cf. Evers & van Kampen 2001, Tracy 2002, van Kampen, to appear). Due to the lexicon the learner returns to the original frame from which the new and perceived pattern can be derived.

(11)   *Treelet for <+fin> head movement*

```
               ZP
             /    \
          XP        ZP
                  /    \
               Fᵢ       ZP        ⎧ ZP selection
                ▲      /  \        ⎪ ZP adjacency
                │   Z (gap)        ⎨ ZP stress
                │                  ⎪ Fᵢ = <+fin> ⎤[Fᵢ --- gap Z opposes to a lexical
                └──────────┘       ⎩      <+C>   ⎦lexical frame with a final Z <–fin>]
```
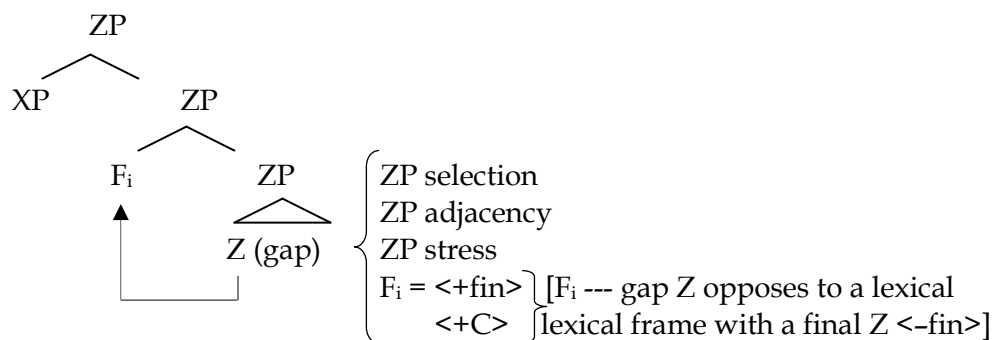
The reduction procedure then triggers the two steps in (12).

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[4]   A discussion about the learnability of island effects in Pullum (1997) and Yang (2002: 112) mentions input data percentages of 1.2% versus 0.03%. Such percentages seem to me unfortunate. The amount of data that reaches the child's eardrums is basically irrelevant. For example, the percentage supporting the Dutch V2 rule is near to 100% and the use of articles before nouns is perhaps 75%. Yet, the child manages to disregard all that evidence until she gets hold of the relevant evidence frames, respectively a frame for illocution marking (C^0/V2) and a frame for argument marking (D^0). Quantities of input data are relevant only if related to an evidence frame. For an alternative analysis of the island effects mentioned in Pullum (1997) and Yang (2002), see van Kampen & Evers (2006), Evers & van Kampen (2008). For an analysis along these lines of long *wh*-movement, see van Kampen (2009).

(12) a.   Reduction of the input yields a simplified binary basic set to get the
             elementary pattern for F_i.
      b.   The reduced pattern highlights a minimal extension F_i that makes the
             pattern more 'adult', i.e. less reduced.

The minimal distinctions between the reduced pattern and the perceived one
function as a data selection mechanism for the step towards F_i (see Berwick &
Weinberg 1984: 208). This overcomes the notorious poverty of the stimulus.

Merge and Move treelets like (10) and (11) are given as pattern-recognition
schemes. They are not necessarily grammar-specific. The recognition of a cate-
gory F_i (grammatical feature) in a set of utterances is the truly innovating step. It
need not come 'easy'. The merging of the new category also involves an abstract
semantic function. These functions may be based on a few simple oppo-sitions of
tense, aspect and definiteness, at the same time they are abstract, language-
specific and very hard to come by in second language acquisition later in life. Yet,
further acquisition steps are blocked until the F_i has been incorpor-ated. The
amount of elementary input structures that are needed in the beginning may run
into six digits of elementary acquisition opportunities (Hart & Risley 1995, van
Kampen, in press). Binarity, recursion, headedness and locality of movement or
the local reach of functional categories follow from the locality of the evidence
frame. In section 2.2, I will show how binarity and recursive stacking may
emerge in early child language.

In short, the input reductions do not yield some sloppy set of deficient
forms. One may rather define them as stages in a procedure for systematic
decoding. The system is designed for that kind of decoding for reasons of
survival by learnability fitness. Let each acquisition step be equivalent to adding
a grammatical feature F_i to the lexicon. That addition (morphological, syntactic
and semantic) takes place within an elementary syntactic 'treelet' as in (10) and
(11). Once the acquisition step has been made, the elementary treelet disappears
and the grammar enriched lexicon remains (*contra* Construction Grammar).

(13) The grammatical feature F_i infects a lexical item due to a repetitive local
      context that unites
                      (i) a morpho-phonological form,
                     (ii) a binary syntactic context,
                    (iii) a semantic distinction.

The images of an acquisition 'treelet' infected by features are taken from Fodor
(1998, 2001) and Roberts (2001). An important difference is that both these
authors still assume that treelets/features are determined by innate factors, a line
of reasoning not followed here. The early structures may demonstrate the rele-
vant categories and their combinatorial rules in minimal treelets. A subsequent
rapid expansion into more complex patterns need no longer be based on addi-
tional categorial properties or new combinatorial rules. Complex constructions
may simply emerge from re-combinations of already established categories and
rules. If so, discussions about the learnability of grammar can be restricted to a
basic set of early acquisition steps. These may reconstruct the categories with

their minimal combination properties and add them to the items in the lexicon. Once the basic category configurations are acquired and stacked in the lexicon, further complexity effects are implied rather than being learned or innate. Such properties in grammar need therefore not be innate in the sense of organs like the eye or the ear. Typological properties are at first simple solutions selected in history for their learnability, and as such they appear unambiguously in the input, given the input reduction filter. See van Kampen (2009) for an analysis that derives long *wh*-movement and island constraints from elementary steps that have a minimalist orientation.

In the remaining of this article, I will show the plausibility of the present approach by a longitudinal picture of *wh*-question formation in the speech of Dutch Sarah and American-English Sarah. The acquisition model presented here is empirically supported by the stepwise acquisition as shown in the child data. The advantage over the approach taken in Fodor (1998, 2001) and Roberts (2001) is then twofold. The treelets as assumed here are not innate but input-derived, and they force predictions about the order of acquisition steps.

2.2. Proto-Grammar

The binary constructions by which children start their grammatical career in Dutch and in English are different due to corresponding differences from the typologically different inputs (V2 and $SV_{fin}O$). The first maximal reductions to binary types show a denotational that characterizes the situation while being supported and preceded either by a topic name, or by an illocution operator. These reductions may be analyzed as a kind of topic adorned comment or an operator adorned comment, see (14). The combination of a comment with an operator or a topic has again the pragmatic status of a 'comment', that is, a simplex or binary characterization of the situation at hand.

(14)

	comment$_{<+topic>}$		comment$_{<+operator>}$		
	topic	comment	operator	comment	
English $SV_{fin}O$	*daddy*	*do*	*wanna*	*bear*	
	door	*open*	-----------		
	rabbit	*on*			
Dutch V2	*papa*	*doen*	*wil*	*beer*	'wanna bear'
	deur	*open*	*kannie*	*dicht*	'cannot close'
	nijntje	*op*	*is/zit*	*op*	'is on
			moet	*doen*	'must do'
			magwel	*kleuren*	'may color'

The comment is some denotational characterization of the situation whether adorned by a topic or an operator or not. The operator may be defined as a standard addition for an illocutive orientation (*wil* wish, *moet* requirement, *kannie* denial, *is/zit* statement, *magwel* permission; see van Kampen 2005). The

topic may be defined as a standard addition for an aboutness orientation. The grammatical development sets in when the comment begins to require a topic or an operator of a certain kind. This is modeled by the context features added in (14) to 'comment'. The relation between the two elements in the binary construction is pragmatic and need not be different from the relation between utterances of two single words in a discourse. That pragmatic relation may develop into a standardized grammatical one, when the properties mentioned in (13) become obligatory and define the combination as a phrase (projection of grammatical features). In this way, recursion (applying words to words and phrases) emerges naturally.

The difference between $SV_{fin}O$ and V2 input reduction causes that $SV_{fin}O$ child English tends to begin all declaratives with a subject, that is, the topic. Dutch *may* begin a declarative with a topic/subject, but it need not do so. Questions and declaratives may as well start with a finite modal-like verb. Remember that before the acquisition point of V2 (week 125) the finite verbs are not yet denotationals, compare the graph in Figure 3. Therefore, they may be classified as modal 'illocution operators' in child language. This 'format' is typical for V2 languages. It sets the stage for the later (syntactic) clause-typing property of V2. The illocution operators will not become finite verbs until after the acquisition of V2, that is, the rise of an elementary lexical paradigm based on the <±fin> distinction for a set of items, the prospective verbs. See Evers & van Kampen (2008) and van Kampen (in press).

The topic/subject in early child Dutch declaratives is far less likely (28%) to appear in clause-initial position than the operator/V<+fin> (72%).[5] Sarah's score for declaratives in weeks 110–125 is listed in (15) (from van Kampen, in press). Week 125 is the acquisition point of V2 for Dutch Sarah. The high amount (51%) of subject/topic-less utterances (15c) is due to the modals that appear as subject-implied factors (van Kampen 2006a).[6] See also Yang (2002: 107) who reports 40–50% V_{fin}-initial sentences in the speech of the child Hein.

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5   Strictly speaking, the topic from proto-grammar can be reinterpreted as subject only after it is obligatorily present and after its position and its case and φ-features become predictable given the comment. The systematic relevance of case and φ-features appears after week 145 for Dutch Sarah with the acquisition of D^0. See graph D in Figure 6 (van Kampen, in press). Early child language turns thereby into later child language. All pragmatic (situation-oriented) categories are replaced by syntactic (clause-internal definable) categories. I propose that the child arrives at that stage when all lexical items are appropriately marked as {<±C>, <±I>, <±V>, <±D>, <±N>}.

6   One reviewer suggests that the (72%) child Dutch V1 utterances (15b–c) are caused by topic-drop, i.e. the dropping of a topic in [Spec,CP], a possibility in adult Dutch. I would rather argue that topic-drop is a discourse-related phenomenon that can only develop with the acquisition of <+D>/argument structure and crucially after the acquisition of V2, not before. See for the theoretical and empirical arguments that early child Dutch do not exhibit topic-drop but mode-implied subjects, van Kampen (2006a). See also Yang (2002: 107, fn. 6).

(15) *Dutch Sarah week 110 till 125 ('acquisition point' V2)*

(relative % of all declarative V<+fin> sentences; out of 595)

a. Subject–V<+fin>	28%	clause-initial *topic*: 28%
b. V<+fin>–Subject	21%	clause-initial *operator*: **72%**
c. V<+fin> (no subject)	51%	

The attention to the modal illocution operator is supported by the maternal input. More than half of the declaratives of the Dutch input (52%) do not start with a subject topic. The percentages in (16) show the prominence of subject-finite verb inversion in speech of (Dutch) Sarah's mother (from van Kampen, in press).[7]

(16) *Adult Dutch ± subject-initial clauses*

Subject +V<+fin> ([Spec,CP] = subject)	257	48%		
Non-subject+V<+fin>+subject ([Spec,CP] = non-subject)	162	30%		
V<+fin>+subject (no/empty [Spec,CP])	97	18%	4%	topic-drop
			14%	narrative inversion
V<+fin> (no subject)	21	4%		

English children, by contrast, pay more attention to the topic+comment types. (Almost) all declaratives brought in by English Sarah's mother are subject-initial and even 63% of her (real) *yes/no*-questions had no subject–Aux inversions at all. They were simply statement frames with a question intonation.[8]

This will soon determine the further development. Typological factors derived from input take effect as (non-biological) determinants for the evolution of grammar.

Both elements in the front-field, topic and operator, are optional in proto-grammar. The presence of the comment is in principle obligatory. The topic and

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[7] Another reviewer remarks that Lightfoot (1999: 153) reports a high percentage (70%) of sentence-initial subjects in adult Dutch. The percentages in (16) then seem to contradict the ones in Lightfoot (1993/1995: 42, 1999: 153). Citing Gerritsen (1984: 110), Lightfoot (1993/1995: 42) states that V2 languages show some 60% subject+V_{fin} order in conversational speech. For unstated reasons, this percentage has risen to 70% in Lightfoot (1999). Gerritsen (1984: 110), though, citing Jansen (1978, 1981), reports about 40% non-subject+V_{fin} order. Note that this is not the same as Lightfoot's statement, witness my table (16) that includes also clause-initial V_{fin} (cf. Yang 2002: 107). However, suppose Lightfoot has good reasons to (silently) leave out the constructions with V_{fin}-initial, then his 60%/40% matches the percentages in my count in (16). These percentages are 48% subject+V_{fin} versus 30% non-subject+V_{fin}, which comes to a ratio of 48/78 = 62% versus 30/78 = 38%.

[8] I counted 209 *yes/no*-questions for English Sarah's mother in the files 11–26. Only 77 of them (37%) had an Aux in C⁰. Only 'real' *yes/no*-questions, i.e. questions that asked for a confirmation or a denial, were counted.
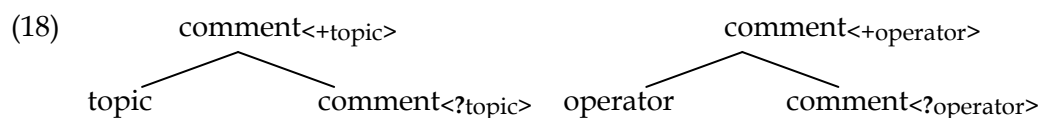
operator are word-status elements (no clitics or affixes) and they are added to a denotational comment.[9] Proto-grammar for both language types shows de facto (mainly) a single front field element, either a single topic or a single operator. The topic may be informally characterized as a word with a pragmatic aboutness function. It defines what the binary combination is about. The operator may be informally characterized as a word that signals a pragmatic illocution.
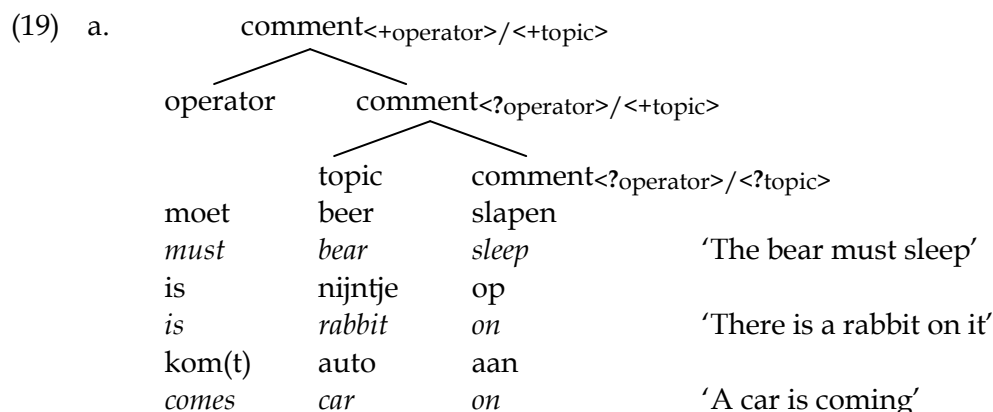
(17)   Optional front field

single topic   *single operator*

function: aboutness   function: illocution (wish, permission, etc.)
type: name   type: designated constant

The distinction between unadorned and adorned 'comment' evolves into a new system when topic and operator become obligatory in discourse-free statements (the non-answer statements).[10]

The either single topic or single operator for a comment can be modeled as in (18).

(18)   comment$_{<+topic>}$ comment$_{<+operator>}$

       topic   comment$_{<?topic>}$ operator   comment$_{<?operator>}$

The comment label continues to be a denotational characterization of the situation when the grammar is extended to three-word combinations. A set of three member utterances that appear in early child Dutch can be seen as rearrangement of the label 'comment' as in (19). The examples are from Sarah before week 122. The structures (19a) and (19b) are semantically equivalent options.

(19)   a.   comment$_{<+operator>/<+topic>}$

               operator   comment$_{<?operator>/<+topic>}$

                             topic   comment$_{<?operator>/<?topic>}$
               moet   beer   slapen
               must   *bear*   *sleep*   'The bear must sleep'
               is   nijntje   op
               is   *rabbit*   *on*   'There is a rabbit on it'
               kom(t)   auto   aan
               comes   *car*   *on*   'A car is coming'

---

9   Clitics and affixes are acquired due to a re-analysis that will take place only after the full-sized variants of the construction have been analyzed and acquired first (van Kampen 2001).
10   Thanks to Marcel den Dikken for pointing this out to me.

b.

comment_{<+operator>/<+topic>}

topic   comment_{<+operator>/<?topic>}

operator   comment_{<?operator>/<?topic>}

beer	moet	slapen	
bear	*must*	*sleep*	'The bear must sleep'
vliegje	is	weg	
fly	*is*	*gone*	'The fly has gone'
mama	moet	ook	
mummy	*must*	*also*	'Mummy must also do that'
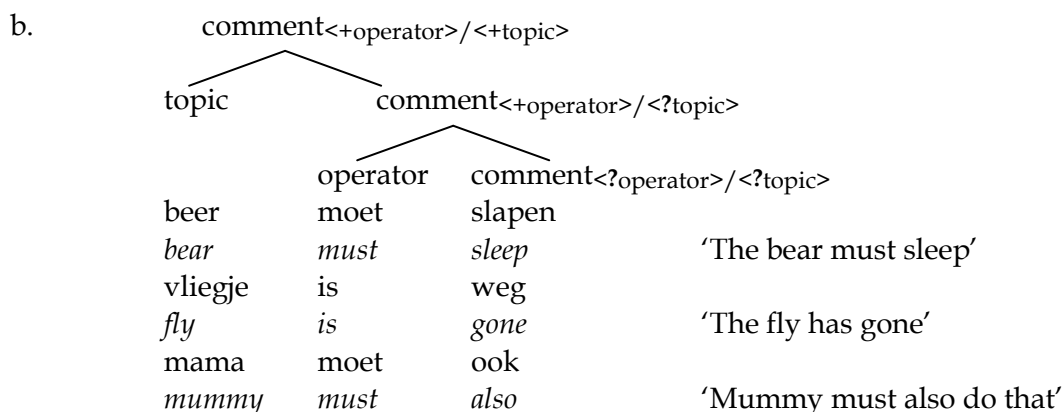
The binary structure from (14)/(18) is maintained in (19). Either an operator is added to a topic-comment structure as in (19a), or a topic is added to an operator-comment structure as in (19b). The sustained binarity for recursive stacking ('asymmetric Merge'; Chomsky 1995) of comment structures need not be considered as a grammar-specific constraint, something given as a grammatical *a priori*. Binarity simply makes use of parts that were already known as analyzable. This 'evolutionary' economy continues to operate and establishes binarity as a general frame preferred for grammar. A triple non-stacking tree is less likely to survive in daily use as it is not supported by previous steps whereas stacking by binarity branching is.

For example, in the vein of Categorial Grammar, if *dog* is identified as <+N> (can be used as a topic-name) and if the article *the* is identified as 'followed by <+N>', then a later appearing *[angry dog]* must be <+N> in *the [angry dog]$_N$* where *angry* is <–N> (not a topic-name) and hence *[dog]$_N$* the head of the phrase *the [[angry]$_{–N}$ dog]$_{+N}$.*. The recursion in *the [angry [dark-haired dog]]* follows logically if the rule N → A+N is repeatedly applied Merge/residuation. The binarity of the system was first a practical start and developed from there into a dominating property of the system. As such it is not necessarily an innate property of grammar, but rather a self-reinforcing tendency of the naïve acquisition procedure. A learner may have acquired the small phrase [β+γ]. When confronted with larger constructs, say [α+β+γ], there will be an immediate preference to hold on to the previous result [β+γ]. That favors the binary analysis [α+[β+γ]]. The pressure of such a learnability preference may in the long run impose on grammars the binarity principle. In general, let grammatical structures have the option to be (i) binary branching as well as multiple branching, (ii) headed as well as non-headed, (iii) locally conditioned as well as non-locally (globally) conditioned. Then, in the long evolutionary run, the restricted system is likely to win the learnability competition.

I see no clear arguments to consider binarity, headedness and recursion as grammatical properties that could not emerge naturally. When a pre-grammatical language would consist of single word utterances, as in very early child language, the relation between such utterances must be a matter of pragmatic understanding. Under frequent use, that pragmatic understanding might standardize to a set of fixed relations that can be supported by a grammatical form of order, inflection or an additional functional word.

The re-combinations in (19) maintain the restrictions known from (14)/(18) that utterances allow a single operator and a single topic only. Later on this type of additions and local feature control will expand in respectively '(semi-) auxiliary cartographies' and multiple argument structures. Yet, at this moment in early Dutch child language the utterances are analyzable in as far as they restrict themselves to a single operator for 'is an illocutionary unit'. That single illocution operator is the later finite verb in first or second position.

2.3. Wh-*question Formation*

Here I come to my central point. Relevant is not the mere frequency of the *wh*-construction, but the way it fits into the current child grammar G_i. The operator context of early Dutch adds a general operator (the later finite verb) to all illocutional utterances, declaratives and questions alike. An additional <+*wh*> operator requires operator stacking and is not particularly welcome. The <+*wh*> element is systematically present in the adult input (99.5%), but systematically disregarded in the Dutch proto-grammar, see (20).[11]

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[11] Adult Dutch may drop the *wh*-pronoun, but does so only rarely. I counted in the speech of Dutch Sarah's mother (files 09–23; child's weeks 107–146) 10 examples out of 674 *wh*-questions, of which 6 were direct imitations of Sarah's *wh*-drop questions. The 4 remaining examples were of the type in (i). The huge percentage of *wh*-drop before the acquisition of V2 in the speech of Sarah (98/108 = 91%) is at odds with the rarity of *wh*-drop in the input (4/668 = 0.5%).

(i)   Ø ben je   nou aan (he)t doen allemaal, Sarah?   (file 13, Sarah week 122)
       are you now on the   do   all   Sarah?
      'What are you doing 'then', Sarah?

This type of *wh*-question modulates the impact of the demand expressed by the question. The use of the sentence adverbial *nou* expresses the speaker emotional state (surprise, irritation, disbelief, etc.) vis-à-vis the interlocutor's behavior. It is the only context in which the *wh*-pronoun is sometimes dropped in adult Dutch.

   A peculiarity of this type of question is the (almost obligatory) use of *nou*. There are parallels for this in the other Germanic V2 languages. Child Dutch also uses the sentence adverbial, but without the emotive intention which is beyond the child's pragmatic ('theory of mind') understanding. *Nou* is overused in child Dutch to make the predicate of questions when the <+*wh*> operator is blocked. It reduces to the adult norm when the *wh*-element is introduced. For the overuse and disappearance of *nou* in child Dutch, see van Kampen (1997: 78f.).

(20) *Dutch proto-grammar: General illocution operator*

comment<+operator>/<+topic>

operator   comment<?operator>/<+topic>
illocution

       topic   comment<?operator>/<?topic>

gaat	pappa	nou doen?	
goes	*daddy*	*now do?*	'What is daddy doing?'
zit	vogeltje	op?	
sits	*birdie*	*on?*	'What is the bird sitting on?'
ga	jij	nou toe?	
go	*you*	*now to?*	'Where are you going?'
moet	dat	nou in?	
must	*that*	*now in?*	'Where must that go?'
komt		daar nou aan?	
comes		*there now on?*	'Who is coming over there?'

Dutch proto-grammar disregards <+*wh*> operators because its standard utterance prefers a single sentence-typing operator, the later V2 finite verb. As we have seen in (15), 72% of the <+fin>/operator elements in early child Dutch declaratives are clause-initial.

The English proto-grammar is different. It does not introduce the general clause-initial illocution operator. For that reason, it allows the <+*wh*> illocution operator as a question-specific device, see (21).[12]

(21) *English proto-grammar: <+wh> operator*

comment<+operator>/<+topic>

operator   comment<?operator>/<+topic>
<+wh>

       topic   comment<?operator>/<?topic>

what	daddy	doing
where	daddy?	
what		doing?
where	you	going?

English proto-grammar allows <+*wh*> elements as lexically restricted operators in stereotype questions (*where – go*?; *what – doing*?).[13] English proto-

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12 The '*wanna*' construction mentioned in (14) is a '*wanna*' pattern, rather than a pattern for modals in general. Next to the modal illocution operator that develops into sentence-typing operator in V2 Dutch, there are other operators in early child language. One may think of deictic operators (*dit/is beer* 'this/is bear', see Evers & van Kampen 2008: 490) or operators for negation (van Kampen 2007). Thanks to an anonymous reviewer for pointing this out to me. The present article focuses on the property of the sentence-typing operator in V2 languages.

13 See also Radford (1990) for an analysis of early *wh*-questions in English as stereotypes. Note also that complex *wh*-phrases (*which N, what N*) do not occur until much later.

grammar allows the <+*wh*> operator because its standard utterance does not have a sentence-typing operator. The Auxes in English regularly mark the predicate that follow the topic/subject. Therefore, English proto-grammar cannot immediately fit in the residual V2 Auxes. Residual V2 left of the topic/subject is disregarded by the child as an anomalous case of inversion. This is reflected in the successive graphs in Figure 4. The first graph, graph C, depicts the rise of Auxes in declaratives (*I <u>can</u> see daddy*). The succeeding graph, graph B2, depicts the rise of inverted Auxes in yes-no questions (<u>*Can*</u> *you see daddy?*). It shows that the (non-inverted) Auxes in I^0 are identified before the (inverted) Auxes in C^0. Aux–subject inversion is obviously harder to acquire. See Evers & van Kampen (2001) for a detailed account of the data selection.
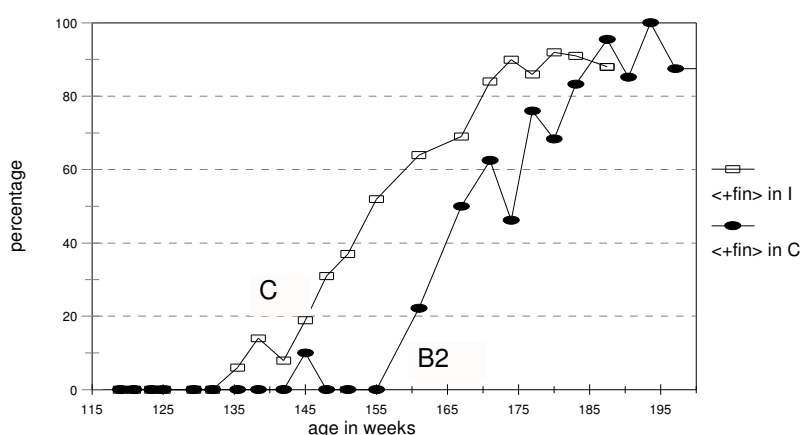

Figure 4: English Sarah: Graph C: <+fin>/<+aux> in I^0 (declaratives)
                          Graph B2: <+fin>/<+aux> in C^0 in *yes/no*-questions

Graph B2 represents residual V2 in *yes/no*-questions. The graph that establishes the residual V2 for American-English Sarah, graph B in Figure 1, generalizes over *wh*-questions and yes-no questions. Graph B in Figure 1 shows how it took American-English Sarah a full year to get the residual V2 in all questions. This extended period of hesitation must partly be due to the cliticized forms of copula, modal and auxiliary verbs in English *wh*-questions. In the speech of English Sarah's mother, two-third (77%) of the auxiliaries and modals were cliticized to the *wh*-pronoun.[14] See some examples in (22).

(22) a.   What'd [: what did] he say ?
       b.   What's your doggie's name ?
       c.   Where's the little doggie ?
       d.   Whyn't [: why don't] you go play with Bobo?
       e.   What's the boy sitting on?
       f.   Who's Daddy got ?

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[14]   I counted the *wh*-questions in the files 1–17, Sarah's weeks 118–133, just before the rise of the <+fin> graph (graph B in Figure 1). In these 17 files, Sarah's mother used 493 *wh*-questions. Of these 493 *wh*-questions, 380 (77%) had an Aux cliticized on the *wh*-element.

This stands in opposition to V2 Dutch. The Dutch modals and auxiliaries are explicitly present in the input as clause-initial operators. The copula/auxiliary *is* may be cliticized in Dutch, but most of the time the full form is used. A count of the copula and auxiliary *is* in CHILDES showed 70% cliticization in adult English (Brown corpus) versus 6% in adult Dutch (Groningen corpus and van Kampen corpus). See the table in (23).

(23)   *Adult input of cliticized and full copula/auxiliary* is

	total *is* and *'s*	full *is*	clitic *'s*	
Dutch (all files Groningen + van Kampen corpus)	29,606	27,872	1,734	**6%**
American-English (all files Brown corpus)	16,263	4,926	11,337	**70%**

One may assume that cliticized forms, i.e. the auxiliaries in English, will not trigger anything until the non-cliticized forms have been acquired and the re-analysis of the cliticized forms becomes possible (cf. Radford 1990, van Kampen 2001). This becomes clear when one splits up graph B from Figure 1 in a graph B1 and B2, as in Figure 5. Graph B1 represents the residual V2 for *wh*-questions. Graph B2 repeats the residual V2 for yes-no questions in Figure 4.
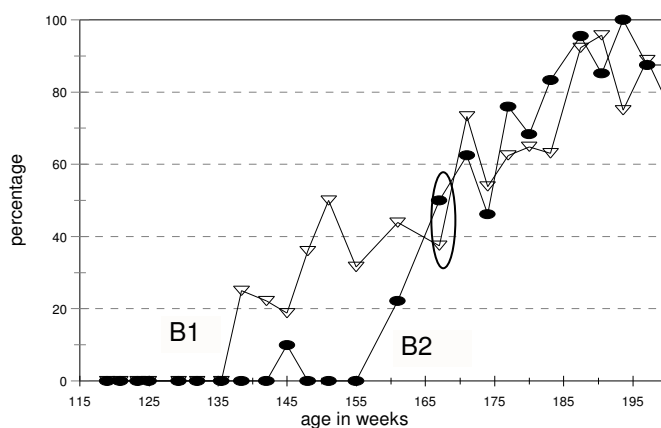

Figure 5: English Sarah: B1: <+fin>/<+aux> in C° in *wh*-questions
B2: <+fin>/<+aux> in C° in yes-no questions

The two graphs more or less coincide from the encircled point at week 167 on. Before that point graph B1 already has set in quite high. This might be due to the fact that the contracted form has not yet been identified as a cliticized 'Aux'. The contracted forms in the English *wh*-questions do not become analyzable before the auxiliary, copula and modal verbs have been acquired separately in *yes/no*-questions. The respective graphs then join at week 167 the general

development in B2 that might be characterized 'residual V2'. After week 167 the acquisition of <+fin> in C^0 follows a uniform development.

In sum, although the *wh*-elements are clearly and explicitly present in the English and in the Dutch input alike, the 'single operator' restriction causes the disregard of the <+*wh*> operator in Dutch proto-grammar. The type of proto-grammar creates a selective environment for certain acquisition steps only. As long as <+*wh*> functions as a question operator, it can be added in child English proto-grammar, but not in child Dutch. English proto-grammar will not select <+fin> Auxes in *wh*-questions, because they are not generally present in the input as clause-initial operators as they are in V2 Dutch.

2.4. Real Grammar

The acquisition difference between the *wh*-elements in English and Dutch has been derived from a difference in their proto-grammar. There appeared a topic-oriented proto-grammar from the English SV$_{fin}$O input versus an operator-oriented proto-grammar from the Dutch V2 input. Proto-grammar is the first attempt of the acquisition procedure. Its parts (comment, operator, topic) have an immediate pragmatic function for the utterance as a whole. The first non-pragmatic categories that emerge in Dutch are V<+fin>/V<–fin>. In adult Dutch, one third of the <+fin> operator elements (input tokens in CHILDES corpus) are variants of denotational comment elements and two third of the <+fin> operator elements (input tokens in CHILDES corpus) have a non-denotational background (auxiliaries, copulas, aspectuals, modals). The graph in Figure 3, repeated here as Figure 6, shows how the operator-marking in child Dutch rises. The amount of operator types (copula, aspectual, modals) rises as well.
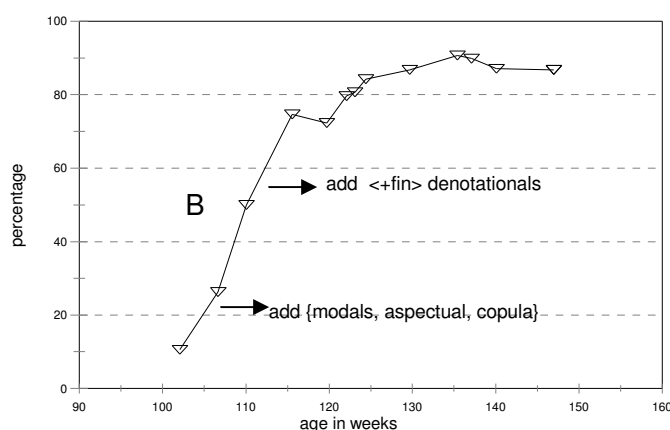

Figure 6: Dutch Sarah: Rise of <+fin> marked predicates

At a certain moment, indicated in the graph, the amount of operator types rises by the use of denotational forms with <+fin>-marking, that is, *beer slaapt ook* 'bear sleeps too', *ik heb snoepje* 'I have candy'. This allows a reinterpretation for the categorial status of lexical items that are involved. All elements that are

marked as <+fin> are part of a morphological <+fin> (operator)/<–fin> (final comment element). The paradigm defines the category <+V>. The <+fin> defines the notion 'illocution operator' (see Evers & van Kampen 2008 for discussion). Now, sentence operators tend to get interpreted as <+V, +fin>. The <+fin>-marking turns the <+V> in V2 Dutch into a sentential operator. The <+V> elements can be combined with topics/subjects and complements (direct, indirect, prepositional objects). The same type of elements (topic names/nouns) can be used in all these positions. The name-like elements tend to be marked by the same functional element (article or article-like form), which, due to its frequency in the input, can be picked up by the child. At the moment that the V2 <+fin> graph in Figure 6 passes the acquisition point at week 125, the <+V> associated topic/subjects and complements (direct, indirect, prepositional objects) begin to be marked by the articles or article-like elements. In this way, the category <+V> gives rise to argument structure frames that are to be stored in the lexicon as well. The names used in the argument positions give rise to the article-like category <+D>. See the rise of articles in the speech of Sarah in Figure 7. The interesting point is that the graph for determiners D<–pro> (articles), and the graph for free anaphoric pronouns (3rd person pronouns) D<+pro> coincide with the graph for D<+wh> (*wh*-pronouns), graph A in Figure 2. For Dutch Sarah, these three graphs reach the acquisition point around the age of 2;9 (week 145). The diagram in Figure 7 compares the acquisition of question pronouns (graph A) with the acquisition of articles (graph D).
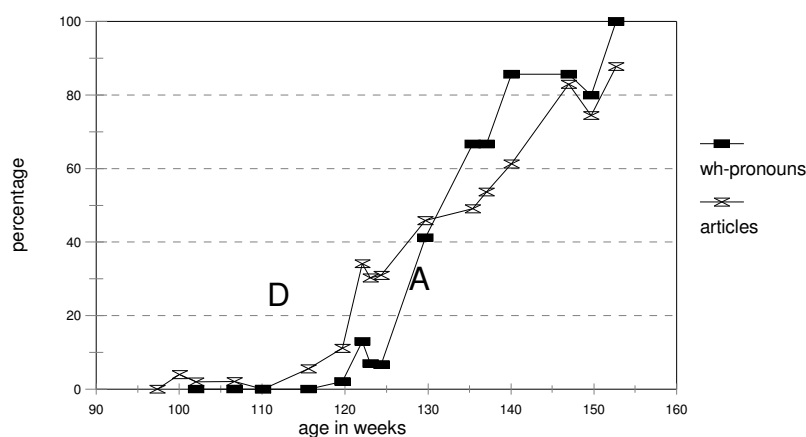

Figure 7: Dutch Sarah: Graph A: D<+pro, +wh> (question pronouns)
                            Graph D: D<–pro, –wh> (articles)

The diagram in Figure 8 compares the acquisition of question pronouns (graph A again) with the acquisition of 3rd person pronouns (graph C).
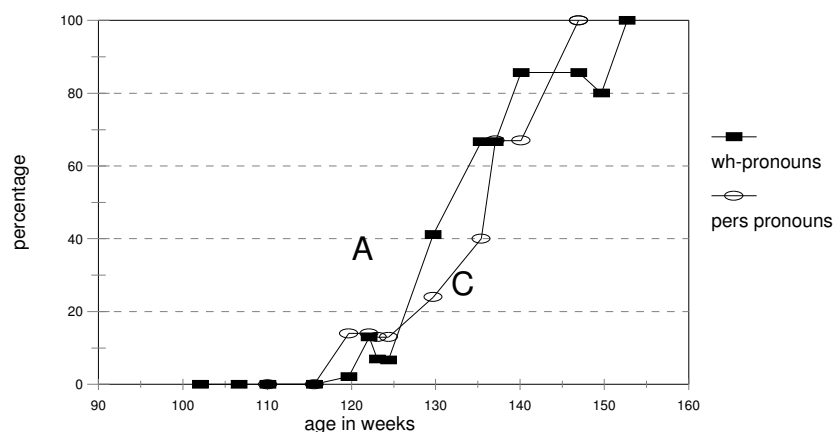
Figure 8: Dutch Sarah: Graph A: D<+pro, +*wh*> (question pronouns)
                           Graph C: D< +pro, –*wh*> (3rd person pronouns)

All these graphs for Dutch Sarah nearly coincide. They represent a more abstract phenomenon, the grammatical marking of discourse reference and clausal argument structure by the category <+D>. Just after the acquisition of V2 (at week 125), the use of the variant <+D> elements before names/nouns begins to rise. Argument structure gets established, once the predicate containing that structure has been shaped by a grammatical marking <+fin>. Predication (C^0/I^0) precedes reference (D^0). It takes the period between 2;4–2;9 (week 120–145) for Dutch Sarah's articles to reach the adult norm. The *wh*-element is a <+D> element too. The acquisition of Move <+*wh*> to [Spec,CP] takes place as soon as <+*wh*> is identified as a <+D> (determiner) in front of NPs. Reinterpreted as a D<+*wh*>, the <+*wh*> gets access to the clause-initial position. See some examples of <±*wh*> preposing in the speech of Sarah before and after the acquisition point at 2;9 (week 145).

(24)   <+wh> *preposing (*wh-*movement)*   *Dutch*

   a.   Wat   doet   de beer?   (Sarah 2;5, week 127)
         what   does   the bear
         'What is the bear doing?'

   b.   Welke   wil   je   boekje?   (Sarah 2;9, week 144)
         which   want   you   booklet
         'Which booklet do you want?'

   c.   Welk boekje   hebben   we allemaal?   (Sarah 3;4, week 174)
         which booklet   have   we all
         'Which booklet do we all have?'

(25)   <–wh> *preposing (topicalization)*   *Dutch*

    a.   Die   bewaar ik ook.   (Sarah 2;6, week 130)
           that   keep   I   too
           'I will keep that one too.'

    b.   De prinses is hele groot.   (Sarah 2;7, week 135)
           the princess is very big
           'The princess is very big.'

    c.   De klitten probeer ik eruit te halen.   (Sarah 3;1, week 163)
           the tangles try   I   out   to get
           'I try to remove the tangles.'

I expect a parallel development for the grammar of English. The category <+V> can be acquired due to the aspectual opposition <±–*ing*> and the associated use of auxiliaries and modals in I^0, cf. the treelet in (10). Once the category <+V> has been established, argument structure can be acquired and get stored in the lexicon (as V^0–DP frames and V^0–PP frames). The <±*wh*> operators are subsequently reanalyzed as preposed DP arguments. English grammar still has to add the residual V2 for root questions thereafter, reanalyzing a bunch of cliticized 'Aux'-constructions. The most important point, though, is the acquisition of the English D^0 articles. Probably, I can maintain my thesis that the acquisition of <+D> is a matter of acquiring argument structure after the acquisition of I^0/V^0.[15]

In sum, the Dutch/English difference in the acquisition of *wh*-questions is due to a difference in binary proto-grammar. Early child language turns into late child language by the three successive steps in (26).

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15   The English <±definite> article opposition *the/a* can be construed as following the I^0 graph, i.e. graph C in Figure 4. Yet, Sarah Brown, as well as other English acquisition children, shows a remarkable use of the element *my* well before the acquisition of I^0, i.e. *my bear, my go, my nice*, etc.. The element *my* stands for a variety of functions in child English, first person possessor (D^0) marking being one of them (e.g., *see my doggie*). One might argue that the use of *my* in early child language is situation-bound like the demonstrative *die* in Dutch proto-grammar. See van Kampen & Zondervan (2005) for an analysis of *my* by Adam (Brown corpus).

(26)   *Successive acquisition steps*

a.   *Proto-grammar*

Dutch (V2)	English (SV$_{fin}$O-residual V2)
• fixed operator initials (modals) learned in declaratives	• fixed topic initials learned in declaratives
• <+wh> operator blocked	• <+wh> operator possible

b.   *Predicate marking*

Dutch (V2)	English (SV$_{fin}$O-residual V2)
• <+fin> marking in C^{o}	• <+ *-ing*> marking in I^{o}
• category <+V>	• category <+V>

c.   *Argument marking*

Dutch (V2)	English (SV$_{fin}$O-residual V2)
• argument structure <+D> marking	• argument structure <+D> marking
• category <+N>	• category <+N>
• D<+*wh*> and move *wh* as argument reordering	• D<+*wh*> and move *wh* as argument reordering

In acquisition step (26c) both grammars prepose the <+*wh*> argument in the initial C^0 projection. The Dutch/English difference in <+*wh*> acquisition is a short-lived phenomenon of early child language that does not survive. Nevertheless, it demonstrates how fairly universal categories and redistributions are acquired from reduced stages of the language type. The order of acquisition steps supports the (minimalist) ideas that the grammatical patterns follow from input and general cognitive abilities. Indications for a biological pre-wired program fall away when it turns out that prospective universals like <+V>, <+N> and "move to C^{0}" are rather defined by and (non-biologically) derived from highly frequent language-specific hints in the input.[16]

Nobody will deny that languages are learned from parental input. Yet, the abstract nature of grammatical categories and their complex interaction in the

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[16]   One reviewer remarks that Dutch children start their two-word phase with infinitival OV predicates and that these OV predicates mark the development of argument structure at an earlier stage than suggested here. Note, though, that the learnability issue is not helped with the assertion that all categorial distinctions are a given *a priori* (Pinker 1984). Within the present learnablity context, no category has been acquired when there is no formal context yet. Early child utterances like *boekje lezen* ('read booklet') result from input reduction. The reduction patterns will satisfy the order in the input, but cannot be analyzed by the child with grammatical notions like object, verb and predicate before V2 has been acquired. All early 'theta assignments', like *boekje lezen*, are probably stereotype lexical associations, as already observed by Lebeaux (1988: 13). For a more detailed analysis of the OV problem, see Evers & van Kampen (2008).

adult language made it questionable that the system could be learned by toddlers. The present proposal nevertheless contends that the early learner reduces the input to small elementary constructions from which the various factors are identified and acquired. No reliance on biologically pre-wired forms of grammar needs to be assumed.

Proof that acquisition took place was based on the child's productions. Comprehension by children is undoubtedly more advanced, due to pragmatic orientation. Yet, grammatical competence is unambiguously present in production which for that reason is a better diagnostic for the system-inherent order of the acquisition steps. The point of interest here is not how early children may 'understand something', but how the order of acquisition steps is imposed by the grammatical system itself, given the child's acquisition strategy.

3.   Biological Construct or Cultural Construct?[17]

3.1.   The Acquisition Model

In a sense, the less one expects from an acquisition model of language, the more features of grammar one is likely to postulate as innate. Innate features need not be acquired. Somehow, they have already drifted in by neural evolution. By contrast, the present acquisition model sets a focus on the learnability of grammar. It needs no more than two elementary acquisition steps, one for a Merge pattern and one for a Move pattern. Highly abstract properties of grammar are subsequently derived from reduced input sentences. To the extend that this can be maintained, the acquisition model implies that the neural structures for grammar must have been acquired by learning, rather than being a pre-wired set of options that is innate due to the neural evolution of the species (van Kampen 2009). The main points of the acquisition model put forward in the article were the following:

Learning strategy

(i)   There is an initial reduction of the input, such that the acquisition device selects the major typological properties (major parameters) of the core grammar. The reduction is due to ignorance about functional structure and not due to *a priori* information.

(ii)   The input-reduction procedure directs the further development by selecting evidence frames that contain no more than one single functional category, i.e. grammatical feature, $<F?>$. Each acquisition step adds a grammatical feature F_i to the lexicon (or adds a grammatical feature F_i to elements already listed in the lexicon), together with the elementary context for F_i.

The context for F_i has appeared in the reduced input as a treelet (in the sense of Fodor 1998, 2001) and it has appeared as

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well (systematically) in the child's productions. This is demonstrated by constructing the longitudinal acquisition graph of F_i.

Learnability hierarchy

(iii)   There is a natural order of acquisition steps, since some grammatical features need others in their minimal frame. This phenomenon explains the temporal order between the acquisition graphs.

(iv)   The probably universal lexical categories V^0 and N^0 are not postulated but derived and acquired from their more language-specific functional environment, respectively the identified illocutionary value of C^0/I^0 and D^0. See also van Kampen (2005), Evers & van Kampen (2008: 504f.).

Outcome: The lexicon

(v)   Although each grammatical feature is first captured within a minimal treelet, the initial grammar is not seen as a bunch of constructions as in Tomasello (2003). The acquisition model is aimed at building up a categorial lexicon that specifies the local combinatorial properties of its items (*contra* Construction Grammar).

I demonstrated that the present acquisition model is able to set several categories and their parameters from input, such as the V2 parameter and the <+*wh*> parameter. The same model was effective in setting the OV parameter and the major lexical categories in van Kampen & Evers (2004), van Kampen (2005), Evers & van Kampen (2008). When the model derives some fundamental and typological properties from reduced input and does so in the same order as in actual child language, it becomes more plausible that all grammatical properties will be acquired in that manner. Notice that it is not assumed that these categories and parameters are used as *a prioris* by the learner. They are rather imposed upon the learner by the treelets of the reduced input.

The simplified and repetitive structures produced by systematic input reduction are not postulated. They are manifest in actual child language. There, they allow that grammatical features are at first learned in a maximally simplified environment. Later on, the same features continue to function in more complicated environments as abstract and interacting factors. It must be an important formal property of natural grammars to have this build-in hierarchic learnability for the grammatical distinctions.

Dresher (1999) has made a simple, but now debatable, objection against UG features and their parametric form. He argued that the UG properties were too abstract and interacting to offer a reliable guidance to an acquisition procedure. In a sense his objection was a rephrasing of Chomsky's argument about the poverty of the stimulus. Yet, such objections, including the argument from the poverty of the stimulus, need no longer hold. The acquisition model proposed above made no direct use of UG features as such. It worked the other way

around. The input patterns simplified by reduction impose such features on the learner. Once acquired, these features are stored in the lexical memory. That is, they are added to the various lexical items as context features. Fortunately, this property of grammatical context is already known as Chomsky's (1995) Inclusiveness Principle. Each time the lexicon is consulted, the (invariably) local context properties are bound to get deployed. The natural consequence is that early acquisition steps must have typological significance. They have established themselves in the lexicon and from there they control further properties. This was clearly seen by Jakobson (1942). He predicted typological significance and a more stable status for features acquired early, whereas features acquired later on were expected to show less stability in history and dialects. Jakobson's view translates easily in an acquisition difference between major parameters and microparameters. The actual discovery of such acquisition differences and their derivation from evidence frames is still to be made, but to my mind we know now where to look. Let me finally turn to the question whether a construct so much designed for diversity and learnability as grammar, must nevertheless be based on innate biologically given frames. As you may expect, I will answer this question in the negative.

3.2.   *The Perfect Language*

Chomsky (2005) assumes three sets of determinants for the acquisition of grammar: (A) general cognitive abilities, (B) innate UG distinctions, and (C) input sentences. He considers the possibility that the determinants in (B) can be minimalized. Minimal assumptions one must make about any combinatorial system would suffice to derive a grammar by means of (A) from input (C). A language controlled by such a grammar is called 'perfect'. It will not need the evolution of pre-wired task-specific neurology. He introduces a distinction, though, between a language faculty in the broad sense (FLB) and a language faculty in the narrow sense (FLN; Hauser, Chomsky & Fitch 2002). I interpret this in the following way. The language faculty in the narrow sense may in principle contain all pieces of grammatical furniture recommended as useful devices in generative grammars, {the system of categories, grammatical relations, binary parameters, projection of labels, locality, binarity, recursion, selectional hierarchies of adverbs and auxiliaries, case systems, chains, movements and their triggers, phi-features, agreement, pronouns, islands, binding principles}.

    If elementary acquisition principles were to derive all these distinctions from input properties only, set (B) gets empty and grammar becomes 'perfect' in the sense of the minimalist program (Chomsky 2005), as advanced in section 2 above or in Evers & van Kampen (2008) and van Kampen (2009). It is revealed as a learnable cultural construct and having no biological determinant (cf. Koster 2009). If by contrast, it turns out that grammar is not perfect in the above (minimalist) sense, then it will require pre-wired innate task-specific neural constructs to acquire language. Then language is unlike the traffic system, a ballet choreography, or the stock market. Then, it is indeed the quirky offshoot from an autonomous innate neural construct and the biolinguistic program is in business. This is not to deny that the combinatorial use of words is a novelty

called 'grammar'. The novelty may emerge from a special neural organ, but it is not necessary to make such a drastic assumption. As a matter of fact, pragmatic and associative relations between content words are present in early pre-grammatical child language, when each content word is used as a separate utterance. These relations between single-word utterances may give way to a set of relations (argument structure, event structure, illocutional structure) applied in a standard way. It seems not unlikely that such standardization of word-word relations may be a natural outcome.

The acquisition analysis above suggests that the acquisition model can be aimed at analyzing language as perfect in the sense of it being a cultural (socially transmitted and evolved) learnable construct, rather than a biological (genetically given) frame. There is no denial of a neural faculty of language in the broad sense (FLB). My skepticism against biolinguistics is only directed at the faculty of language in the narrow sense (FLN). This is not yet a common stance among generative grammarians. Some of them consider it even the hallmark of the generative enterprise that the study of grammar should postulate an innate task-specific neural complex. None of the usual arguments seem to me convincing or even relevant. I will shortly review them as recently brought up in Piattelli–Palmarini (2008). Thereafter I will turn to the nature of the faculty of language in the broad sense.

Piattelli–Palmarini (2008) protests against the idea that grammar might be a cultural construct that caused as a secondary effect the evolutionary enlargement of the human brain, a view developed in Deacon (1997). Linguistic inquiry, Piattelli–Palmarini argues, has shown all kind of unexpected consequences and curious restrictions in grammar. This suggests, he feels, a biological source for grammatical distinctions. I do not see that point. Unexpected consequences and curious restrictions hold for any complex system, whether biological or cultural. As far as cultural constructs are concerned, one may think of the riddles in number theory. Piattelli–Palmarini (2008) is also in favor of a biological origin for grammatical distinctions because children are said to acquire language 'easily'. I doubt that as an argument for the biological status of the construct. It rather seems that young children are unbelievably vigorous learners in all kind of physical, social or cultural competences. Within months six year olds get the basic competences for reading, writing, drawing, counting, biking, playing the piano, swimming, knitting, tying ones shoes, and a variety of social games. Acquiring a language is but a bit different. It is an extensive device. Learning must begin earlier and will take longer, but the same prolific flexibility for learning is in action. Children start small and often one sees their short but considerable concentration. As for language acquisition, they remain engaged for years in a round-the-clock training with strong and immediate rewards. Relative ease in language acquisition may be no more than the impression of a somewhat distracted father. Another point that Piattelli-Palmarini brings up refers to the spontaneity of grammatical reactions. I do not get that point either. Reactions by trained participants in chess, soccer or music have to be immediate and spontaneous as well. That is the fun they yield. On the other hand, carefully wording a letter is the opposite of rambling off. It is true that verbal reactions from the top of one's head still fit the rules of grammar, but that holds no less for

whatever rule-governed behavior. In general, conscious training in cultural constructs would not take place if it did not have such clear and selectional consequences. Grammatically governed achievements are no exception as is daily demonstrated in the school system and in society at large. And if this holds for the finer points of lexicon and grammar, why not for all points? And if this holds in history, why not in pre-history?

When grammatical systems seem designed for learnability and UG distinctions seem learnable by a few elementary steps that have a minimalist orientation, as advanced in section 2, one need not postulate a task-specific and innately pre-wired neural system to offer the learner possible frames for grammar. The fully learnable grammar as a cultural construct is on a par with other constructs and inventions that human beings employ in order to survive, such as ways of gathering food, weapons and shelter, constructing tools to get tools, and presserving fire. When clans or tribes in completely different parts of the world show far reaching parallels between their cultural devices, from fishing gear to grammatical devices, this proves that these devices are parallel solutions to parallel problems irrespective of postulations about innateness.

The emergence of grammar must be dependent upon an environment that invites the frequent use of content words and the inventive flexibility of a young brain. Both factors are relevant anyway. One may of course postulate additional factors, such as genetically innate parameters of grammar, but these must remain speculation. The major conclusion appears less speculative and more promising for advanced research in child language: Grammar is to be analyzed as fully learnable. Its intricacies should in the first place be explained by paying more attention to the stepwise procedure that is present in child language itself.

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Evolution, Perfection, and Theories of Language

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In this article it is argued that evolutionary plausibility must be made an important constraining factor when building theories of language. Recent suggestions that presume that language is necessarily a perfect or optimal system are at odds with this position, evolutionary theory showing us that evolution is a meliorizing agent often producing imperfect solutions. Perfection of the linguistic system is something that must be demonstrated, rather than presumed. Empirically, examples of imperfection are found not only in nature and in human cognition, but also in language — in the form of ambiguity, redundancy, irregularity, movement, locality conditions, and extra-grammatical idioms. Here it is argued that language is neither perfect nor optimal, and shown how theories of language which place these properties at their core run into both conceptual and empirical problems.

1.   Introduction

Linguistic theory is inevitably underdetermined by data. Whether one is trying to characterize the distribution of *wh*-questions across languages or account for the relation between active sentences and passive sentences, there are often many distinct accounts, and linguistic data alone is rarely absolutely decisive. For this reason, theorists often appeal to external considerations, such as learnability criteria (Gold 1967, Wexler & Culicover 1980), psycholinguistic data (Schönefeld 2001), and facts about the nature and time course of language acquisition (e.g., the accounts presented in Ritchie & Bhatia 1998). There is also a move afoot to constrain linguistic theory by appeal to considerations of neurological plausibility (Hickok & Poeppel 2004, Marcus, in press). And there is a long-standing history of constraining linguistic theory by appealing to considerations of cross-

linguistic variation (Greenberg 1963, Chomsky 1981a, Baker 2002). Here, we consider a different sort of potential biological constraint on the nature of linguistic theory: Evolvability.

Constructing a theory which says that language is evolvable involves looking at what we know from evolutionary biology about what typically evolving systems look like, what kinds of properties they have, and then applying this to questions about the plausible nature of language. Here, our focus will be on the plausibility of recent suggestions (e.g., Chomsky 1998, 2002a, 2002b, Roberts 2000, Lasnik 2002, Piattelli–Palmarini & Uriagereka 2004, Boeckx 2006) that language may be an 'optimal' or near-optimal solution to mapping between sound and meaning — a premise that has significant impact on recent developments in linguistic theory.

In what follows, we will argue that the presumption that language[1] is optimal or near-optimal is biologically implausible, and at odds with several streams of empirical data. We begin with some background in evolutionary theory.

2.   Evolution, Optimality, and Imperfection

Our analysis begins with a simple observation: Although evolution sometimes yields spectacular results, it also sometimes produces remarkably inefficient or inelegant systems. Whereas the Darwinian phrase (actually due to Huxley rather than Darwin) of "survival of the fittest" sometimes is misunderstood as implying that perfection or optimality is the inevitable product of evolution; in reality, evolution is a blind process, with absolutely no guarantee of perfection.
To appreciate why this is the case, it helps to think of natural selection in terms of a common metaphor: as a process of hill-climbing. A fitness landscape symbolizes the space of possible phenotypes that could emerge in the organism. Peaks in the landscape stand for phenotypes with higher fitness, troughs represent phenotypes with lower fitness. Evolution is then understood as the process of traversing the landscape. Our focus in the current article is on a limitation in that hill-climbing process, and on how that limitation reflects back upon a prominent strand of linguistic theorizing. The limitation is this: Because evolution is a blind process (Dawkins 1986), it is vulnerable to what engineers call the problem of local maxima. A local maximum is a peak that is higher than any of its immediate neighbors, but still lower (possibly considerably lower) than the highest point in the landscape.

In the popular "fitness landscape" terminology of Sewall Wright (1932), the perfect solution and the optimal solution to a given problem posed by the

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[1]   The term 'language' itself is of course intrinsically ambiguous; the term can, among other things, refer to the expressions in a particular language, to the underlying cognitive system itself, to its biological and neurological manifestation, or to a formal model of the system. Here, our discussion pertains primarily to the latter (although the former two will be mentioned from time to time); that is, what is often referred to as the human language faculty, which is formally modeled, as a grammar, in different ways by different linguistic theories.

organism's environment can (and often do) differ in their location. While perfection holds only of the highest peak, lower peaks in the landscape may in some circumstances be optimal. But, in the words of Simon (1984), natural selection does not even necessarily seek optimality. Rather, evolution essentially serves as it were a satisficing agent; rather than inevitably converging on the best solution in some particular circumstance, it may converge on some other reasonable if less than optimal solution to the problem at hand.

Perhaps the most accurate phraseology is that of Dawkins (1982) who uses the term 'meliorizing', which captures the fact that evolution is constantly testing for improvements in the system, but not explicitly guided to any particular target and by no means guaranteed to converge on perfection or even optimality. Perfection is possible, but not something that can be presumed.

2.1. Imperfections in Nature

In the real world, evolution sometimes achieves perfection or near-optimality, as in the efficiency of locomotion (Bejan & Marden 2006), but has in many instances fallen short of any reasonable ideal. The mammalian recurrent laryngeal nerve, for example, is remarkably inelegant and inefficient, following a needlessly circuitous route from brain to larynx posterior to the aorta. While in humans, this may not add up to a significant amount of extra nerve material, in giraffes it is estimated to be almost twenty feet (Smith 2001). The problem here is one of what Marcus (2008) calls evolutionary inertia — the tendency of evolution to build new systems through small modifications of older systems, even when a fresh redesign might have worked better.

The human spine is similarly badly designed (Krogman 1951, Marcus 2008). Its job is to support the load of an upright bipedal animal, yet a much better solution to this problem would be to distribute our weight across a number of columns, rather than let a single column carry it all. As a result of the spine's less than perfect design, back pain is common in our species. Here again, evolutionary inertia is the culprit — the human spine inherits its architecture, with minor modification, from our quadrupedal ancestors, even though a single column works better in bearing horizontal loads than it does in bearing vertical loads. Although a sensible engineer could have anticipated the ensuing problems, the blind process of evolution could not.

Another illustration of the friction that derives from evolutionary inertia is the human appendix, an example of what is known as a vestige. This is a different type of imperfection, an example of a structure that has no current place in the organism at all. Its existence does not seem to increase our fitness in any way, and its poor structure can lead to blockages which cause sometimes fatal infection (Theobald 2003). The appendix was an earlier adaptation for digestion of plants in our ancestors, now not required by non-herbivorous humans. Although we might have been better off without an appendix and the ensuing risk of infection, evolution lacks the capacity to anticipate; because of the architecture of evolutionary inertia we are stuck with the risks despite a lack of corresponding benefits. (Yet another example comes from human wisdom teeth, which are imperfect due to the problem of fit that our larger third molars pose for

our modern jaws. Our ancestors had larger jaws that comfortably accommodated the larger wisdom teeth, but cumulative gradual adaptive evolution has decreased our jaw size over time, resulting in pain on eruption, and impacting of the wisdom teeth.)

2.2.   *Imperfections in Human Cognition*

In human cognition too, imperfection arising from gradual adaptive evolutionary processes seems common. Human memory, for instance, is far from perfect (Marcus 2008). It can be easily distorted by environmental factors, and we often blur together memories of similar events, remembering the general but not the specific. For example, we may remember some fact we read, but not where we read it. Furthermore, our memories can be tested, and often distorted in stressful circumstances, such as under the questioning in a courtroom. Marcus argues that location-addressable memory, such as computers have, would be much more useful to modern humans, but we are the result of gradual cumulative evolution from ancestors who dealt in the here-and-now, where context-dependent memory was a good enough tool. Once more, evolution did not have the foresight to bestow on us the kind of memory that would be a better solution to problems faced by modern humans.

Human belief too, shows evidence of imperfect design (Marcus 2008). Our beliefs are also subject to biasing or warping. Although we may believe that we reason objectively, this is often not the case. Context, emotion, and unconscious biases, such as what we are familiar with, or the confirmation bias, can all warp our beliefs. Again, this imperfection is the result of cumulative evolution from an ancestor that needed to act, but not often to think or reason, evolution once again lacking the foresight required to know that reasoning objectively and logically would be more useful to us.

3.   Is Language Different?

If all this is taken for granted in biology, it is not taken for granted in linguistics. To the contrary, in recent years it has become popular to assume that language may well be perfect, or nearly so. Chomsky (2002a: 93) has argued that "language design may really be optimal in some respects, approaching a 'perfect solution' to minimal design specifications"; similarly, Roberts (2000), for example, has argued that language may be a computationally perfect system for creating mappings from signal to meaning.

Could language be different, more perfect than other aspects of biology? Since the balance of perfection and imperfection could vary between domains, we see this as a fundamentally empirical question. Since imperfection exists, it seems unreasonable to simply presume linguistic perfection, but near-perfection exists, too, as in the primate retina's exquisite sensitivity to light (Baylor *et al.* 1979).

That said, *a priori* it would be surprising if language were better designed than other systems, for the simple reason that language is, in evolutionary terms,

an extremely recent innovation. By most recent estimates, language emerged only within the last 100,000 years (Klein & Edgar 2002), and as such there has been relatively little time for debugging.

3.1.   *Imperfections and Inefficiencies in Language: Some Empirical Evidence*

At least superficially, instances of imperfection seem plentiful in language, most notably in all manner of speech errors, such as the phonological slip in *written a splendid support* (instead of *written a splendid report*), the lexical slip in *a fifty pound dog of bag food* (instead of *a fifty pound bag of dog food*) (from Fromkin's Speech Error Database), or the Spoonerism (attributed to Reverend Spooner himself) in *You have hissed all the mystery lectures* (instead of *You have missed all the history lectures*). According to the taxonomy of Dell (1995), there are at least 5 distinct types of speech error (exchanges, shifts, anticipations, perseverations and substitutions), which can apply at some 10 different linguistic levels (from sentence through word, morpheme, syllable and phoneme, to feature). Frequencies of occurrence are as high as 1–2 per thousand words.[2]

Similarly, people frequently misparse passives with non-canonical relations (e.g., reading *man bites dog* as if it were *dog bites man*, Ferreira 2003) and interpreting sentences in ways that are internally consistent. For example, subjects often infer from the garden-path sentence *While Anna dressed the baby slept* both that the baby slept (consistent with a proper parse) and that Anna dressed the baby (inconsistent with what one would expect to be the final parse, Christianson *et al.* 2001). Likewise, they are vulnerable to "linguistic illusions", such as the belief that *More people have been to Russia than I have* is a well-formed sentence, when it is in fact not.

Still, such errors do not necessarily bear on more architectural questions about the nature of grammar, *per se*; they might be seen as purely a matter of performance. What of competence grammar? Here, too, we will suggest, rumors of linguistic perfection are exaggerated.

3.1.1. *Redundancy*

Turning to competence, and the core syntactic system, a first type of imperfection comes under the heading of redundancy. We will define redundancy as the ability of more than one structure or (sub-)system to carry out the same function. Redundancy therefore entails duplication or inefficiency in a system. A perfectly designed system would surely eschew what is not just clumsy, but may also be more costly, requiring instead a system that is streamlined and efficient.

Yet language is replete with redundancy, not just in the occasional genuine synonym (*couch* and *sofa*) but also in more subtle areas such as case marking. The language faculty makes available two possible manners of marking case on a noun — by imposing strict word order constraints, or with the use of inflectional

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[2]   This measure holds for English, based on an analysis of the London–Lund corpus (Garnham *et al.* 1981), but there is no reason to think that it differs greatly cross-linguistically (Dell 1995).

morphology. Languages like English mostly make use of the former strategy, and languages like Russian typically use the latter. Either would suffice, but from a sheer elegance perspective, it is somewhat surprising that human languages fail to adopt a consistent solution. Meanwhile, languages like German show that *both* strategies can be used concurrently — in a highly redundant fashion. In (1a), the inflectional morphology on subject and object differs. This contrasts with (1b), where the definite article for feminine nouns does not differ in form from nominative to accusative case:

(1) a.   Der   Hund beisst   den   Mann.   *German*
          the.NOM dog   bites   the.ACC man
          'The dog bites the man.'
    b.   Die   Katze beisst   die   Frau.
          the.NOM cat   bites   the.ACC woman
          'The cat bites the woman.'

While in (1b), only word order can signal case, in (1a) both inflectional morphology and word order signal case. We know here that word order is playing a part in (1a), and it is not simply the case that the morphology does all the signaling, because SVO is the default order in German main clauses, if the opposite order is used, as in (2), intonational differences show this as somehow marked.

(2) Den   Mann beisst der   Hund.   *German*
    the.ACC man   bites the.NOM dog
    'The dog bites the man.'

A second instance of redundancy is seen in person and number morphology. It is very often the case that a language will redundantly mark person and/or number on more than one element in a phrase or sentence. In English, for example, we get cases like (3), where every single word in the sentence is marked in some way for plurality.

(3)   Those four people are teachers.

What is remarkable about this is how easily in principle it could avoided: Mathematical and computer languages lack these sorts of redundancies altogether.

Redundancy can of course be adaptive. It benefits humans to have two kidneys, and it benefits birds to have excess flight feathers (King & McLelland 1984). In a similar way, synonyms might be argued to be adaptive due to the advantage they confer when retrieval of a particular lexical item fails. Or, it might be argued that in a noisy channel, redundantly specifying some parts of the code would lead to increased communicative success. Perhaps, then, examples like this should not be thought of as imperfections. However, the redundancies we in fact observe appear too arbitrary and unsystematic to be explained strictly in terms of their benefits towards communicating relative to noise in the communi-

cation channel, especially in comparison to the more systematic techniques one finds in digital communication. The parity system, for example, that modems use – making the 8th bit a 1 ('odd parity') if the number of '1's in the first seven bits is itself odd, otherwise zero — is systematically applied to every byte in a stream; redundancies in language are frequently far less systematic. Plurality is marked in some instances but not others, for example. Patterns of syncretism often keep redundancies themselves from being systematic. Furthermore, the existence in natural languages of redundancies that have no apparent advantage — where artificial languages lack them — undermines the case that language is maximally elegant or economical, and emphasizes the extent to which the details of grammar are often imperfect hotchpotches.

In fact, a case of the very opposite of what is here defined as redundancy gives us a further imperfection in language. If redundancy involves multiple structures carrying out the same function, the doubling or tripling of function that is seen in syncretic forms such as the past and passive participles in English, or nominative and vocative case morphology on certain classes of nouns in Latin (Baerman *et al.* 2005), leads to imperfection in the form of a lack of clarity. Differing functions being fulfilled by identical structures might be considered optimal or perfect under an interpretation appealing to efficiency or simplicity, yet taken to extremes the system that emerges is far from usable.

3.1.2. Ambiguity

Ambiguity, both lexical and syntactic, provides another type of imperfection present in natural language, but not in formal languages.[3] Lexical ambiguity comes in the form of homonymy, for example, *bear* as an animal versus *bear* as a verb of carrying, and polysemy (which differs from homonymy in that the meanings of the multiple lexical items that sound alike are connected in some way), for example, *mouth* of a river, or of a person, *wood* as a part of a tree, or as an area where many trees are growing. In both cases, the signal on its own is not enough to pick out a meaning. The use of a lexically ambiguous word requires the listener to take the immediate context and his world knowledge into account in order to correctly assign a meaning to the speaker's utterance, thus making the process inherently less efficient than it would be given a non-ambiguous system.

If the syntactic component of the grammar is understood as responsible for creating a mapping between signal and meaning, the most natural manner in which it would do this is to map a single unique signal to a single unique meaning. Syntactic ambiguities can be looked at as violations of this intuitively elegant system of one–to–one mapping.[4] In syntactic ambiguities, single signals

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[3]   One possible counterexample that has been suggested to us is the operator '=', which in some computer languages functions as both an assigner and a comparison operator. However, it is interesting to note both that this particular ambiguity in programming languages is parasitic on a lexical ambiguity in natural language, and that it has been readily resolved in many more modern programming languages, simply by assigning distinct operators to equals and assignment.

[4]   Following Higginbotham (1985), it is possible that ambiguities such as in (4) and (5) stem from sets of sentences that are effectively akin to homonyms, sounding alike but having distinct meanings. However, such an analysis does not eliminate the issue of ambiguity, it

are mapped to *multiple* meanings. In (4a), for example, the signal maps equally to two meanings, (i) where I use green binoculars to see the girl, and (ii) where I see the girl who has a pair of green binoculars. The signal in (4b) maps to four meanings, (i) where I stand on the mountain and use green binoculars to see the girl, (ii) where I use green binoculars to see the girl who comes from the mountain, (iii) where I stand on the mountain and see the girl who has a pair of green binoculars, and (iv) where I see the girl who is from the mountain who has a pair of green binoculars. In (5), syntactic ambiguity results from elision, mapping the signal to two possible meanings, (i) where John saw a friend of John's and Bill also saw a friend of John's, and (ii) where John saw a friend of John's and Bill saw a friend of Bill's.

(4)   a.   I saw the girl with green binoculars.
       b.   I saw the girl with green binoculars from the mountain.

(5)   John saw a friend of his and Bill did too.

To be sure, ambiguity can be used by the speaker intentionally to create vagueness. For example, when, in the context of a job reference, I say *I can't recommend this person enough*, I am being deliberately evasive. In addition, there are cases of syntactic ambiguities too that can be resolved by context. But even when both deliberate and immediately resolvable ambiguities are factored out, a considerable amount of unintended — yet in principle unnecessary — ambiguity remains (e.g., Keysar & Henley 2002).

3.1.3. Irregularity

Languages also deviate from elegance and simplicity in the widespread existence of linguistic irregularity, both lexical (morphological) and syntactic. If language were perfect, then we would expect that it should be fully regular and systematic, as all formal languages are. In natural language, mappings between sound and meaning are created in inconsistent, almost messy ways.

Morphological paradigms are the most obvious case of irregularity in language — the verbal paradigm for the verb *to be* in many languages, or the formation of plural nouns in English — but this imperfection can also be seen in other areas of the grammar. Syntactic irregularity is found in extra-grammatical idioms (Fillmore *et al.* 1988) like *by and large, all of a sudden*, and *so far so good*, where lexical items are combined in a way completely unpredictable by the grammar of the language in question. For example, there is no rule in the grammar of English that permits the conjunction of a preposition like *by* with an adjective like *large*. Nor is there any rule in the grammar of English that says two adjective phrases (*so far, so good*) can be concatenated. Such irregularity has no counterpart in synthetic languages, and forces the parser to do more work than is strictly necessary (e.g., in determining whether input strings are to be interpreted compositionally or idiomatically).

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merely re-locates it, and still requires the listener to make mappings from surface strings to underlying meanings that are not one-to-one and not specified by the grammar.

3.1.4. Needless Complexity

A fourth class of imperfection in language concerns intricacies that the linguistic system could function without. The first example of this type of needless complexity concerns the form and interpretation of sentences like (6):

(6)   Who did John meet?

Here, the object of the meeting event is questioned by placing the lexical item *who* at the start of the sentence. However, we interpret *who* at the end of the sentence, as belonging after the verb *meet.* Linguistic theories which assume a derivational approach to language posit an operation in the grammar which permits elements to be displaced from one position to another. Chomsky (2002b) argues that movement is motivated by the need to distinguish between the deep semantics of argument structure and the surface semantics of discourse structure. So, *who* is an argument of *meet*, but the fact that (6) is a question is signaled by moving the *wh*-word to the beginning. However, movement is not necessary here as this kind of distinction can be made in other ways. Intonation can mark surface semantics — in fact, English topic/comment and focus semantics are much more frequently marked intonationally than by syntactic movement. Another option is to use morphological markers, like Japanese *wa*. The cases here are specific, but the point can be generalized — if there exist languages that do not require movement to make the distinction between deep and surface semantics, then why does the language faculty need to make this operation available at all? In some eyes, movement may be a more elegant way of signaling this semantic distinction than, say stacks or special features, but a system lacking any of these is more elegant still.

Operations such as movement that are part of language competence are constrained by locality conditions. This means that it is not permissible to apply linguistic operations just anywhere, but that they are constrained to apply within limited structural domains. For example, (7a) is more acceptable than (7b) because the *wh*-phrase in the initial position of the sentence has moved a relatively short step in (7a) (from after *persuade*), but in (7b) has moved a step longer than is permitted (from after *visit*).

(7)   a.   Who did John persuade to visit who?
        b.   *Who did John persuade who to visit?

These too are absent in formal languages and seem to add needless complexity. Locality conditions force the learner to execute extra computation in that he must figure out for his language where the boundaries that divide what is local from what is not lie. A linguistic system designed with efficiency and economy as its central concern would minimize the work the learner must undertake. The question then is why movement and constraints on locality exist. One possibility is that if our linguistic representations are subject to the limitations of the type of memory we have inherited from our ancestors (Marcus, in press) locality conditions allow us to process complex linguistic expressions in

the fragmented pieces we are capable of dealing with. What is an imperfection by the measure of efficiency and economy can be explained by our evolutionary history. Language is imperfect and messy because evolution is imperfect and messy.

4.   If Language Is Not Perfect, Might It Be Optimal?

The examples presented in the previous section strongly suggest that, empirically, the human language faculty fails to meet the strict criterion of perfection, but they still leave open a weaker possibility. Could language be seen as some sort of optimal tradeoff? Although perfection and optimality are often conflated in discussions of this issue in the literature, the two notions are certainly conceptually distinct. Perfection entails an absolute, the best in all possible circumstances, while optimality entails points on a gradient scale, each of which can only be reached by overcoming some limitations, and thus is the best in some specific circumstances only. As Pinker & Jackendoff (2005: 27) note, "nothing is 'perfect' or 'optimal' across the board but only with respect to some desideratum".

The immediate question, then, is: "Is there any criterion by which language could be considered to be optimal?" A number of criteria spring immediately to mind: ease of production, ease of comprehension, ease of acquisition, efficient brain storage, efficient communication, efficient information encoding, and minimization of energetic costs. Let us consider each in turn.

First, one could imagine that language might be optimal from the perspective of speakers, minimizing costs for producing expressions. In reality, however, this criterion is not always met. In cases of morphological redundancy, such as that seen in person and number morphology mentioned above, where the speaker has to produce this type of inflection on multiple (in some cases every) lexical items in one sentence, the computational costs for the speaker rise considerably. In question formation, the speaker is forced to calculate locality conditions to ensure a *wh*-phrase is not uttered in an illegitimate position in the sentence, again a case of increased computational load.

What of optimality from the opposite perspective? If production costs are higher than strictly necessary, is this because comprehension costs are kept low? Could language be optimal from the hearer's perspective, allowing speakers' utterances to be interpreted easily? Here again, the answer seems to be no. Both lexical and syntactic ambiguity lead to increased complexity for the hearer. Additional computation must be undertaken in order to select the correct interpretation of a number of possibilities. Movement also causes difficulties for comprehension, because resolving filler-gap dependencies can be costly, especially when they are not signaled in advance (Gibson 1998, Wagers 2008).

Is it then language acquisition that drives the system to be optimal? Are comprehension and production complicated because the crucial consideration is that the system must be easily learnable? Here again, the answer appears to be no. Ambiguity (both lexical and syntactic), extra-grammatical idioms, and movement, for example, all complicate acquisition, because one–to–one mapping

between signal and meaning is upset, because rules of the grammar are not consistently followed, and because filler-gap relations must be mastered.

Could language be optimal because it is stored in the brain in the most efficient manner possible? Again, probably not: Morphological irregularity and idioms belie this criterion too. Storage is inefficient in cases where each entry in a verbal paradigm constitutes a separate entry. With idiomatic expressions, the number of entries in the lexicon grows even further.

A fifth criterion suggests that language might be considered optimal if communication between speaker and hearer were as efficient as possible. Yet again, this criterion can be discounted when we consider ambiguity. Both lexical and syntactic ambiguity can lead to communication breakdown, and the subsequent need for speakers to make corrections or amendments.

Another possible measure of optimality might be in terms of the amount of code that needs to be transmitted between speaker and hearer for a given message that is to be transmitted. It is not obvious how to explicitly measure this, given the complexities of human communication (what counts as the message that it is to be transmitted), but this proposal too seems to run headlong into the sort of imperfections seen above (ambiguity, movement, redundancy, etc.).

It turns out, then, there is — despite numerous proposals — no obvious desideratum by which language can plausibly be said to be optimal.

A true devotee of the notion of language as optimal solution could of course turn to combinations of criteria, for example, could language be a system that yields an optimal balance between ease of comprehension and ease of acquisition? It is possible, but here too we are skeptical. With no *a priori* commitment to which combinations might be optimized, and no specific account for why some of these criteria but not others might be optimized, the advocate of linguistic optimality risks getting mired in a considerable thicket of *post hoc* justification. It is easy to see in broad outline how natural selection might have favored a system that rewards each of these properties, but there is little predictive power; there is no reason from these as first principles, for example, to predict that natural languages would (or would not) have locality conditions. Formal languages lack them, they complicate acquisition, and inasmuch as extra entities such as bounded nodes need to be computed, they presumably also complicate comprehension. Imperfections such as morphological redundancy could be seen as optimizing ease of comprehension, but imperfections like syntactic ambiguity and movement operations do the opposite; imperfections like syncretism and lexical ambiguity arguably reduce demands on long-term memory (inasmuch as they demand a smaller number of lexical entries) but considerably complicate comprehension, and deviate from a kind of elegant one-to-one mapping principle that is found in formal languages. Taken together, the five criteria yield a very weak stew; there is no clear prediction from first principles of what a language should be like, only (see Table 1) a set of inconsistent and largely *post hoc* attributions, with no genuine explanatory force.

Quirk of language	Consequences	Alleged optimization
lexical ambiguity	complicates comprehension; complicates acquisition	reduces number of lexical entries
syntactic ambiguity	complicates comprehension; complicates acquisition	reduces number of constructions
morphological irregularity	reduces storage efficiency	
extra-grammatical idioms	complicates comprehension; complicates acquisition; reduces storage efficiency	increases creativity
morphological redundancy	complicates production	simplifies comprehension; simplifies acquisition
movement	complicates comprehension; complicates acquisition	fits more naturally with information structure
locality conditions	complicates comprehension; complicates acquisition	

Table 1: Quirks of language and the lack of optimization in language

In reality some quirks of language may have more to do with history than optimal function (Marcus 2008). Our susceptibility to tongue-twisters, for example, may come from the evolutionary inertia (Goldstein *et al.* 2007, Marcus 2008) inherent in repurposing an ill-suited timing system to the purposes of speech production, rather than any intrinsic virtues. Similarly, locality conditions may exist as an accommodation to an underlying memory substrate that is poorly suited to language (Marcus, in press) rather than as a solution that could be considered optimal from any design-theoretic criteria.

5.   The Minimalist Program and Perfectionism

Talk of language and its apparent imperfections takes on special significance in light of its role in the formulation of one linguistic theory that has been prominent in recent years — the Minimalist Program, as introduced by Chomsky (1995). Here, a presumption of linguistic perfection (or near-perfection) is central, with Chomsky (2004: 385) suggesting that language may come close "to what some super-engineer would construct, given the conditions that the language faculty must satisfy". Roberts (2000: 851) has gone so far as to suggest the Minimalist Program's assumption that language is a computationally perfect system for creating mappings between signal and meaning "arguably represent[s] a potential paradigm shift" in Generative Grammar.

5.1.   Vagueness

5.1.1. Optimality versus Perfection

The first issue is that the difference between optimality and perfection is never clarified in the minimalist literature. At the end of the 1990s, Chomsky (1998: 119) claims that "language is surprisingly 'perfect'". Yet only a few years later, he states that "[t]he substantive thesis is that language design may really be optimal in some respects, approaching a 'perfect solution' to minimal design specifications" (Chomsky 2002a: 1993), and then, just a page later in the same publication, he says that "[t]he strongest minimalist thesis would be this: […] Language is an optimal solution to legibility conditions". Nowhere are perfection and optimality teased apart in this literature, yet as was hinted at in section 2, these terms should be applied in significantly different cases.

5.1.2. Optimal for What?

Inasmuch as the Minimalist Program is tied to the notion of optimality, it is immediately vulnerable to all the concerns outlined in section 3 above, to wit, unless there is some clear, *a priori* criterion for optimality, claims of optimality have little force. As Lappin *et al.* (2000) and Wasow (2002) have noted, Chomsky himself is not particularly clear about his criteria. One could imagine that minimalism might seek optimality in terms of a linguistic architecture that minimized energetic costs, and reduced computational load, but advocates of minimalism have never been particularly clear about the criteria.

As Lappin *et al.* (2000) note, if language were optimal in terms of computational simplicity, it would require the minimum amount of computational operations and apparatus; it would not exceed the computational requirements of any artificial system that could be created to undertake the same job. Given the presence of redundancy, movement, locality conditions, and other imperfections discussed above, this possibility seems like a non-starter. Computational simplicity is further compromised by the kinds of "economy conditions" (see below) assumed in minimalist analyses, which require that all possible outputs given the lexical items inputted be computed and compared in order to determine the most economic option (Johnson & Lappin 1997).

The minimalist position similarly cannot be rescued by appealing to the more modest criterion of optimal compromise examined in section 3. No compelling reasoning has been presented in the literature to illustrate the pertinent criteria for which language is considered optimal, and how the conflict between these is reconciled by the properties the linguistic system shows.

5.1.3. Optimality and Economy

In the minimalist literature, optimality (or perfection) seems most often to be equated with "economy", and with the related suggestion that all properties of

language might derive from *virtual conceptual necessity*,[5] a term glossed by Boeckx (2006: 4) as "the most basic assumptions/axioms everyone has to make when they begin to investigate language".[6]

In one respect, this notion is admirable (if unsurprising): Linguistic theorizing, like all scientific theorizing, should be guided by considerations of parsimony. If two theories cover some set of data equally well, but one does it with fewer stipulations or fewer parameters, we should, other things being equal, choose the "simpler theory".

But researchers under the minimalist umbrella often seem to take parsimony a step further, and suggest that independently of the character of the linguistic data, a theory with few principles or representational formats is to be favored over a theory with more principles or representational formats. For example, the Minimalist Program reduces the levels of representation to just two — Phonological Form (PF) and Logical Form (LF), arguing that "virtual conceptual necessity demands that only those levels that are necessary for relating sound/sign and meaning be assumed" (Boeckx 2006: 75) — where previous theories also posit Deep Structure (DS) and Surface Structure (SS). In our view, such assumptions are risky. To paraphrase Einstein, a theory ought to have as few representational formats as possible, but not fewer; the correct number of levels of representations could well be one or two, but it could be three or four or even ten or twenty; this is simply a matter for empirical investigation. For example, research in autosegmental phonology suggests that multiple levels (or tiers) of representation are required to account for processes such as tone (Goldsmith 1976); one would not want to revert to a single level account simply because fewer levels are superficially simpler or more economical.

A second type of economy lurks behind the first: An assumption that linguistic competence is in some significant fashion mediated by something akin to energetic costs. Economy of this sort is reflected in the types of economy considerations that have been employed since the earliest times of Generative

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[5]   For a critique of the coherence of the very notion of *virtual conceptual necessity*, see Postal (2003).

[6]   Unfortunately, there is no clear consensus about what such assumptions might be. On the restrictive side, virtual conceptual necessity might consist of little more than a requirement that sound be connected to meaning (Chomsky 1995, Boeckx 2006), with other properties, for example, binary branching, derived rather than stipulated as necessities. On the less restrictive side, however, even puzzling properties such as "displacement" (movement), which hardly seem logically necessary, are also included, as in Boeckx's (2006: 73) suggestion: "Chomsky (1993) remarked that one way of making the minimalist program concrete is to start off with the big facts we know about language […]. These are: (i) sentences are the basic linguistic units; (ii) sentences are pairings of sounds and meanings; (iii) sentences are potentially infinite; (iv) sentences are made up of phrases; (v) the diversity of languages are the result of interactions among principles and parameters; (vi) sentences exhibit displacement properties […]. Such big facts are, to the best of our understanding, essential, unavoidable features of human languages […]. They thus define a domain of virtual conceptual necessity". In our view, this broader formulation considerably weakens the explanatory force of virtual conceptual necessity. Although (i)–(iv) seem like plausible minimal requirements, (v) and (vi) seem to be empirical observations about human language, not logical requirements: hence properties that demand explanation, rather than mere stipulation.

Grammar (see review in Reuland 2000), as in Chomsky & Halle's (1968) evalu-
ation procedures for grammars. More recent minimalist versions include locality-
driven constraints such as Shortest Move, where a lexical item can be moved
from one position in a sentence to another only if there is no other position closer
to the lexical item that it could move into, and necessity-driven constraints such
as Last Resort, where a lexical item will be moved from one position to another
only if no other operation will result in grammaticality (Chomsky 1995).
Unfortunately, minimalism, as currently practiced, wavers considerably as to
what is allegedly being economized.

Consider, for example, the nature of the Spell-Out operation in later
versions of minimalism. Spell-Out is the operation that applies once all lexical
items in a lexical array have been combined through Merge and Move, sending
the semantic features of these lexical items to LF and the phonological features to
PF. In those formulations that follow Chomsky's (2001) *Derivation by phase*
architecture, Spell-Out operates not once at the end of a derivation, but multiple
times throughout it. Under this view, the derivation advances in stages or phases,
at each phase only a sub-set of the lexical array being visible. Once the items in
this sub-set have been combined, Spell-Out of this phase takes place. The
advantage that is put forward for such a system is the decrease in memory
requirements — the material that must be 'remembered' until the point of Spell-
Out is considerably less. Yet, a system that applies Spell-Out only once could be
argued to be advantageous in that the machinery for applying the operation is
invoked only once in the derivation. The question then becomes: Is it
computationally simpler (and hence more optimal) for the Spell-Out operation to
apply multiple times to small amounts of material, or only once but dealing with
a larger amount of material? Without a clear answer to this question, references
to economy become too evanescent to have real force.

A second case pertains to the operation of Agree. Agree allows for
uninterpretable features on lexical items to be checked and removed before Spell-
Out. In earlier versions of the theory (Chomsky 1995), Agree was permitted to
apply only to elements in a particular local relation to each other — a Specifier–
Head relation. Later, this stipulation was relaxed, allowing Agree to apply more
freely. An additional rule was then required in order that illicit Agree relations
could be ruled out (Chomsky 2001). While it might appear intuitively as if
permitting Agree to apply freely is a simpler, more optimal approach, the
question is whether the additional c-command rule that must be imposed negates
this. Is it computationally simpler (and hence more optimal) to apply Agree
freely and eliminate problem cases with an additional rule, or to restrict Agree
from the start to applying only in local domains? Once more, the Minimalist
Program offers nothing in the way of a discriminating measure.

Whether the type of economy measures that the Minimalist Program has in
mind are better defined as perfection or as optimality, we have shown that
neither is plausible for language. Taking this path leads the Minimalist Program
into two different kinds of problematic positions, which we will examine in the
following sections.

5.2. *Capturing the Facts of Language Leads to Abandoning Perfection*

Even if the notion of optimality could be tightened in order to give it more force, a more serious problem would remain: So far as we can tell, Minimalist theory cannot actually work unless it abandons the core presumption of perfection or optimality. Minimalism equates perfection with a type of bareness that derives from admitting only what is strictly necessary. But, as Newmeyer (2003: 588) puts it, practice rarely if ever meets that target; in his words, "no paper has ever been published within the general rubric of the minimalist program that does not propose some new UG principle or make some new stipulation about grammatical operations that does not follow from the bare structure of the MP". In actual practice, many of the mechanisms and operations that have been introduced into the system appear to be motivated not from virtual conceptual necessity, but rather from empirical realities that could *not* have been anticipated from conceptual necessity alone. For example, phases, movement, and constructions all seem to require additional machinery, and none have counterparts in formal languages. Capturing them seems inevitably to take the theory away from the perfection that is its ostensible target.

Consider (8a), and its Japanese counterpart in (8b):

(8) a.   What did John buy?   *English*

     b.   John-wa   nani-o   kaimasita ka?   *Japanese*
          John-TOP   what-ACC   buy   Q
          'What did John buy?'

What would be the simplest and most elegant way to capture the cross-linguistic facts illustrated in (8a) and (8b) within a minimalist framework? One option might be to say that English question words appear sentence-initially, whereas Japanese question words appear *in situ* in a position further to the right. This is a simple, economical, minimalist account. However, it misses the fact that although 'what' appears in initial position syntactically, semantically, it belongs in final position, and therefore there is more in common between English and Japanese than initially appears the case. However, to account for this fact, the theory has to add machinery, and so the account we get is no longer simple, economical or, minimalist.

Indeed, Kinsella (2009) has gone so far as to argue that EPP features have been added to the minimalist architecture specifically to drive movement, and for no other reason; there is (once again) no analog in formal languages, and no obvious reason that they should exist, for example, following from virtual conceptual necessity. As Chomsky (2000: 12) notes, "[i]n a perfectly designed language, each feature would be semantic or phonetic, not merely a device to create a position or to facilitate computation". EPP features, however, represent exactly that — features which create a position (the specifier position of the head holding the [EPP] feature), and which facilitate computation (by forcing a movement operation to apply). It is this essential tension which pushes the minimalist architecture away from the evolutionarily implausible ideal of

economy and elegance.

One seems to be left, in short, with a choice between (i) a theory which delineates an optimal system of language, but that fails to account for the data, and (ii) a theory which accounts for the data of human language, but delineates a system which is not optimal. Operations such as Move, features such as [EPP], and computations such as the generation of multiple derivations from one lexical array, to then be chosen between (such as is required in Chomsky 2001), do not belong in a bare minimal system, yet seem like concessions the Minimalist Program must introduce in order to account for the facts.

5.3.   *The Redistribution of Labor*

More broadly speaking, many minimalist analyses seem to achieve elegance only in Pyrrhic fashion, through a redistribution of labor that keeps syntax lean but at the expense of other systems, the burden of explanation shifted to phonology, semantics, and the lexicon, but the overall level of complexity much the same as before.

The phonological component of the grammar, for example, now looks after optional movements, such as Heavy NP Shift, topicalization, extraposition, and the movements required to deal with free word order languages. Also removed to this component of the grammar are the more obligatory movements of object shift and head movement, as in, for example, verb second languages. As a strongly lexicalist theory of language, the minimalist lexicon takes over the work required to deal with *wh*-movement, and case assignment, in the form of uninterpretable features. The binding of pronouns and anaphora is in at least some minimalist approaches (partly) the responsibility of the semantic component (Chomsky 1993, Lebeaux 1998). These redistributions may well be well-motivated, but simply shifting computations that were once assumed to be syntactic to these other components does not make the grammar as a whole any more optimal, simple, or perfect. In the limit, if one simply deems syntax to be the elegant, non-redundant part of language, the notion of elegance becomes tautological, and the notion of syntax itself loses any connection to the very linguistic phenomena that a theory of syntax was once intended to explain.

As Table 2 makes clear, this general trend is common. Many of the canonical issues that were given a strictly syntactic analysis in Government and Binding theory are removed to other components of the grammar — semantics, discourse, and in particular, phonology, and the lexicon, leaving a more minimal syntax, but considerably greater complications elsewhere, and suggesting that some degree of complexity that departs from virtual conceptual necessity may be inevitable, even if it is redistributed.

Problem	GB solution	MP solution
Head movement (e.g., Verb Second)	<u>Syntax</u>: movement of a category head to another category head position, e.g., V to I or C (Haider & Prinzhorn 1986, den Besten 1989)	<u>Phonology</u>: covert movement after Spell-Out (Chomsky 2001, Boeckx & Stjepanović 2001)
Object Shift	<u>Syntax</u>: DP movement to specifier position above VP in an extended IP (e.g., AgrOP), licensed by verb movement (Holmberg 1986).	<u>Phonology</u>: movement of object specified with a [–Focus] phonological feature to a position governed by a [+Focus] element (Holmberg 1999)
Passives	<u>Syntax</u>: DP movement from canonical object to canonical subject position for reasons of Case assignment (Chomsky 1981b)	<u>Phonology</u>: thematization/ extraction rule extracts direct object to the left edge of the construction (Chomsky 2001)
Wh-movement	<u>Syntax</u>: movement of *wh*-phrase to [Spec,CP], plus parameter determining level of representation at which the specifier of an interrogative CP must be filled (Lasnik & Saito 1992)	<u>Lexicon</u>: [*wh*]-feature on *wh*-phrase and interrogative C for checking, plus [EPP]-feature on interrogative C in non-*wh*-in-situ languages (Chomsky 2001)
Case Assignment	<u>Syntax</u>: assignment operation — transitive verbal head assigns accusative case to object DP under government, inflectional head assigns nominative case to subject DP in Spec–Head relation (Chomsky 1981b)	<u>Lexicon</u>: uninterpretable formal case features are checked via agreement of ϕ-features (Chomsky 2001)
Binding of pronouns & anaphors	<u>Syntax</u>: Binding Conditions A and B (Chomsky 1980)	<u>Semantics</u>: Binding Conditions A and B (Chomsky 1993), Binding Condition A (Lebeaux 1998)

Table 2: Shifting burdens of explanation and the Minimalist Program

6.   The Reality of Imperfection and its Implications for Linguistic Theory

If the analyses given above are correct, it is unrealistic to expect language to be a perfect or near-perfect solution to the problem of mapping sound and meaning, and equally unrealistic to expect that all of language's properties can be derived straightforwardly from virtual conceptual necessity. The sorts of optimality-, economy-, and parsimony-driven constraints that advocates of minimalism have emphasised may well play an important role in constraining the nature of language, but if our position is correct, there is likely to be a residue that cannot be derived purely from such *a priori* constraints.

6.1.   Beyond Virtual Conceptual Necessity

Two of the most salient forms of this residue — characteristic properties of

human languages that do not seem to follow from virtual conceptual necessity — are idioms and the existence of parametric variation between languages that cannot be boiled down to simple differences in word order (Broekhuis & Dekkers 2000).

Consider first idiomatic expressions, such as *kick the bucket*, *keep tabs on*, extra-grammatical examples of the sort discussed in section 3.1.3, and the many constructional idioms and partially-filled constructions discussed by Culicover & Jackendoff (2005) (e.g., *to VERB one's BODY PART off/out*, giving us *He sang his heart out*, *He yelled his head off*, *He worked his butt off*, etc.). In the first instance, the very existence of such phenomena does not accord well with minimalist principles: Formal languages, which generally lack idioms, are more economical, more parsimonious, and more elegant. One might ultimately craft a minimalist account of idioms, but it is hard to see how to do so without stretching one's notion of conceptual necessity.

Many seemingly straightforward patches to the Minimalist Program either fail or undermine the overall goals of minimalism. For example, one might suggest that the compositional operation of Merge could apply to units larger than individual words, but as Jackendoff (to appear) notes, on this proposal, partially-filled cases such as 'take X to task' are problematic. If Merge were to target the whole unit directly from the lexicon, it would need to be categorized as a verb rather than a verb phrase (phrases must be created by merging smaller units together), but it is not clear how or why a verb would be allowed to have an open argument position within it, and how this argument position would be filled given that Merge cannot target parts of an undecomposable unit. Alternatively, along the lines of Rögnvaldsson (1993), one might allow syntactic composition rules to operate in the lexicon, but although this might account for cases with an idiosyncratic semantics only, it leaves those cases which also have an idiosyncratic syntax, such as *be that as it may*, unexplained. Yet another possibility, along the lines of Svenonius (2005), might be to account for idioms in terms of more complex tree structures (Banyan trees) and movement to a position that is part of some unconnected structure (sideward movement, Nunes 1995), but this seems to be a clear case of adding machinery beyond what is conceptually necessary in order to account for the data.[7] [8]

Certain cross-linguistic variation, too, poses difficulties for theories that vest heavily in economy. Consider, for instance, the question of whether a language requires a phonologically overt subject (e.g., English) or not (e.g., Spanish), or of whether in a given language the verb comes before its object (e.g., English) or after (e.g., Japanese). In earlier theories, these questions were answered by appealing to the notion of parameters set during acquisition.

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[7]   Banishing idioms to the 'periphery' rather than the 'core' does not really help. It may well be that idioms somehow sit outside the regular form-meaning mapping rules of the language, but the fact remains that idioms are pervasive in human languages (Jackendoff, to appear), and that they are absent in formal languages; as such, their existence in *human* language must be explained.

[8]   Even in approaches that treat idioms in much the same way as non-idiomatic constructions (e.g., Distributed Morphology, Halle & Marantz 1993), complexity lingers, for example, in the form of a post-syntactic idiosyncratic meaning component.

Although that explanation still seems reasonable to the present authors, parameters of this sort actually pose difficulties for any orthodox version of minimalism. Take for example the original definition of the *pro*-drop parameter (Rizzi 1986), according to which the person and number features of the phonologically null subject are determined by the verb it occurs with. While this conjecture is quite reasonable, it poses difficulty for minimalist approaches, in which the person and number features of a verb are determined by the subject of that verb, in an Agree relation. In particular, on minimalist accounts, the null subject is licensed by the agreement features of the verb, inherently it cannot be specified with agreement features, but the verb's agreement features must be given their value by the null subject. To fix this, additional machinery of some form must be added to the minimalist architecture. One possibility (Alexiadou & Anagnostopoulou 1998) is to stipulate that agreement features are already valued on the verb in languages which allow phonologically empty subjects. This, however, requires stipulating that the distribution of such features differs cross-linguistically, and undermines the idea that a verb is not intrinsically singular or plural, 1st, 2nd, or 3rd person (Kinsella 2009). A second option is to say that null subjects possess the agreement features required to give value to the verb's features (Holmberg 2005). This, on the other hand, requires stipulating that the null subject has its identity already, suggesting that the lexicon must contain multiple null subject entries, and taking the null pronoun very far from its original characterization (Kinsella 2009).

The word order effects that the head directionality parameter gives rise to can be accounted for in the Minimalist Program in one of three ways, but each adds complexity to the system. The first says that the Merge operation which combines lexical items into larger structures is subject to a condition deciding which element of the pair being combined will determine the category of the combined unit (as a simplified example, if a verb and a noun combine, will the unit they form be a verb phrase or a noun phrase?); cf. Saito & Fukui (1998). The second posits a rule in the phonological component of the grammar which looks after the linear order of words, rearranging any orderings which are not permitted in the language in question. This, of course, is simply the type of redistribution of labor (from syntax to phonology) discussed in section 5.3. The third possibility (Kayne 1994) assumes a universal underlying order and invokes movement in the syntactic component, thus requiring additional features to be added in order to drive movement in languages whose surface order differs from the underlying order.

If the restrictions that the Minimalist Program places on language were to be relaxed, better analyses for idioms, or for parametric variation, might be possible. Instead of beginning with the assumption that the system should be optimal, economic and simple, and having to then add to the syntactic machinery in unconvincing and arbitrary ways in order to account for particular facts, it would surely be preferable to admit complexity from the outset and account for the data using rules, operations, and generalizations that apply across the system as a whole. Indeed, alternative frameworks for theorizing about language, which do not place perfection and economy at their core, offer more convincing accounts for these cases.

For example, idioms might be more naturally captured by construction-based approaches to language (e.g., Goldberg 1995, Kay & Fillmore 1999, Culicover & Jackendoff 2005) that posit a continuum of form-meaning mappings (constructions), where individual lexical items sit at the idiosyncratic end of the continuum, and general phrase structure rules, such as *VP → V NP*, sit at the general end, idioms sitting somewhere in the middle. Hardly elegant (and such theories have their own problems, Crain *et al.* 2009), but perhaps demanded by the empirical data. The redundancy of lexical storage that emerges from such a position would only be possible in a framework that accepts the existence of imperfection.

Optimality Theory, meanwhile, might lend insights into parametric variation. An optimality-theoretic take on the pro-drop parameter invokes the constraint of SUBJECT (which stipulates that a sentence must have an overt subject), which will be ranked high in languages like English, but will be out-ranked by many other conflicting constraints in languages like Spanish. This competition between constraints is seen clearly in the explanation for the existence of semantically empty subjects in languages which require an overt subject. The constraint of FULL-INT (which stipulates that all elements in a sentence must have meaning, i.e. expletive elements like 'it' and 'there' are ruled out) is in direct competition with the constraint of SUBJECT (Grimshaw & Samek–Lodovici 1998). In null-subject languages, FULL-INT is ranked higher than SUBJECT, that is, SUBJECT can be violated in order to satisfy FULL-INT. These languages, unlike English, disallow overt expletive elements; the reverse ranking of these two constraints would result in an overt expletive as we get in English.

(9)   a.   Piove.   *Italian*
            rains
            'It rains.'

      b.   *Il piove.   *Italian*
            it rains
            'It rains.'

      c.   *(It) is raining.   *English*

This alternative approach neatly captures the facts as a result of relaxing the demands of perfection and economy. It posits multiple constraints where a more parsimonious system might prefer to posit just one, and it allows (even demands) that these constraints compete, without demanding that a single one-size solution should optimally fit all.

More broadly, the fact that languages vary is not *per se* predicted by virtual conceptual necessity — one could easily imagine some species having sound-meaning mappings but having only a single grammar. Likewise, it seems unlikely that one would *a priori* expect that there would be significant arbitrary variation within a given language; constructed languages do not typically contain irregularities, idioms, and the like. Such variation — within languages and between languages — is characteristic of human language, and indeed

among the properties that most markedly differentiate human languages from other formal languages. To put this somewhat differently, if linguistics is to capture what is characteristic of human language, it cannot simply provide a kind of Platonistic conception of what ideal languages would be, it has to describe — and ultimately explain — the character that human languages actually have.

6.2.   A Recipe for (Bio)linguistics

The recognition that there are possible sources of imperfection in language must be reflected in how the language theorist goes about his day-to-day work. Moving forward, we suggest that the following principles should be followed:

(A)   Economy cannot be presumed. Although economy may contribute to the nature of language, one should not add features or operations to the system merely in order to achieve economy at a higher level of explanation.

(B)   One should not assume a priori that every property of language is rule-based. Individually stored examples may oppose the clean simplicity of a system that is entirely rule-based, but experimental evidence shows that the most parsimonious account may sometimes be a more complicated one (Pinker 1991, Prasada & Pinker 1993, Marcus *et al.* 1995).

(C)   One should not presume *a priori* that there is an absence of redundancy. A framework which is compatible with the existence of this imperfection may actually be more correct than one that is not compatible with it.

Biolinguistics is characterized by Boeckx & Grohmann (2007) in the editorial of the inaugural issue of this journal as an interdisciplinary enterprise concerned with the biological foundations of language. In order to fulfill this mission, biolinguists must take seriously insights from other disciplines. If our argument here is correct, at least one strand of recent linguistics — its tendency towards a presumption of perfection — is at odds with two core facts: The fact that language evolved quite recently (relative to most other aspects of biology) and the fact that even with long periods of time, biological solutions are not always maximally elegant or efficient. To our minds, anyway, the presumption of perfection in language seems unwarranted and implausible; a more realistic theory of language may reverse this trend, and look towards possible imperfections as a source of insight into the evolution and structure of natural language.

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Full Interpretation of Optimal Labeling

Hiroki Narita

This article proposes that the label for each syntactic node/set is fully derivable from Agree operating on edge-features of lexical items. It is also proposed that the derivations of labels transparently carve the path for θ-marking at the semantic interface. When tied with the label asymmetry condition at the Sensorimotor-interface and principles of derivational economy, this theory of labeling/Agree derives effects of what is traditionally called the θ-Criterion at the semantic interface. Ramifications for the principle of Full Interpretation are also discussed.

Keywords: Agree; edge-feature; Full Interpretation; label(ing); θ-Criterion

1.   Introduction

Since the advent of the *bare phrase structure* (BPS) theory (Chomsky 1994 *et seq.*), the role that labels play in the computational system of human language faculty (henceforth syntax for short) has been the subject of heated controversy. Since the BPS theory immediately makes it possible for syntax to generate an infinity of syntactic objects (SOs) by recursive application of *Merge* from bottom-up, without making any recourse to non-terminal symbols, projections or labels,[1] sound methodological minimalism naturally starts scrutinizing the notion 'label' itself, raising the following question:

(1)   Does the theory of human language really need to assume labels/labeling to set an empirically adequate account of the known variety of linguistic phenomena?

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[1]   Merge is a symmetric set-formation operation, which maps n SOs $\alpha_1, \ldots, \alpha_n$ to a set of them, $\{\alpha_1, \ldots, \alpha_n\}$.

Although the answers proposed in the past vary (see, e.g., Chomsky 1995, 2000, Collins 2002, Boeckx 2008, Fukui 2006a, 2008, forthcoming, Irurtzun 2007, Hornstein 2009, and Narita 2007, 2008 among many others), many researchers seem to assume that the notion 'label(ing)' is a necessary part of a good linguistic theory of sufficient descriptive/explanatory adequacy.[2] I concur. Indeed, there is evidence that labels yield instructions to both the Conceptual–Intentional system (C–I) and the Sensorimotor system (SM).

As for the C–I side, I would like to first point out the growing body of empirical evidence for the *Predicate-Internal Argument Hypothesis* (PIAH), launched by the advent of the VP-internal subject hypothesis (Koopman & Sportiche 1983, Fukui 1986, Sportiche 1988, Kuroda 1988). The leading assumption of the PIAH is that every predicate-argument structures of a predicate category P is 'saturated' within the projection of P: For example, all nominal arguments of a verbal category are base-generated/externally merged within *v*P. Here we see a direct mapping from syntax to semantics, which has been shown to be crucially mediated by labels. Thanks to the PIAH, linguists now can entertain the strongest possible hypothesis regarding the relation between syntax and C–I, namely that predicate-argument structure is syntax (Hale & Keyser 1993, 2002, Hinzen 2006). In particular, the PIAH suggests that the following syntax/C–I correspondence holds, which I would like to call the *PIAH-Conjecture*:

(2)   *PIAH-Conjecture*

If an SO {α, β} is labeled by the label of α, C–I interprets it as α θ-marking β (if α is a predicate category).[3]

Note crucially that the predicate-argument relation is asymmetric: A predicate category P *θ-marks* an argument category A, not vice versa. Note further that it is always the predicate category P that projects over its argument A asymmetrically. This correspondence rather strongly suggests to us the possibility of motivating label(ing) from C–I considerations: Labeling is a syntactic operation that codes the predicate–argument asymmetry between Merge-mates that appears subsequently at the point of C–I-interpretation (see Irurtzun 2007).

Interestingly, empirical evidence suggests that labeling feeds asymmetry to SM as well. One of the fundamental phonological operations, feeding SM, is *linearization*. Presumably due to the modality restriction imposed by the SM system, the hierarchical, '2D' structure generated by syntax is 'unpronounceable'. Such an unpronounceable input must be transformed to a corresponding pronounceable output of some form, satisfying SM-interface condition.[4] Linearization refers to the phonological mapping of an input hierarchical SO to a corresponding

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[2]   Collins (2002) replaces 'label' with an alternative notion 'locus', but essentially the same question applies to the latter, too.

[3]   For now, this parenthesized qualification seems necessary, since we know that not all labels have interpretation at C–I: Consider, for example, subject raising to [Spec,TP], which is assumed to let T project, even though T does not θ-mark the moved subject in any obvious sense. But see section 3 below.

[4]   Of course, I do not deny the existence of phonological properties that make recourse to hierarchical syntax in some fashion, for example, prosody. See Samuels (2009a) for related discussion.

sequence of phones readable by SM. I would like to point out that, although past proposals are diverse, they seem to have reached a consensus that the linearization process requires asymmetrically labeled syntactic input. Let us refer to this requirement by linearization as the *Label Asymmetry Condition*.

(3)   *Label Asymmetry Condition*

SM-linearization works properly for a syntactic node/set only if one and only one label is defined for that node/set.

To take a representative example, Kayne's (1994: chap. 3) LCA-based account of linearization resorts to asymmetric labels. Specifically, in order to let a specifier/ adjunct of head H asymmetrically c-command its H′/HP-sister, Kayne's account must rely on May's (1985) category/segment-distinction on syntactic nodes (saying that any specifier/adjunct merger splits the target category into segments), which in turn is made available by asymmetric labels (but see Uriagereka 1999). Indeed, without the category-segment distinction on each node, "specifiers and adjoined phrases appear to have no place" in his theory (Kayne 1994: 16). Chomsky's (1995) modified LCA carefully avoids this problem by assuming that non-minimal, non-maximal projections (X's) are invisible to the LCA and hence they do not c-command their sister Spec in the first place, which clarifies the relevance of labels to the Kaynean antisymmetry program. It is Fukui & Takano (1998) who show that the recourse to c-command is actually eliminable from the Kaynean antisymmetry program, a proposal that further clarifies the crucial relevance of labels to linearization. Their proposal is that linearization uniformly maps headed-nonhead distinction on two Merge-mates to postcedence (if α projects over β, then {α, β} is mapped to a string where β precedes α), yielding the universal Spec–Compl(ement)–Head word order, with apparent 'head-initial' Spec–Head–Compl order being derived by Head–to–Spec movement; see already Takano (1996). Both Kayne and Chomsky's label–and–c-command-based anti-symmetry theory and Fukui & Takano's only–label-based theory share the goal of deriving the effect of head-parameter from invariant UG axioms. In retrospect, Chomsky's (1981) head-parameter, some version of which is adopted by a number of researchers even currently (e.g., Epstein *et al.* 1998, Richards 2004, Fox & Pesetsky 2005), was the first proposal that clearly expressed the crucial relevance of projections/headedness to linearization. All in all, it should be clear that all of the past proposals on linearization processes rely on asymmetric labels, which I take to mean that the role of asymmetric labeling at linearization is indispensable.

In short, the asymmetry between a predicate and its argument is uniformly traced by asymmetric labels (PIAH-Conjecture). SM also exploits the same sort of asymmetry for linearization purposes. If we are right in seeing syntax as a device generating instructions to C–I and SM, and if both C–I and SM utilize asymmetry *of the same sort*, then it becomes plausible to suppose that labeling as an asymmetry-coding device is a syntax-internal operation (see already Chomsky 1994, 1995; see also Boeckx 2008). It is essentially these empirical considerations that lead me (among others) to reserve a positive answer to the question in (1).

However, recall that the minimalist program is a research program guided

by the *strong minimalist thesis* (SMT) that human language is an optimal linker of C–I and SM (see Chomsky 2001, 2008, Berwick & Chomsky, to appear, and Narita 2009d for varying definitions thereof). Since minimalism holds the SMT not only as a methodological guideline but also as a substantive empirical hypothesis about biological reality, we also have to ask whether there is any sense in which the notion 'label' is a 'must' for a perfect system like human language, which itself emerged in almost just an 'eye-blink' in evolutionary time. Thus, the question is:

(4)   Does 'label' count as a virtually conceptually necessary part of human language (an optimal C–I/SM-linker, insofar as the SMT holds)?

It is essentially with respect to this substantial minimalism question that Chomsky (2007a: 23) rightly reminds us that "it may be that as understanding progresses, the notion 'label' will remain only as a convenient notational device, like NP, with no theoretical status," and argues that "reference to labels (as in defining c-command beyond minimal search) is a departure from the SMT, hence to be adopted only if forced by empirical evidence, enriching UG." I am also sympathetic to this argument. I will hint in section 2 that there may be no such thing as 'projection' or percolation of features as implicitly assumed by virtually every previous theory of labels, though I will also argue in sections 2–4 that the driving force of labeling, which I will propose to be *Agree* operating on *edge-features* of lexical items (LIs), actually constitutes a part of syntax's optimization to C–I-interpretation. Thus, this article is an attempt to articulate my own moderate "Yes" to question (1) and a moderate "No" to question (4) at the same time.

2.   A Unification of Labeling and Agree

If labels are generated by syntax, a computational system that optimally interfaces with C–I and SM, insofar as the SMT holds, then syntax should be designed in the way that it generates labels in an optimal way. This section provides my own proposal for how syntax meets this task. The core proposal is that labels in syntax can be defined in terms of Agree with respect to edge-features of LIs. I will first set out some of my assumptions about the functioning of Agree in section 2.1. Then I will attempt to unify labeling and Agree in section 2.2, where I will also provide a theoretical characterization of the edge-feature (EF). Discussion on how the proposed system works will follow in section 2.3.

2.1.   *Some Background Characterization of Agree*

Since Merge is just a set-formation operation that combines SOs, it cannot rearrange elements internal to already created SOs. Thus, Merge obeys the *No-Tampering Condition* (NTC):

(5)   *No-Tampering Condition* (NTC)

Merge of X and Y leaves the two SOs unchanged (Chomsky 2008).

However, empirical evidence suggests that something that cannot be expressed by Merge is also at stake in human language: Linguistic expressions exhibit some dependency between two non-sister LIs that cannot be readily captured by Merge. For example, in the *there*-expletive construction in (6) the main verb exhibits singular number agreement with an associate NP. Similarly, the negative particle in the matrix clause can license an NPI within its c-command domain as in (7). To take another example from Japanese (8), the wh-in-situ in the embedded clause is licensed by the question particle *ka* in the matrix clause, and so on.

(6)   There *seems* to be likely to be *a boy* in the garden.

(7)   I *don't* think *anybody* will take French this semester.

(8)   Kimi-wa [John-ga   *nani*-o   katta   to]   omotta-ndesu *ka*?   *Japanese*
       you-TOP   John-NOM   what-ACC   bought   that   thought-POL   Q
       'What did you think John had bought?'

So syntax must provide a way (or several ways) to code such (potentially long-distance) non-sister relations between two LIs. Chomsky (2000) and many other subsequent works suggest that *Agree* is responsible for capturing (at least some of, optimally all of) such dependencies.

Agree is a dependency established between a pair of LIs by a derivational search operation relative to a given feature F. Some LI P with an uninterpretable feature F acts as a *probe*, and it seeks in a certain search domain a matching F on a *goal* LI G for establishing an Agree-dependency between them. In what follows, I will adopt the term *Search* to refer to the derivational search operation in question and *Agree* to refer to the relation established thereby, respectively, a distinction that is sometimes blurred in the literature but is nevertheless important, as I will claim. If P's Search reaches a matching goal G with respect to F (henceforth, *P Searches$_F$ G*), then Agree-relation with respect to F holds from P to G (henceforth, *P Agrees$_F$ with G* or *Agree$_F$(P, G)*).

Given that the asymmetric probe-goal distinction on the two Agree-mate LIs (P and G) arises derivationally at each application of Search, I claim that the following holds for any Agree-relation.

(9)   *Agree is asymmetric*

Agree$_F$(X, Y) ≠ Agree$_F$(Y, X).

Moreover, following Chomsky (2000) and many subsequent works, I assume that there is a structural condition on the possible application of Agree, namely that for any Agree$_F$(P, G), the search domain for a probe P is restricted to P's c-command domain in a given SO. This condition can be stated as in (10).

(10) *The c-command domain condition on Search*

For any $Agree_F(P, G)$, G must be within P's sister/complement.

I further adopt Chomsky's (2001) hypothesis that uninterpretable features that probe are nothing more than features that lack value. Unvalued features that have established appropriate Agree-relations get deleted at the point of *Transfer* (Chomsky 2001, 2004, 2008). Transfer is an operation that stripes off a certain well-defined domain of an already constructed SO to C–I and SM. The domain subjected to Transfer (the Transfer domain for short) becomes inaccessible to further syntactic operations — the *Phase-Impenetrability Condition* (PIC); see Chomsky (2000 *et seq.*).

(11) *Phase-Impenetrability Condition* (PIC, Chomsky 2000)

In a phase α with head H, the domain of H is not accessible to operations outside α, only H and its edge are accessible to such operations.

Certain unvalued features are designed to probe matching goals (their interpretable valued counterpart) within their search domain, establishing one or more Agree-relations before Transfer. If appropriate Agree-relations are established, unvalued features can get deleted by Transfer, and if not, they will remain 'uninterpretable', leading the derivation to crash. I follow Uriagereka (1999) and many subsequent works in assuming that Transfer can apply multiple times in a given derivation. For concreteness, I specifically assume with Chomsky (2000, 2004, 2008) that a certain class of LIs, called *phase heads*, trigger Transfer of their complement at the completion of their computation.

I will further assume that Agree is a transitive relation.

(12) *Agree is transitive*

For any feature F and any three LIs X, Y and Z, if $Agree_F(X, Y)$ and $Agree_F(Y, Z)$ hold, then $Agree_F(X, Z)$ holds.

Already many researchers propose some versions of (12) (Frampton & Gutmann 2000, Adger & Ramchand 2001, Legate 2005, Haegeman & Lohndal, in press; see also Hiraiwa 2005). For example, Adger & Ramchand (2001) and Legate (2005) propose that (12) is responsible for the successive cyclic realization of relativizing complementizers in Scottish Gaelic relative clauses. Consider (13).

(13) An duine [**a**   thuirt   e [**a**/*gun   bhuaileas   e]]   *Scottish Gaelic*
      the man   C-REL said   he C-REL/that strike-REL   he
      'The man that he said he will hit'

                                                        (Adger & Ramchand 2001: 9)

According to Adger & Ramchand (2001), the most deeply embedded clause contains a gap for relativization, whose interpretive Var(iable)-feature triggers the (successive cyclic) complementizer agreement in question. Interestingly, not only the main relative clause but also its subordinate clause exhibits the

relativized complementizer *a*. Adger & Ramchand (2001) and Legate (2005) propose that Agree is essentially a transitive relation (12), and that the cyclic establishment of Agree-relations (each of which is phase-bound) can result in the apparent long distance multiple agreement in question. Their proposal can be schematically shown as in (14).

(14) *Legate's and Adger & Ramchand's analysis: Agree and transitivity*

the man $[C_1 \ldots [v_1 \ldots [C_2 \ldots [v_2 \ldots pro_{Var} \ldots]]]]$
$\underbrace{\quad}_{\text{Agree}_{Var}} \underbrace{\quad}_{\text{Agree}_{Var}} \underbrace{\quad}_{\text{Agree}_{Var}} \underbrace{\quad}_{\text{Agree}_{Var}}$

According to the theory of phases by Chomsky (2000, 2007a, 2008), no probe in a given phase Ph can look into subordinate phase domains, due to the PIC (11). Since finite clauses should constitute phases, Var-feature probing from the highest relative clause complementizer should not be able to reach the deeply embedded gap, crossing multiple phase boundaries. However, if Agree is a transitive relation, then each of the lower phase heads can establish Agree$_{Var}$ to pass up the relevant Agree$_{Var}$-relation to the highest relative complementizer, as depicted in (14). Combined with the phase theory, this set of data constitutes good evidence for the transitivity of Agree (12).[5] See Legate (2005) for further evidence for this proposal.[6]

Summarizing, Agree is asymmetric (9) and transitive (12), subject to the c-command search domain condition (10) and the PIC (11), as I will assume in what follows.

2.2. *The Agree$_{EF}$-based Label Theory: An Outline*

Now we are ready to articulate the core proposal that *Agree$_{EF}$ can fully derive labeling*, where EF stands for *edge-feature* (Chomsky 2007a, 2008). This sub-section will outline the gist of the Agree$_{EF}$-based label theory.

According to the BPS theory, every syntactic expression is composed by recursive application of Merge in a bottom up fashion. Since all the SOs are composites of a finite number of lexical items (LIs), 'computational atoms' for syntax, these LIs must contain a property that enables them to undergo Merge. Chomsky (2007a, 2008) calls this property the *edge-feature* (EF):

> For an LI to be able to enter into a computation, merging with some SO, it must have some property permitting this operation. A property of an LI is called a *feature*, so an LI has a feature that permits it to be merged. Call this the *edge-feature* (EF) of the LI. […] The fact that Merge iterates without limit

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[5]   This view is corroborated by Adger & Ramchand's (2001) independent argument that the A'-dependency in Scottish Gaelic does not involve null operator movement.

[6]   Many of the 'multiple agreement' phenomena (such as those discussed by Hiraiwa 2005) can be readily accounted for in terms of the transitivity of Agree (12), without recourse to Hiraiwa's powerful mechanism of *Multiple Agree*, a set of simultaneously established Agree-dependencies from a single probe to multiple goals. I will leave the issue of Multiple Agree open in what follows. Note that the transitivity of Agree is anyway necessary to account for the cross-clausal long distance agreement effects as in (13), since finite clauses should constitute phases impenetrable for later operations (due to the PIC; but see Bošković 2007).

is a property at least of LIs — and optimally, only of LIs, as I will assume.

(Chomsky 2008: 139)

According to this proposal, each LI is associated with an undeletable EF. Since my proposal will rely on some specific assumptions on the EF, I will first make them explicit. In the end, I propose that the EF is closely related to the matter of how labels are provided in syntax.

I follow Chomsky in assuming that each LI is associated with an EF.[7] I further assume with him that the presence of an EF signifies Mergeability. Merge applies freely, regardless of whether internally or externally, so long as the Merge-mates are associated with EFs (Chomsky 2007a, 2008). Since each LI will retain its Mergeability throughout the derivation, we are led to assume that the EF is undeletable. Further, since there is no reason to suppose otherwise, I assume that one and the same EF is associated with every LI, and consequently that the EF does not have any value sub-specification. That is, I assume that the EF is unvalued.[8,9]

However, note that the EF is just a feature. We have assumed above that Agree/Search constitutes an indispensable part of syntactic derivations, and that Search is triggered by an unvalued feature (probe). If an EF is a feature that lacks value, then there is no principled reason to exclude the possibility that the EF is also a feature that can act as a probe for Agree. In what follows I will pursue exactly this possibility. I will specifically claim that Agree$_{EF}$ is at the core of labeling in syntax.[10]

Every LI in the human Lexicon is associated with an EF which is unvalued but nevertheless undeletable. I specifically propose that due to this property, the EF can act either as a probe or as a goal for Agree$_{EF}$ repeatedly. A probing feature seeks its matching counterpart, namely a feature of the same sort. Thus an EF can search an EF, which can be found virtually everywhere in the derivational workspace, since every LI has an EF. Moreover, since the goal EF always lacks value by definition, Agree always fails to value the probing EF (and the goal, too). Thus, even after the establishment of Agree, EFs remain active, possibly participating in subsequent Agree with another EF. Some unvalued features need to be valued by the Agree-mate at Transfer in order to receive legitimate interpretation, but not the EF, as I assume.[11] That is, I specifically assume that the EF is a feature that can be deleted or appear at interfaces without getting valued.

Given these background assumptions, my proposal is that labels are fully

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[7]   Maybe with a possible exception of interjections, if they lack EFs (see Chomsky 2008:1 39). I put this matter aside in what follows.

[8]   In fact, Chomsky (2007a: 8) suggests, "[v]ariation among LIs with regard to deletability of EF would be a departure from SMT, so we assume that for all LIs, one or the other property holds." Here I am pointing out that variation among LIs with regard to the value-specification of EF would also be a departure from the SMT.

[9]   See Narita (2009a, b) for further discussion of EFs.

[10]   See already Fukui (2006a, 2008), who proposes that the EF is crucially at stake in the labeling operation in syntax (what he calls *Embed*). I am specifically proposing here that what utilizes the EF for labeling purposes is no different from Agree. See also Narita (2007) and Boeckx (2008) for some relevant discussion.

[11]   Here, I am departing the oft-held assumption that feature-valuation is a necessary part of Agree.

derived by means of Agree$_{EF}$-relations established in syntax. The proposed definition of labels is given in (15).

(15)   *The Definition of Labels*

For any SO Σ, an LI H is the label of Σ =$_{def}$ H Agrees$_{EF}$ with the rest of the LIs within Σ.[12]

The subsequent discussion will substantiate the proposal.

2.3.   How It Works

This sub-section outlines how the proposed Agree$_{EF}$-based label theory works. Underlying this proposal is the assumption that some interface conditions require that each node/set be unambiguously labeled. As discussed in section 1, the label asymmetry condition, feeding linearization by SM, is one such constraint:
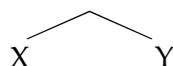
(3)   *Label Asymmetry Condition*

SM-linearization works properly for a syntactic node/set only if one and only one label is defined for that node/set.

Some C–I conditions are to be explicated in the subsequent discussion.

2.3.1. Head–Compl Cases

To start the illustration of how the proposal works, consider first the simplest case of merger of two LIs, say X and Y.

(16)   External Merge of X and Y → {X, Y}

X   Y

Here, both X and Y are associated with EFs. The label asymmetry condition (3) forces syntax to generate a label for this symmetric Merge-result, which is just a set, without internal ordering among elements: {X, Y}. Our proposal postulates that this is done by means of Agree$_{EF}$. Suppose that X's EF probes into X's c-command domain, which is just another LI Y in the case in question. Y has an EF, so this probe can establish an asymmetric relation Agree$_{EF}$(X, Y).

(17)   X Searches$_{EF}$ Y → Agree$_{EF}$(X, Y) holds.

X   Y

Search$_{EF}$

The result is that X Agrees$_{EF}$ with the rest of the LIs contained in (16), thus X will qualify as the label for {X, Y} by definition (15). (The label LI will be informally marked by underscores here and below.)

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[12]   See fn. 25 for a slight modification of (15).

In (16), X and Y mutually c-command each other, thus each can probe the other. Thus if, on the other hand, Y Searches$_{EF}$ X at (16), Y will qualify as the label instead. The choice of which Searches$_{EF}$ which here is in principle free, I assume, insofar as the choice yields a right sort of structure interpretable at the C–I- and SM-interfaces. The effects of Agree$_{EF}$ at the C–I-interface will be discussed in section 3 in detail.

Note also that even after Agree$_{EF}$(X, Y) holds as in (17), both EFs of X and Y will remain unvalued. Since we assumed that an unvalued EF can probe, X and Y seem to be still eligible candidates for establishing 'another' Agree$_{EF}$-relation (Agree$_{EF}$(X, Y) or Agree$_{EF}$(Y, X)) in (17). However, I claim that it is not the case. Consider establishing the 'second' Agree$_{EF}$(X, Y) first. This relation is completely useless: it does not result in any new label, and the Search-operation resulting in such a futile Agree-relation is presumably ruled out by some economy principle (I will return to this matter later in section 3.2.) Consider next another possible Agree-relation in (17), Agree$_{EF}$(Y, X). In (17) X first Searches$_{EF}$ Y, letting X become the label of (17). But both EFs remain unvalued and hence eligible for initiating anther application of Agree, so let us suppose that Y can in principle initiate Agree$_{EF}$(Y, X) at (17). According to (15), this would cause Y to project in addition to X, thus resulting in a structure where *more than one label coexists*, what Citko (2008) calls a 'Project Both' structure: {X̲, Y̲}. I argue that such an ambiguously labeled structure violates the label asymmetry condition (3), and therefore is ruled out at SM. In general, in order to satisfy (3), it must be the case that only one label is generated per each Merge-result set/node. However, if syntax establishes Agree$_{EF}$(Y, X) in addition to Agree$_{EF}$(X, Y) in (17), label asymmetry disappears: both X and Y become the label of the Merge-result by definition (15). Then, the SO would become unpronounceable again, and the SM-linearizability conditions presumably filter out such an 'asymmetry-breaking' Search$_{EF}$-operation, as I propose.[13] Thus, at each external merger of two LIs, one and only one of them probes the other in any convergent derivation, establishing Agree$_{EF}$, which in turn generates a label for the Merge-result by (15), deriving the head-complement configuration.

If the complement is just an LI, as in (16), the labeling convention in (15) is pretty straightforward, since basically it is just a matter of establishing one Agree$_{EF}$-relation between the 'head' LI and its sister LI. But (15) is expected to cover the head-complement cases in general. Suppose another LI, say Z, is merged with (17).
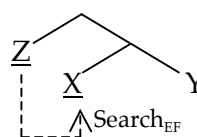
(18)   External Merge of Z. → {Z, {X̲, Y}}
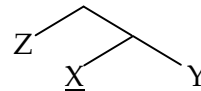
Suppose further that we want Z to become the label of (18). In order for Z to project by our definition of labels (15), Z must undergo Agree$_{EF}$ with all the LIs contained in (18), here X and Y. Suppose that Z undergoes Agree$_{EF}$ with X in (18).

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[13]   Though see fn. 33.

(19)   Z Searches$_{EF}$ X → Agree$_{EF}$(Z, X) holds.

This Agree-relation in effect derives the label Z for (18). The reason lies in the transitivity of Agree discussed above (12), repeated here.

(12)   *Agree is transitive*

For any feature F and any three LIs X, Y and Z, if Agree$_F$(X, Y) and Agree$_F$(Y, Z) hold, then Agree$_F$(X, Z) holds.

In our case (18), given Agree$_{EF}$(Z, X) and Agree$_{EF}$(X, Y), Agree$_{EF}$(Z, Y) is deduced by the transitivity of Agree (12).

Notice that X and Y mutually c-command each other, hence they should be equidistant to the probe Z, and Y should be a potential goal for Z's probe at (18). However, even if Z Searches$_{EF}$ Y and establishes Agree$_{EF}$(Z, Y), it does not generate a label for (18): all we have then are Agree$_{EF}$(Z, Y) and Agree$_{EF}$(X, Y), a combination of which does not derive any label by our definition of labels (15). Thus, in order to provide a label for (18), Z needs to enter Agree$_{EF}$(Z, X) anyway, hence Z's Search$_{EF}$ of Y counts as a superfluous operation for the purpose of labeling. Such a futile application of Search presumably violates some computational economy principles governing syntax: A derivation D$_1$ where Z Searches$_{EF}$ Y and then X at the point of (18) is presumably blocked by the presence of a more economical derivation D$_2$ in which Z only Searches$_{EF}$ X, deriving label X by the transitivity of Agree. I will come back to this issue in section 3.2.

2.3.2. Internally Merged Spec

Let us turn to Spec cases. I will first discuss the cases of internally Merged Specs, and then those of externally Merged Specs.
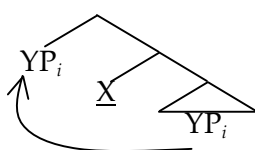
Suppose that recursive application of Merge constructs an SO (20), which contains a sub-constituent YP. Suppose that X is the label of (20). According to our labeling convention (15), that is, X has established Agree$_{EF}$-relations to all the LIs within its complement.

(20)
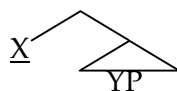
Suppose further that internal Merge dislocates YP to the edge of (20):

(21)

The usual consensus is that dislocation/internal Merge in human language necessarily results in 'uniform chains', in the sense that it does not alter the maximal/minimal status of the label of moved elements (Chomsky's 1995 *Chain Uniformity Condition*; see also Emonds's 1970, 1976 'structure-preserving' hypothesis). That is, the label of the target SO projects in all instances of internal Merge.[14] In the present case, the internal Merge of YP necessarily results in the projection of X. But why should it be the case? The label asymmetry condition (3) requires that the Merge-result (21) be provided with an unambiguous label. The observation amounts to the claim that it is always X that enters $Agree_{EF}$-relations with the LIs in YP. But why? Why can't other LIs, say the label of YP, $Search_{EF}$ X and establish $Agree_{EF}$/labeling?

In fact, our $Agree_{EF}$-based label theory provides a straightforward answer to the question, when combined with the *copy/remerge theory of movement* (Chomsky 1993, 1995, 2004). Note that prior to internal Merge of YP, X was the label of (20). By our definition of labels (15), X $Agrees_{EF}$ with the rest of the LIs within (20), crucially including those contained in YP. Note further that the YP internally merged to the edge of (20) is just a copy of the YP contained in (20), and X has already established $Agree_{EF}$-relations to all the LIs contained in the original occurrence of YP. Internal Merge is just *remerge*, and the remerged YP is just the same SO as the one contained in XP. One generally expects that the syntactic relations which hold from α to β is identical for all the copies of β, if there are any. Thus, for example, we see that the copies of the same DP would share the θ-roles, Case-agreement relations to the checking head, 'indices'/binding-relations, and so on. So should the $Agree_{EF}$-relations between X and the LIs within YP be. Thus a generalization (22) holds, which is just a straightforward consequence of the copy/remerge theory of movement.

(22) *Agree is conservative over internal Merge*

Copies/occurrences of the same SO created by internal Merge share the same set of Agree-relations.

X has already $Agreed_{EF}$ with the LIs within YP at the derivational point of (20). Thus, due to (22), the same $Agree_{EF}$-relations must be conserved for the two copies created by internal Merge. That means, X must $Agree_{EF}$ with the LIs in both of the two copies of YP. *Therefore*, X is the label of (21) by definition. In this way, we derive the Chain-Uniformity effect of internal Merge without adding any independently stipulated constraints to the theory of UG. Let us call this result *Corollary 1*.

(23) *Corollary 1*

If α is internally merged to an SO β labeled by an LI H, leaving its occurrence within β, then the Merge-result is labeled by H.

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[14] Donati (2006) claims that it is not always the case, raising the *wh*-free relative construction as a possible counterexample. Since alternative analyses are readily available (see, e.g., Caponigro 2002, 2003), I put this matter aside.

This way, the Chain-Uniformity effects are subsumed under the copy/remerge theory of movement.

Note that I am departing from the once-dominant assumption that internal Merge should be triggered by a viral 'EPP-property' of the attracting head. It has been widely assumed in the literature that internal Merge is a 'costly' operation and should be employed only when its application contributes to checking of a viral uninterpretable feature called the 'EPP-feature' (the last resort conception of movement; Chomsky 1986, 1995). However, Chomsky (2004, 2007a, 2008) argues that this was a wrong conception of internal Merge. Rather, without any further stipulation, Merge should be able to take as input either two independent SOs (as in external Merge) or two SOs one of which is part of the other (as in internal Merge). Correspondingly, "I[nternal]M[erge] (= Move, with the 'copy theory') is as free as E[xternal]M[erge]" (Chomsky 2008: 140), unless stipulated otherwise, due to the undeletability of EFs. Thus, every LI can in principle be subject to internal Merge, insofar as an undeletable EF is present. No extraneous and redundant 'EPP-feature-checking' is stipulated to be necessary to drive internal Merge.[15]

2.3.3. Externally Merged Spec

So far we have seen that our Agree$_{EF}$-based label theory correctly captures head-complement cases and internally merged Spec cases. Our next task is to extend our discussion to the cases of Spec-headed external merger. Consider an SO of the form {H, XP}, where H, an LI, Agrees$_{EF}$ with all the LIs within XP, thus H qualifies as the label. Suppose that an independently constructed SO, say YP, is externally merged with that SO. For concreteness suppose Y Agrees$_{EF}$ with all the LIs contained in YP, thus Y qualifies as the label of YP.

(24)
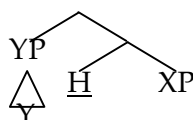
Suppose that we want the result that H projects in (24). Since the existence of externally merged Specs in human language is undeniable (consider, e.g., an external argument DP merging into [Spec,vP]), syntax should have a way to let H project in the configuration like (24). According to our definition of labels (15), this essentially means that H can enter Agree$_{EF}$ with the LIs within YP, in particular Y. However, recall the c-command domain condition on Search, repeated here, which restricts the search domain for a probe to its sister/complement.

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[15]   Although internal Merge itself should be as free as external Merge, there may be some language-specific phonological constraints that require some Agree$_F$-relation to have a phonological reflex in terms of dislocation, utilizing the availability of free (internal) Merge: For example, it might be the case that some phonology, say of Hungarian, requires the goals of Agree$_{Q/WH}$ to be pronounced adjacent to the [+WH] C, whereas such requirement is absent in other phonological systems, say of Chinese. We can easily recapture the (only) apparent 'costfulness' of internal Merge in these terms.

(10)   *The c-command domain condition on Search*

For any Search for Agree$_F$(P, G), G must be within P's sister/complement.

The Spec of H (YP here) is, however, by definition out of H's c-command domain. What we want is nevertheless the result that H can Search$_{EF}$ into YP here. To ensure this, Search should be able to extend its search domain to its Spec, at least in some environment. Then, the problem is:

(25)   How can a probe H search into its Spec, while still conforming to the c-command domain condition on Search (10)?

I would like to sketch two options in resolving this matter, both of which are compatible with the subsequent discussion.

2.3.3.1. Option A

Option A will seriously entertain some consequence of Chomsky's (2000, 2008) conception of phase. According to Chomsky, Transfer applies cyclically at the completion of each phase, sending off the phase-interior domain (the complement of the phase head) to the C–I- and SM-interfaces. Syntax will then 'forget about' the Transferred domain completely, behaving as if it is not there in the derivational workspace anymore. That is to say, if a phase head H in a configuration {YP, {H, XP}} Transfers XP, then only the phase head H and its 'edge', YP, will remain visible to syntax after Transfer.

Suppose, then, that after Transferring its first-merged complement (XP), a phase-head H will be able to regard its second-merged phrase (YP) as its 'second complement'. I would like to propose along this line of reasoning that the head H will also become able to extend its search domain to this second complement, too.

Returning to the problem of externally merged Spec in (24), addressing question (25), suppose that H is a phase head. Suppose further that H subjects its complement XP to Transfer, before or after the external merger of YP, which results in the elimination of XP from the derivational workspace:

(26)   H Transfers the complement XP: {YP, {H, ~~XP~~}} → {YP, H}
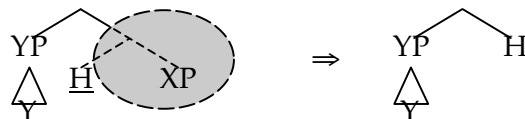
Now YP becomes the 'second complement' of H, and H can search into YP, while still satisfying the c-command domain condition on Search. The label asymmetry condition requires that the structure (26) should be provided with an unambiguous label, so Agree$_{EF}$(H, Y) is required to set H as the label of (26).

(27)   H Searches$_{EF}$ Y → Agree$_{EF}$(H, Y) holds.
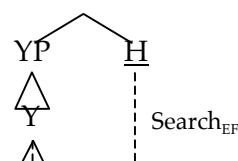
The transitivity of Agree (12) derives Agree$_{EF}$-relations from H to all the LIs within YP (since Y already Agrees$_{EF}$ with them), and H is assigned to (27) as its label by definition (15).

Note that, insofar as we keep to Option A, the projection of a head H over an externally merged Spec crucially hinges on the (prior or subsequent) Transfer of the first complement by H. Then, under the assumption that all nodes must be labeled, the consequence is (28).

(28)   An LI H can have an externally merged Spec only if H's complement can be subject to Transfer.

(28), a corollary of Option A, has certain virtues: first of all, it can make sense of Chomsky's (2000, 2001, 2008) distinction between the transitive/unergative v*P-phase and the unaccusative/passive vP-phase. Rather than introducing a stipulative distinction between transitive/unergative v* as a strong phase head and unaccusative/passive v as a weak phase head, we can rather say that (28) forces only v in the transitive/unergative construction to Transfer its complement, since it will take an external argument DP/NP as an externally merged Spec. In contrast, v in the unaccusative/passive construction will not have an external argument DP as its Spec, thus it is free from the pressure of Transferring its complement, yielding in its apparent 'weak phase' nature. Moreover, (28) will set an explanation of the claim that only a phase head can have an externally merged Spec. This claim seems supported by a growing body of empirical data: See McGinnis's (2001) argument that introduction of an indirect object requires another (strong) phase head called Appl(icative) (see also Pylkkänen 2008); see also Chomsky's (2007a) and Fukui & Zushi's (2008) claim that only nPs which host determiners in their Spec constitute (strong) phases. These considerations provide indirect but important support for (28), and thus for the proposed reverse-engineering of labels.

2.3.3.2. Option B
Another possible solution to the question in (25) will rely on the possibility of syntactic Head(–to–Spec)-movement, which is independently argued for by Fukui & Takano (1998, 2000), Toyoshima (2000, 2001), and Matushansky (2006). Suppose that H in (24) is further internally merged to (24), resulting in (29):

(29)
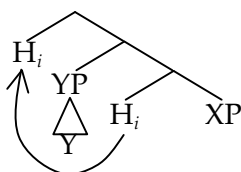
Nothing proposed so far blocks this internal Merge, given the assumption, reached in section 2.3.2, that internal Merge is (as) costless (as external Mer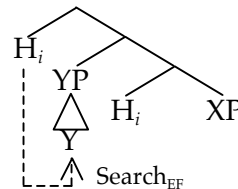ge). Now, the higher copy of H takes YP in its c-command search domain. Then, nothing assumed so far prevents this occurrence of H from probing its expanded search domain (see Bošković 2007 for a proposal in the recent framework that moved elements can probe). Then, suppose that H enters $Agree_{EF}(H, Y)$:

(30) H $Searches_{EF}$ Y $\rightarrow$ $Agree_{EF}(H, Y)$ holds.

The result is that H becomes the label of (30): $Agree_{EF}(H, Y)$ and the transitivity of Agree (12) leads to H $Agreeing_{EF}$ with the rest of the LIs within the entire SO (30), thus projecting by definition (15).

In this way, Option B does not resort to a prior application of Transfer as Option A does, though derivations in line with Option B do not yield (28) in any obvious way.

Note that (30) is a 'Project Both' structure in some sense, i.e. both of the Merge-mates' labels, both H, 'project', which might be problematic for the label asymmetry-based linearization purposes, no matter what linearization mechanism ultimately turns out to be correct. However, note that the two 'heads' are just two occurrences of one and the same LI H, and empirical evidence suggests that usually all but one occurrence of an LI can remain unpronounced at the SM-interface. If either one of the occurrences of H is chosen to be unpronounced, then (30) becomes still linearizable, as I assume (see Narita 2007, 2008 for some relevant discussion). It is not unreasonable to assume that there can be language-specific or construction-specific variation in which copy of H to pronounce in such a configuration.[16,17]

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[16] Alternatively, if not H itself but an LI that H $Agrees_{EF}$ with, say X, is internally merged to the edge in question and $Agrees_{EF}$ with Y 'on H's behalf', then the transitivity of Agree in combination with $Agree_{EF}(H, X)$ still derives $Agree_{EF}(H, Y)$, letting H qualify as the label.

   (i) $[X_i [[_{YP} ...Y...] [H [_{XP} ... X_i...]]]]$

This derivation is free from the label ambiguity problem that (30) potentially faces. It is not unreasonable to assume that natural languages may differ, under Option B, in which head (H or X) to move to provide the H-label in such a configuration. In these ways, we may be able to provide some independent motivation of Fukui & Takano's (1998, 2000) V/N-movement parameter.

[17] I would like to point out that (30) is exactly the structure that Zoerner (1995), Oshima & Kotani (2008), and Narita (2009c) propose for coordinate structures (where H is the coordinating particle like *and*). They propose that coordination involves a structure of the form in (i), where the Co(ordinator)-head iteratively move to the edge and project as many times as there are coordinand XPs.

   (i) $\{Co_i, \{AP, \{Co_i, \{BP, ... \{Co_i, \{YP, \{Co_i, ZP\}\}\}...\}\}\}\}$

I would like to leave these two options (Option A and Option B) open here, since the following discussion is compatible with either of them. They can provide accounts of the cases of externally merged Specs on a case-by-case basis.

2.4.   *No Feature-Percolation Necessary*

The discussion above showed that the proposed system can readily derive labeling facts in both cases of Head–Compl merger and Spec–Headed merger, in accordance with the definition of labels in (15) (repeated here).

(15)   *The Definition of Labels*

For any SO Σ, an LI H is the label of $\Sigma =_{def}$ H Agrees$_{EF}$ with the rest of the LIs within Σ.

Note that label as defined here is nothing more than a convenient well-defined shorthand for the LI prominently Agreeing$_{EF}$ with the rest of LIs for a given SO.

So far, we refrained from assuming any copying operation of some LI as a label-designator, as once assumed by Chomsky (1994, 1995) in his formulation of Merge as creating $\{\gamma, \{\alpha, \beta\}\}$, where γ is a copy of either α or β (see also Fukui 2006a, 2008, forthcoming). Nor did we assume any 'feature-percolation' mechanism that "projects" features of LIs to some phrasal node/set. To say the very least, our Agree$_{EF}$-based label theory does not need any such 'feature-percolation' mechanism as a necessary part of the theory of syntax. We can informally say that an LI H (say within α) 'labels' or 'projects' in an SO Σ as nothing more than a shorthand for H Agreeing$_{EF}$ with all the LIs contained in Σ, but the word 'label'/'project(ion)' is potentially misleading, since it strongly implies that some 'feature-percolation' carries up ("projects") the features of the head LI to nodes/sets it labels, an unnecessary stipulation that should be avoided unless thoroughly justified by empirical evidence (see Narita 2009a, b for a concrete proposal that 'feature-percolation' can be eliminated in human language; Samuels 2009a makes a similar argument in the domain of phonology). That said, it becomes questionable whether the notion 'label' itself has any significance in syntax, independent of its ingredient Agree$_{EF}$-relations. I will return to this point in due course.

Iteration of copies of Co is proposed by these researchers to capture, among other things, the availability of (optional) multiple pronunciation of the coordinator particle, as in (ii).

(ii)   a.   John will criticize [$_{CoP}$ Mary (and/or) Bill (and/or) Sue and/or Tom].
         b.   John will [$_{CoP}$ criticize Mary (and/or) praise Bill (and/or) humiliate Sue and/or admire Tom].

As pointed out by Oshima & Kotani, the topmost occurrence of Co can also surface in some languages, too, as in French (*et* A *et* B), Italian (*e* A *e* B), Russian (*mo* A *mo* B), Serbo-Croatian (*i* A *i* B), Japanese (A–*to* B–(*to*), A–*mo* B–*mo*), Godoberi (A–*la* B–*la*), etc.. These considerations further substantiate the plausibility of the syntax of coordinate structures in (i), which I claim to be an instantiation of Option B. See Narita (2009c) for further discussion on how such 'Project Both' structures can be linearized by means of cyclic Transfer.

3.   θ-Criterion in Optimal Labeling

This section discusses what the proposed Agree$_{EF}$-based label theory can tell us about the nature of semantic interpretation at C–I. The claim to be made is that the proposed theory deduces the effects of what has been called the θ-Criterion (Chomsky 1981).

3.1.   *Theta Principle*

Empirical evidence (Koopman & Sportiche 1983, Fukui 1986, Sportiche 1988, Kuroda 1988, Huang 1993) suggests that the PIAH-Conjecture (2), repeated here, holds for human language.

(2)   *PIAH-Conjecture*

If an SO {α, β} is labeled by the label of α, C–I interprets it as α θ-marking β (if α is a predicate category).

The PIAH is a hypothesis that every argument of a predicate category P is base-generated/externally merged within the projection of P. Thus, an internal argument DP of a verb V is base-generated into the complement of V, an external argument DP into the Spec of *v*, and so forth. We assume that (2) holds at C–I, and that syntax is designed to generate labels to serve for a proper interpretation conforming to (2).

However, it should be pointed out that not all labels enter into θ-theoretic predicate–argument relations. For example, consider subject raising to [Spec,TP]. In a simple clause with a transitive verb, like *John loves Mary*, the subject DP *John* is first base-generated at [Spec,*v*P], entering the external θ-role assignment by *v*, and then attracted by T to its Spec due to Case/agreement reasons (the 'EPP' phenomena, whose characterization has been under much controversy). The standard assumption is that T projects at the internal merge of the subject DP to T'. But this T-projection does not enter into a predicate-argument interpretation in any obvious way. Or in general, the labeling at internal Merge does not serve for thematic interpretation, as observed throughout the history of transformational grammar. What this means is that satisfaction of (2) is a sufficient but not a necessary condition for label-projection. Then, the presence of labels in syntax is considered not to be fully justified by the C–I-interface (in particular θ-theoretic) considerations like the PIAH-Conjecture (2), even though it maximally satisfies the label asymmetry condition by SM (3). If that is the case, then labels seem to be a potential candidate for 'imperfection', from the viewpoint of syntax's optimization for C–I, which is assumed to be prior to the secondary optimization for SM-interface conditions (Chomsky 2007a, 2008).

However, the discussion in section 2.3.2 showed that the labeling in the internal Merge cases is only derivative, and the structure-preserving projection of H is not triggered by Search$_{EF}$ by H but only derived from the conservativity of Agree over (internal) Merge (22), repeated here.

(22) *Agree is conservative over internal Merge:*

Copies of the same SO created by internal Merge share the same set of Agree-relations.

By contrast, the Agree$_{EF}$ from a predicate category P (say a verb) to its externally merged argument A (say a DP) is always established by a genuine derivational Search-operation triggered by the EF of P as a probe and targeting that of (the head of) A as a goal. This dichotomy rather suggests the possibility of explaining the partial transparency between labels and predicate-argument relations in a non-stipulative way.

I propose that the *Theta Principle* I suggest in (31) holds at the C–I-interface:

(31) *Theta Principle*

α Searches$_{EF}$ β in syntax. ⇔ α θ-marks β at C–I.

(31) says that only derivational search of EF, Search$_{EF}$ (a sufficient but not a necessary condition for Agree$_{EF}$) feeds θ-theoretic interpretation. I claim that (31) will exclude labeling of internal Merge from feeding any θ-theoretic interpretation, while still setting a general account of PIAH-effects in a non-stipulative way.

Consider a concrete example, an English sentence *Brutus stabbed Caesar*. Cast in a neo-Davidsonian event semantic representation, the interpretation is something like (32).

(32) a.   Brutus stabbed Caesar.
      b.   ∃*e*: Past(*e*) & Stab(*e*) & Agent(Brutus, *e*) & Theme(Caesar, *e*)
           (There is an event *e* such that it was in the past, it was a stabbing, its agent was Brutus, and its theme was Caesar.)

The syntactic derivation of this sentence will generate the structure in (33).

(33)
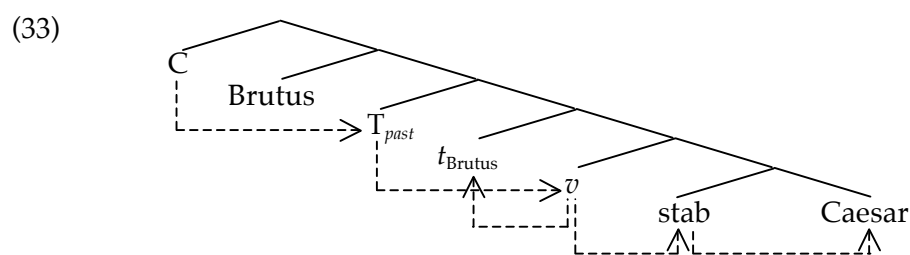
Derivational Search$_{EF}$-operations involved in this example are probings of EFs from *stab* to *Caesar*, from *v* to *stab*, from *v* to *t$_{Brutus}$*, from T to *v*, and from C to T, indicated as arrows in (33). Readers can easily notice that they are all what we need for θ-theoretic relations among LIs: *stab* assigns an internal Theme θ-role to the object *Caesar*, *v* assigns an caused-sub-event status to V (and/or, *v* 'verbalizes' the root *stab*, in terms of Distributed Morphology; see Halle & Marantz 1993, 1994 among others), and it also assigns an external Agent θ-role to (an occurrence of) *Brutus*, the past tense (T) is predicated of *v*, and C (finiteness, assertion of 'truth',

etc., which result in existential closure of an event variable) of T. These Search$_{EF}$-operations inductively result in the Agree$_{EF}$-relations necessary for appropriate labeling, but note that labels have no θ-theoretic effects at C–I in themselves, according to the Theta Principle (31). In general, the PIAH-Conjecture becomes a corollary of the Theta Principle. Call this Corollary 2:

(34) *Corollary 2*

      If the label of α θ-marks the label of β in the configuration {α, β}, the label of α necessarily projects (Chomsky 1995, 2000).

The Theta Principle (31) states that the label of α θ-marks the label of β at the C–I-interface only if it Searches$_{EF}$, and hence Agrees$_{EF}$ with, the label of β in syntax. As we have seen, the transitivity of Agree leads to the projection of the former by definition (15).

Chomsky (1995, 2008) among many others observes that for the most part θ-theoretic relations (or the 'Conceptual' aspects of interpretation) are determined by instances of external Merge (cf. *the duality of semantics* in the sense of Chomsky 2004, 2008). This observation is in fact a predicted consequence of the theory presented above. External Merge is a merger of two SOs that have been independent of each other, hence there is no Agree$_{EF}$-dependency between the two Merge-mates. The resultant structure is thus *prima facie* 'labelless', which is problematic from the viewpoint of (at least) the label asymmetry condition by SM (3). Thus at each application of external Merge, some Search$_{EF}$ must be involved in order to provide a label to the Merge-result. This is why external Merge always feeds some θ-theoretic interpretation. By contrast, for internal Merge the label is determined by an already established Agree$_{EF}$-relation, whose (perhaps only) function is to provide asymmetric labels for SM-linearization. Thus, if there is really a one-to-one correspondence between Search$_{EF}$ and θ-marking as stated in (31), the primacy of external Merge for θ-theoretic interpretation is in fact a natural consequence of the inner workings of the proposed Agree-mechanisms.

Thus, if we assume that the label asymmetry condition (3) requires each node/set within a given SO be unambiguously labeled, what we obtain from (31) is the following set of corollaries:

(35) *Corollary 3*

      External Merge always feeds θ-role assignment.[18]

(36) *Corollary 4* (to be strengthened)

      Internal Merge of α to β, leaving the original occurrence of α within β, *need not* feed any θ-role assignment by (the label of) β to α.

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[18]   External merger of expletives like *there* in English seems to be an apparent counter-example to Corollary 3, since it does not feed any obvious θ-theoretic interpretation at C–I. However, the problem is not so much lack of θ-theoretic interpretation as lack of *any* interpretation of expletives themselves. I propose that what is special about expletives is not that external merger fails to assign a θ-role to them but that their 'zero' lexical semantics consequently nullifies θ-roles assigned to them, hence they are still not counterexamples to Corollary 4.

The primacy of external Merge for θ-theoretic interpretation can be seen as a consequence of these corollaries, a welcome result of the proposed system of labeling (cf. the duality of semantics).

3.2.   *Economy and θ-Theory*

In the preceding discussion, I outlined how the proposed theory can be connected to considerations on C–I-interpretation. Taking the PIAH-facts as a clue, I proposed that Search of EFs from α to β is in one-to-one correspondence with the predicate-argument asymmetry between α and β at C–I. So far, I have largely refrained from discussing the potential overgeneralization possibly resulting from the proposed system. For example, if EFs are ubiquitous, appearing on all LIs, and $Search_{EF}$ alone can trigger θ-role assignment at C–I, then what prevents an LI X from assigning identical θ-roles to more than one element? Or what prevents an LI Y from receiving more than one θ-role? What prevents a θ-role of an LI X from being assigned to an LI Y which is located far from X, while the locality of θ-role assignment is typically restricted to X's 'governing domain'? (See Chomsky 1981, Marantz 1984.) Or what restricts the θ-domain even more locally, excluding, for example, the possibility of 'exceptional θ-marking' comparable to exceptional Case-marking (ECM)? Potential worries are abundant.

My answer to these questions is a minimalist one: It is principles of computational efficiency that crucially restrict modes of $Search_{EF}$ in syntax, and thus of θ-role assignment at C–I. Let me articulate this view.

Minimalist inquiry is guided by the core intuition that human language is the simplest possible computational system whose function is to generate an infinite range of linguistic expressions subject to interpretation by performance systems. Computations by such a system are expected to be optimal, each applying as small a number of operations as possible, and excluding anything unnecessary. Then, we expect that computations in syntax obey an economy constraint like (37):

(37)   *Principle of Derivational Economy*

   If syntax can generate two convergent derivations D_1 and D_2 for the same interface interpretation from one and the same lexical array, and if D_1 consists of all the derivational steps (operations) contained in D_2 plus some more steps, then the more economical derivation D_2 will block D_1.

Whether (37) requires some global computation (as in Chomsky 1995; see also Fukui 1996) or its effect is restricted to some well-defined computational subdomains of a given derivation (such as phases; see Chomsky 2000, 2001; see also Collins 1997) is under controversy, an issue that will not concern us here. What is important to the present discussion is that any version of (37) will ensure that (38) holds (either globally or locally within each phase).[19]

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[19]   It is proposed by van Riemsdijk (2008) that a third-factor principle that tends to avoid a consecutive sequence of identical elements, what he calls *Identity Avoidance*, is also at work in human language (Swiss relative clauses, OCP effects in SM, etc.; see also N. Richards

(38)   *Corollary of No Redundant Search*

If Agree$_F$(X, Y) is established, X can no longer Search$_F$ Y redundantly.

Redundant application of Search between the same probe and goal does not gain any new Agree-relation, and is totally futile from a computational point of view, thus excluded by (37).

Now I demonstrate that (37) and (38) derive a number of further favorable consequences, answering the questions raised at the beginning of this sub-section.

First, (38) will let us strengthen Corollary 4 in (36) as:

(39)   *Corollary 4* (strengthened)

Internal Merge of α to β, leaving the original occurrence of α within β, *cannot* feed any θ-role assignment by (the label of) β to α.

As we have already concluded in (36), any element G moving to the edge of P has already entered Agree$_{EF}$(P, G), thus no further Search$_{EF}$ is required at internal Merge, at least for asymmetric labeling purposes. Moreover, an already established Agree$_{EF}$(P, G) precludes P from Searching$_{EF}$ G redundantly, due to (38). Consequently, θ-role assignment from P to G at C–I, which is in one-to-one correspondence with P's Search$_{EF}$ of G, is also precluded.

Moreover, another corollary of (38) is that θ-marking is always to the head/label of the complement SO, which can be stated as in (40):

(40)   *Corollary 5*

If an LI H is externally merged with an SO Σ and act as a probe for Search$_{EF}$, H always Searches$_{EF}$ (and hence θ-marks/'s-selects') the label of Σ.[20]

Consider the derivation of (19) again, summarized here as (41).

(41)   a.   Merge(X, Y) $\rightarrow$ {X, Y}.
        b.   X Searches$_{EF}$ Y. $\rightarrow$ Agree$_{EF}$(X, Y) holds,
              leading to the projection of X.
        c.   Merge(Z, {X, Y}) $\rightarrow$ {Z, {X, Y}}.
        d.   Z Searches$_{EF}$ X. $\rightarrow$ Agree$_{EF}$(Z, X) holds.
              Given Agree$_{EF}$(Z, X) and Agree$_{EF}$(X, Y), the
              transitivity of Agree (12) derives Agree$_{EF}$(Z, Y),
              leading to the projection of Z.
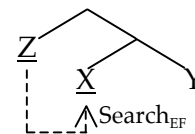
2007, Boeckx 2008: sect. 3.5).

   (i)   *Identity Avoidance* (van Riemsdijk 2008)
          *XX

(38) can also be seen as another manifestation of Identity Avoidance.

[20]   Note that, according to Option A in section 2.3.3.1, in the case of externally merged Specs as in (27), [Spec,YP] becomes a 'second complement' for the purpose of Agree after Transfer of P's complement XP.

If the derivation reaches the point (41d), Z cannot Search$_{EF}$ Y anymore due to the already established Agree$_{EF}$(Z, Y) and the Corollary of No Redundant Search (38). Moreover, Z cannot Search$_{EF}$ Y at the point of (41c), either. Suppose Z Searches$_{EF}$ Y instead of X at the point of (41c) as in (42d) below:

(42) a. Merge(X, Y) → {X, Y}.   (=(41a))
      b. X Searches$_{EF}$ Y. → Agree$_{EF}$(X, Y) holds,
          leading to the projection of X.   (=(41b))
      c. Merge(Z, {X, Y}) → {Z, {X, Y}}.   (=(41c))
      d. Z Searches$_{EF}$ Y. → Agree$_{EF}$(Z, Y) holds.
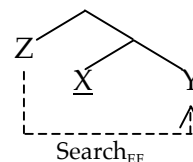
Note that Z still cannot project, given the absence of Agree$_{EF}$(Z, X). Thus, due to the label asymmetry requirement that all SOs be labeled, Z is required anyway to Search$_{EF}$ X in addition. However, this derivation, comprising Z's Search$_{EF}$ of both X and Y is less economical than the one in (41) where Z Searches$_{EF}$ only X but not Y. Thus, the principle of derivational economy (37) ensures that the more economical derivation in (41) wins, blocking the less economical one in (42).

This illustration shows that at any external merger of a projecting LI X and a phrase YP labeled by an LI Y, it is always the most economical for X to Search$_{EF}$ Y for the purpose of labeling the Merge-result. Principles of derivational economy rule out any other, less economical derivations. Therefore, any externally merged LI is forced to Search the EF of the label/head of its sister/complement. Hence Corollary 5 holds.

Once Corollary 5 is established, we can further derive (without any further stipulation) Corollary 6 (43), a canonical observation that at least goes back to Chomsky (1986).

(43) *Corollary 6*

      There is no 'exceptional θ-marking'. (An LI H cannot Search$_{EF}$/θ-mark into the Spec of its complement.)

Furthermore, insofar as we keep away from the possibility of sideward movement (Nunes 2001, 2004; see fn. 21), we also achieve Corollary 7.

(44) *Corollary 7*

      No LI can be θ-marked by two distinct LIs.

Suppose an LI A (say the label of AP) receives a θ-role from P. Then, it follows from the Theta Principle (31) that P Searches$_{EF}$ A at some point of the derivation. Given Corollary 5 (40) that Search$_{EF}$/θ-marking is always to the head/label of the complement, it must be the case that P takes AP as its complement (that is, P is externally merged with AP). In order for a distinct LI Q to assign its θ-role to A, then AP must also be in Q's complement, too, but this is impossible: internal Merge of A(P) to the edge of some SO Σ must yield a set, namely {A(P), Σ}. A(P)'s sister will be always a phrase Σ, which can never be Q, an LI. Thus, there can be no LI Q distinct from P that can take AP as its complement, Searching$_{EF}$/θ-

marking A.[21]

Consequently, we now have an explanation for the observation that θ-role assignment is indeed an interpretive phenomenon tied to external Merge. Another important consequence is that there is no 'movement into θ-position' (and hence control cannot be reduced to movement; see Brody 1999, 2002, Culicover & Jackendoff 2001, Landau 2003 for the latter point).[22] If there is really any instance of θ-role assignment by a predicate category P to a moved category G (or its head) which P already Agrees$_{EF}$ with, as sometimes claimed to be possible by not a small number of researchers (Bošković & Takahashi 1998, Hornstein 1998, 1999, 2001, Boeckx & Hornstein 2003, 2004), then my proposal fails, and it becomes a curious question why in these cases such a 'redundant' Search$_{EF}$ is allowed to be applied, violating otherwise natural principles of computational efficiency (37)/(38). Crucially, note that in our theory 'movement into θ-position' is banned (Corollary 4 (39)) exactly for the same reason why the 'exceptional θ-marking' is absent (Corollary 6 (43)), why θ-marking/s-selection is always to the complement head (Corollary 5 (40)), why external Merge but not internal Merge always feeds some predicate-argument structure (Corollary 3 (35)), and why the effects of duality of semantics hold at all. Therefore, any advocates for 'movement into θ-position' who wish to deny Corollary 4 (39) or Corollary 7 (44) must carry a heavy burden of proof for their selective disapproval of the proposed system.

What we can conclude from the discussion is that principles of computational economy severely constrains the possible mode of Search$_{EF}$, and hence that of θ-marking.[23] The theory, when tied with the Theta Principle (31), makes a number of strong (and apparently correct) predictions about the possible range of θ-theoretic interpretations at C–I, which as a whole constitute the effects of what has been called the *θ-Criterion*. We find it of particular significance that our theory essentially deduces the θ-Criterion as a corollary of principles of computational efficiency, maximally conforming to the SMT. Several important ramifications of the proposal are to be discussed in the next section.

4.   Reverse-Engineering Interfaces

The minimalist program is a pursuit of *principled explanation* of language, which attributes the properties of syntax to computational optimization principles and interface conditions that language must meet to be usable at all (see Chomsky 2008; see also Narita 2009d). What we discussed in the previous discussion was a

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[21]   As noted above, this discussion leaves open the possibility of an LI Q Searching$_{EF}$/θ-marking the label of its complement AP that is introduced there by sideward movement (Nunes 2001, 2004), if such an instance of Merge is allowed in syntax.

[22]   Again with a possible exception of sideward movement cases. See fn. 21.

[23]   As Juan Uriagereka (p.c.) points out, it is as if syntax can calculate all the possible paths for Agree/Search in a given derivation and choose the path contains the least derivational steps, a situation quite reminiscent of Hamilton's Principle in physics, according to which nature (light, motion, etc.) chooses the path which requires the least effort. See Fukui (1996) for a nice summary of minimization principles in various sciences potentially relevant to biolinguistics.

hypothesis as to how syntax satisfies the constraints it obeys via Agree$_{EF}$. Now we would expect that this study would let us know more about the nature of some such constraints.

4.1. Full Interpretation of Syntactic Derivations

The Theta Principle (31) says that C–I interprets Search$_{EF}$, the essential trigger for the labeling effects, as the one-to-one instruction for θ-marking.

(31) *Theta Principle*

α Searches$_{EF}$ β in syntax. ⇔ α θ-marks β at C–I.

It is "as if syntax carved the path interpretation must blindly follow" (Uriagereka 1999: 275, 2002: 64, Hinzen 2006: 250, Chomsky 2007a: 15).

Note that according to the current proposal, Search$_{EF}$, the θ-marking indicator, is itself a strictly derivational operation, hence *not part of linguistic representations* in any sense. There is evidence supporting this important consequence of our theory: Uriagereka & Pietroski (2002) observe that no known languages have formatives that exactly corresponds to neo-Davidsonian θ-predicates like Theme, Agent, and so on. They write:

> We find it significant that no language we know of has lexical items synonymous with the (meta-language) expressions 'Theme', 'Agent', 'Benefactive', and so on. One can say that there was a boiling *of* the water *by* John; but 'of' and 'by' do not *mean* what 'Theme' and 'Agent' mean. This is of interest. Languages have words for tense, force indicators, all sorts of arcane quantifications and many others. Yet they do not lexically represent what seems to be a central part of their vocabulary. […] We think this sort of fact reveals a simple truth. *θ-roles are not part of the object-language.*
>
> (Uriagereka & Pietroski 2002: 278)

Our Theta Principle can make perfectly good sense of their important observation, thus render it as its supporting evidence: θ-marking emerges solely as a result of Search$_{EF}$, a purely syntax-internal, strictly derivational operation, not a representation, hence there are no representational counterparts of θ-roles in the humanly possible Lexicon.

The hypothesis that the C–I-interface can actually 'see' the purely syntax-internal derivations, such as Search$_{EF}$, is a non-trivial claim. This hypothesis is quite congenial to Epstein & Seely's (2002) proposal (extended to some degree) that syntax interfaces with C–I and SM at each and every application of rules in syntax, or it is as if Transfer (the interfacing operation) is 'buried in' syntactic rules themselves.[24]

It is important in this context to note that Pietroski (2005, 2007, 2008, to appear) makes a very interesting hypothesis on the syntax/C–I-interface, what he calls *Predicate Conjunctivism*. Briefly put, its claims can be summarized as follows

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[24] Epstein & Seely's (2002) original claim was only that Spell-Out, a 'SM-branch' of the Transfer which stripes off SOs to the SM-interface, is buried in each application of Search/Agree.

(45)   *Predicate Conjunctivism* (Pietroski 2005, 2007, 2008, to appear)
   a.   Each LI is an atomic *monadic predicate*.
   b.   Merge signifies *conjunction* of predicates ('&') at C–I.
   c.   Each SO of the form {α, β} is a monadic predicate of a complex sort, whose meaning is determined by conjoining the monadic predications α and β.

According to this view, each LI is a monadic predicate that constitutes an instruction for the C–I system to fetch a corresponding atomic concept. For example, *red* fetches an atomic concept RED and *ball* fetches an atomic concept BALL, each of which can be used as a monadic predicate, being predicated of something. Similarly, a phrasal Merge-composite *red ball* just means a composite concept that can be used as a monadic predicate satisfiable by something both red and ball. Here, Merge signifies conjunction ('&') of two monadic predicates (RED and BALL).

Returning to our previous example *Brutus stabbed Caesar* (32), repeated here as (46) with its structure and interpretation, we see how nicely the target event-semantic interpretation can be derived by recursive application of predicate conjunction *á la* Merge and θ-marking *á la* Search$_{EF}$.

(46)   a.   Brutus stabbed Caesar.
       b.   ∃e: Past(e) & Stab(e) & Agent(Brutus, e) & Theme(Caesar, e)
             (There is an event e such that it was in the past, it was a stabbing, its agent was Brutus, and its theme was Caesar.)
       c.
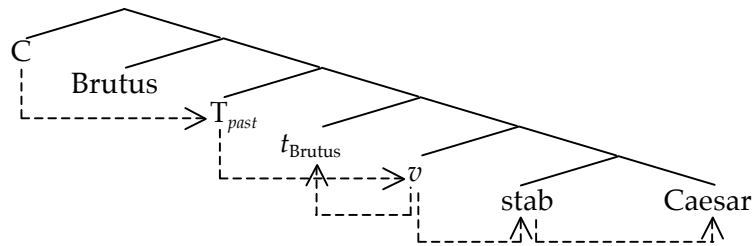
Indeed, if syntax utilizes Merge as the sole structure building operation, it is natural to expect that this single operation corresponds to the sole semantic interpretation rule, given that syntax is optimized for C–I insofar as the SMT holds. As Merge is symmetric, this semantic rule is expected to be a symmetric one, too, thus predicate conjunction as proposed by Pietroski is really a good (arguably the best) candidate. Further, θ-marking, an asymmetric relation which is now claimed to have a one-to-one correspondent in syntax (Search$_{EF}$), can alter the argument nPs to monadic predicates that can then be conjoined with the higher event predicates by predicate conjunction. Further addition of adjuncts, concatenated with SOs without involving θ-marking (see Chametzky 2000, Hornstein & Nunes 2008) will just signify simplistic conjunction, as predicted by Predicate Conjunctivism.[25]

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[25]   A common assumption is that adjuncts are SOs that do not receive any θ-roles. If such a standard view is on the right track, then our Theta Principle (31) predicts that no Search$_{EF}$ is

(47) a.   Brutus stabbed Caesar **quickly**.
      b.   ∃*e*: Past(*e*) & Stab(*e*) & Agent(Brutus, *e*) & Theme(Caesar, *e*) &
             Quick(*e*)
             (There is an event *e* such that it was in the past, it was a stabbing, its
             agent was Brutus, its theme was Caesar, and it was quick.)

(48) a.   Brutus stabbed Caesar quickly **with a knife**.
      b.   ∃*e*: Past(*e*) & Stab(*e*) & Agent(Brutus, *e*) & Theme(Caesar, *e*) &
             Quick(*e*) & **With-a-Knife**(e).[26]
             (There is an event *e* such that it was in the past, it was a stabbing, its
             agent was Brutus, its theme was Caesar, it was quick, and it was with
             a knife.)

These observations point to the conclusion that Merge transparently carves the
path for predicate conjunction at C–I, with the help of θ-marking, carved by
Search$_{EF}$. Combined with the Theta Principle, now we have the following two
fundamental C–I-interpretation rules, each of which strictly corresponds to one
of the fundamental operations in syntax, Merge and Search$_{EF}$.

(49) a.   Merge(α, β) in syntax. ⇔ α(*e*) & β(*e*) at C–I.
      b.   X Searches$_{EF}$ Y in syntax. ⇔ Y(*e*) → θ$_X$(Y, *e*) at C–I.

It is as if each application of these syntactic operations interfaces with C–I. Or, even
more radically put, it may be that these syntactic operations themselves *are* C–I-
interpretation operations (Merge *is* '&', Search$_{EF}$ *is* θ-marking, etc.).

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involved in cases of adjunct merger. As a consequence, adjoined structures should be
labelless. (See already Hornstein & Nunes 2008, Boeckx 2008 for such a view.) But this con-
sequence seems problematic for our hypothesis that SM (in particular linearization) requires
asymmetric labels (3). If adjoined structures are labelless, SM cannot assign to them linear
ordering based on label asymmetry, thus they would be 'too symmetric' to linearize.
However, if we assume with Chomsky (2004) that adjuncts are introduced by a distingu-
ished *pair-Merge* operation, creating ordered pairs of the form ⟨α, β⟩ (cf. Saito & Fukui 1998).
(In order to distinguish it from pair-Merge/adjunction, Chomsky sometimes uses the term
set-Merge to refer to the ordinary Merge, creating a simple unordered set {α, β}.) Ordered
pairs are intrinsically asymmetric, and pair-Merge straightforwardly weaves the headed/
adjunct asymmetry in the resultant ordered pair. Then, even if adjoined structures might
lack label asymmetry, they still exhibit *pair-Merge* asymmetry. Then, it is not implausible to
assume that phonological operations can utilize this asymmetry, avoiding the linearization
problem raised by the lack of labels. Consequently, it is necessary to slightly revise our ori-
ginal definition of labels (15) as follows:

   (i)   *The Definition of Labels*
           For any SO Σ, an LI H is the label of Σ =$_{def.}$ H Agrees$_{EF}$ with the rest of the LIs *set-
           Merged into* Σ.

Another possible way out is to reanalyze the apparent adjunct-like elements as either a head
or a Spec of some functional category (*the Cartography hypothesis* by Cinque 1999, 2002, Rizzi
2004). In fact, any of the modifier categories can be syntactically analyzed either as a true
adjunct, or a Spec or a head of some abstract functional category. The decision among these
options should be made by biolinguists on a case-by-case basis, I believe.

[26]   More accurately, this conjunct should be regarded as a shorthand for something like
       With(Knife, *e*) or ∃*x*: Knife(*x*) &With(*x*, *e*). See Pietroski (2005) for details.

Once we find that *some* syntactic operations transparently instruct corresponding C–I-interpretations, minimalists want to ask *to what extent* such a transparent syntax/C–I mapping holds. Evidently, the strongest answer would be: "maximally" or "to the fullest extent." This desideratum can be rephrased in terms of an extended version of the principle of Full Interpretation (Chomsky 1986), which can be called *Derivational Full Interpretation* (DFI).

(50) *Derivational Full Interpretation* (DFI, the strongest version)

Every syntactic operation correlates with a corresponding interpretation at C–I.

This is, arguably, the strongest possible hypothesis regarding the transparency between syntax and C–I. (Can we make any stronger sense of C–I optimization than this? Perhaps not.) Then, we are interested in how close the actual syntax satisfies this desideratum.[27]

Potential counterexamples are abundant. Interestingly, most of them come from various 'virus-checking' proposed in the vast literature. The clearest example is Search with respect to φ-features: It is predominantly assumed that the functional categories v^* (or V) and T (or C) are associated with 'viral' unvalued φ-features that are forced to Search_φ their matching goal D(P)s within their c-command search domain.[28] In general, φ-feature-checking do not feed any obvious C–I-interpretation, and are hence potential counterexamples to our desideratum (50). The same holds for many other virus checking Search-operations proposed in the past literature.

However, it should be noted in this context that, without stipulation, there is no obvious conceptual necessity for a perfect 'sound'-'meaning' linking system like human language (insofar as the SMT holds) to employ anything like 'viral' uninterpretable features such as φ-features and corresponding valuation operations that check them off. Thus, the existence of viruses is an apparent 'imperfection' of human language, hence a potential SMT-killer. This worry in fact goes back to at least as early as Fukui & Speas (1986), who first expressed the hypothesis that viruses on functional categories can be parametrically absent in some I-languages, suggesting Japanese as one of the clearest instantiations of "no virus"-type languages. See especially Fukui's subsequent works (1986, 1988, 2006a, 2008, and papers collected in 2006b) for the apparently plausible and "rarely challenged"[29] hypothesis that Japanese lacks any viruses in its Lexicon (see also Kuroda 1988, 1992, Hoji 2003). Just consider numerous facts attested in this language, such as the lack of morphological subject-verb agreement, the lack of determiners, the lack of singular/plural morphological distinction on nominal inflection and hence the generic 'mass'-like characters of nouns, the lack of real

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[27] It would also be a desired ingredient of Uriagereka's (2008) *co-linearity thesis*. See also Narita (2009d) for discussion.

[28] It does not matter whether the viral φ-features of T are inherited from C or those of v^* are inherited by V, as proposed by Chomsky (2007a: 2008).

[29] "Rarely challenged," not really because there exist a number of serious counterargument against it, but rather because it is quite descriptively convenient to postulate such viruses. See Narita (in press) for discussion.

pronouns and anaphors, the lack of expletives, the possibility of multiple Specs (the lack of the 'one-Spec-per-one-head' constraint), the lack of WH-movement (WH-in-situ), the existence of optional scrambling, multiple nominatives, multiple genitives, all of which point to the need of reconsidering the universalist conception of viral features ($u\varphi$, uWH, etc.), which is now seen as both conceptually and empirically unfavorable. See Narita (in press) for discussion.[30]

Chomsky (1995: chap. 4) once tried to explain the existence of viruses in syntax by stipulating that such viruses are there to be used as the actual triggers of dislocation operations (Move, Attract, internal Merge) ubiquitous in natural languages. This proposal has numerous followers in the field. However, once we emancipate ourselves from such a stipulation and instead assume with Chomsky (2008: 140) that undeletable EFs of LIs allow internal Merge to apply (as) freely (as external Merge), we cannot blame dislocation for the source of viruses anymore.

Thus, if 'virus checking' (such as φ-feature agreement) is really a syntactic operation, as standardly assumed, then our strongest possible conception of Full Interpretation (50) immediately fails, yielding an apparent departure from the SMT. Logically speaking, then, we should drop either (i) the assumption that (50) is a viable hypothesis, *or* (ii) the assumption that virus checking is a syntactic operation.

As for the first possibility, Indeed, DFI (50) is very easy to withdraw (only the SMT favors (50)), but minimalists want to keep to it as close as possible, keeping the departure from the SMT minimal. Recall the hypothesis that syntax is (secondarily) optimized for SM-purposes, too (see section 4.2). Then, it might be that such virus checking operations, although semantically uninterpretable, actually serves for SM-optimization in some sense. Recall further that virtually all the virus checking operations proposed in the literature are provided with some manifestation of morphological agreement as their evidence. Then, such valuations apparently have some morpho-phonological consequences. Then, we might be able to keep a weaker version of DFI, allowing syntax to serve not only for C–I-interpretations but also for some SM interpretations, too.

(51) *Derivational Full Interpretation* (DFI, a weaker version)

Every syntactic operation correlates with a corresponding interpretation either at C–I or at SM.

There is still a strong sense in which (51) conforms to the SMT, given that language must satisfy both SM and C–I usability conditions. As is to be discussed at length below in section 4.2, externalization at SM might be quite a complex task, which may require a lot of 'computational tricks'. Then it is reasonable enough to suppose that there can be some syntactic operations which are responsible mainly for SM-purposes, such as realization of agreement morphology: for example, Agree$_\varphi$. Then a revised, second best hypothesis of DFI (51) might still be a tenable constituent of optimal syntax, while allowing virus valuation to be syntactic operations.

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[30]   See also Chandra (2007) and Hornstein (2009).

What is more interesting is to pursue the second possibility, namely to keep the strongest DFI in (50) as such and rather abandon the assumption that morphological agreement/valuation (such as what is called Agree$_\varphi$/Search$_\varphi$) itself is an independent syntactic operation. For example, we might want to entertain the hypothesis that morphological virus-checking is in fact a post-syntactic, morpho-phonological manipulation. See, for example, Bobaljik (2008) for a quite relevant argument for the view that morphological φ-feature valuation is a post-syntactic morpho-phonological operation. Or, Chomsky's (2007a, 2008) recent proposal that Agree/Search is derivationally simultaneous with Transfer might be quite relevant (see also Hiraiwa 2005 and Boeckx, to appear). According to this hypothesis, all the virus valuation operations take place at the phase level, with a designated phase head LI constituting the sole computational locus of them. Among these synchronized operations is the interfacing operation Transfer, sending off the phase-interior domain (the complement of the phase head) to the C–I- and SM-interfaces. The phase head manipulates the phasal syntactic representation as much as needed (maybe by inheriting its viruses to the next lower non-phase head LI; see Chomsky 2007a, M. Richards 2007) in order to attain successful valuations for viruses at Transfer. In this hypothesis, then, in a certain sense, virus valuation really is *just a part of the Transferring operation*: Transfer assigns whatever value is available to the unvalued features within the phase-interior domain, maybe in accordance with some heuristic locality constraints like relativized minimality, without invoking valuations as *operations* independent of Transfer itself. Interfacing (for which Transfer is claimed to be responsible) is virtually conceptually necessary (that's the whole function of syntax to begin with), thus if valuation to unvalued features can be reduced to this kind of 'repair strategy' at Transfer, then it might still be possible to keep the strongest possible formulation of DFI (50) as such, maximally conforming to the SMT.[31] Note that even in this line of approach, Search$_{EF}$/θ-marking should be necessarily a syntactic operation. Then, the second hypothesis sketched here amounts to the reformulation of syntactic Search-operations as primarily responsible for carving C–I-interpretations (such as θ-marking), while removing most of its alleged responsibility for virus checking.

It is customary to assume that it is the principle of Full Interpretation in the rather traditional, representational form (Chomsky 1986 et seq.; M. Richards 2007) that categorically resists uninterpretable/unvalued features remaining at the C–I (and SM) interface, and thus necessitates virus-checking (deletion of uninterpretable features) as syntax-internal operations. The representational version of Full Interpretation can be stated as (52):

(52) *Representational Full Interpretation* (RFI; cf. Chomsky 1995: 194)

SOs that are subjected to interpretation must be constituted entirely of interface-legitimate objects (which crucially excludes unvalued/uninterpretable features).

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31   See Boeckx (to appear) for an intriguing suggestion essentially pointing to the effect of this proposal.

Then, in a sense, there is a certain tension between the derivationally revamped Full Interpretation (DFI) in its strongest form (50) and RFI (52), as long as we grant the existence of uninterpretable features (for want of a better explanation of their origins; see Narita (in press) for further discussion). The former does not want any syntactic operation to be responsible for virus-checking, while the latter does. We have seen that we can find a point of compromise by (i) weakening DFI to (51), letting it speak to interpretive effects at SM, or (ii) eliminating virus-checking as an independent syntactic operation.

Only further empirical inquiry can advise us to decide which version of Full Interpretation is on the right track (both DFI (50)/(51) and RFI (52), or just one or the other, or none).[32] Before leaving this discussion, I would like to point out that the rather pervasive *duality of semantics* (Chomsky 2001, 2004, 2008, 2007a) lends further empirical support to DFI (either (50) or (51)). Recall that we adopted Pietroski's Predicate Conjunctivism and claimed that Merge corresponds to conjunction ('&') (see (45)). Given that the principle of derivational economy (37) excludes any superfluous steps in syntactic derivations, we naturally expect that any application of internal Merge is tied to some interpretation beyond conjunction, since the first application of Merge, i.e. external Merge, is enough to instruct that much. Moreover, recall the corollary of our system that internal Merge (in contrast to external Merge) *cannot* feed any θ-marking. If DFI holds and every syntactic operation should correlate with non-trivial interpretation at C–I (or SM), then, the prediction is:

(53)   *Corollary 8*

Internal Merge correlates with interpretation beyond conjunction and θ-marking.

(53) is allied to Chomsky's hypothesis on the duality of semantics, which postulates that the dichotomy of external versus internal Merge correlates with that of 'conceptual'/'deep' versus 'intentional'/'surface' semantics. Chomsky (2001: 34ff.) specifically proposes, building on the insight from Fox (2000) and Reinhart (1997, 2006), that any application of internal Merge (other than those necessary for convergence, e.g., virus-checking) is required to yield 'surface interpretation' *INT*. INT is supposed to include intentional and discoursal effects like scope, topic-comment, old/new information, definiteness and specificity, context-confinement, force, so on so forth, which seems to be characterizable only as 'anything but θ-theoretic interpretation'. Consider further Reinhart's (1997, 2006) and Fox's (2000: 75) argument that (optional) application of QR must result in scope shifting of quantificational expressions. Given that QR is an instance of

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[32]   One might argue that scrambling of the Japanese sort constitutes counterevidence to the stronger DFI (50) (though not to the weaker one (51)), given that this operation is often claimed to be 'semantically vacuous' (though phonologically not) in many cases (see Saito 1989 among others; see also Fukui & Kasai 2004). However, see Kuno (2003) for a view that scrambling in Japanese, though free from any ancillary feature-checking, nevertheless feeds an interpretation (sometimes hard to detect as such) where the root clausal constituent is interpreted as being 'predicated of' (or being 'about') the scrambled constituent (presumably due to the interface economy of the Fox-Reinhart sort, as Kuno argues; see below).

internal Merge that has no phonetic effect, either version of DFI, (50) or (51), predicts that QR must have a non-vacuous interpretive consequence at C–I, along the line with Corollary 8. These results can be readily recaptured as another consequences of the far-reaching derivational syntax-semantics transparency, namely DFI, thus we don't have to postulate an independent condition specific to optional movement anymore, a further simplification of UG.

4.2.   On the Label Asymmetry Condition

Given the discussion above, we may want to conclude that the notion 'label' itself is of relatively small significance to the theory of syntax. It is just a well-defined shorthand for an LI prominently Agreeing$_{EF}$ with other LIs (definition (15) is repeated here).

(15)   *The Definition of Labels*

   For any SO Σ, an LI H is the label of Σ =$_{def}$ H Agrees$_{EF}$ with the rest of the LIs within Σ.

Labels themselves have no obvious interpretation at C–I, though they might derivatively constitute convenient instructions to SM-linearization. Only Search$_{EF}$ is proposed to feed interpretation at C–I.

   My proposal was partially based on the assumption that the label asymmetry condition at the SM-interface (possibly among others) requires that each node/set within a legitimately interpretable SO be unambiguously labeled in order to be linearizable at all, (3) repeated here.

(3)   *Label Asymmetry Condition*

   SM-linearization works properly for a syntactic node/set only if one and only one label is defined for that node/set.

And I worked out how syntax computes derivations conforming to this constraint, crucially excluding labelless structures and ambiguously labeled (Project Both) structures. Readers can readily interpret this hypothesis in *teleological/functional* terms: the SM-interface for some yet poorly understood reason came to have such constraints as (3) largely independently of syntax, and syntax is essentially made to serve to provide only expressions conforming to them. In this view, the properties of syntax are explained essentially as a *function of interface constraints*. Note that the very reason why human SM happened to adopt such linguistic particulars as the label asymmetry condition (3) remains unaccounted for. For further inquiry, if it is ever possible, much evidence seems to be required to be drawn from what we call comparative ethology (Hauser *et al.* 2002, Fitch *et al.* 2005).

   I am happy to leave the possibility of this sort of reasoning, as it might actually turn out to be correct. But in addition to this, I would like to add that another non-teleological/non-functional explanation of the label asymmetry requirement is readily available, too. Let me briefly sketch this alternative below.

Chomsky (2007b: 17) suggests that "language evolved, and is designed, primarily as an instrument of thought, with externalization [satisfying SM conditions] a secondary process." Assume that much is on the right track. Then, it is not unreasonable to assume that, insofar as there is no need to externalize them, syntax can freely feed linguistic expressions to the C–I system, without bothering to serve for SM conditions, the label asymmetry condition being among them. Then, such 'language of thought' will entertain an infinite range of symbolic thoughts, some of which might be unexternalizable and hence usable only when confined to the C–I domain, for example, symbolic expressions that leads to ambiguously labeled structures[33] or labelless structures. Derivations of such expressions that would 'sound crazy' if sent to SM may contain LIs being multiply θ-marked, LIs θ-marking multiple LIs, LIs θ-marking each other, LIs θ-marking themselves, Ds θ-marking Vs, Ps θ-marking Ts, Ns without morphological case, Ps specified as first person singular masculine, so on so forth.

In such a view, syntax is really unbounded, infinitely generating structures, whose expressive potentials may well be in many ways far beyond the confinement by SM externalizability conditions. Rather, the phonological system is only "doing the best it can to satisfy the problem it faces: to map to the SM-interface SOs generated by computations that are "well-designed" to satisfy C–I conditions" (Chomsky 2008: 136). Externalization is quite a complex task, required to satisfy a number of modality dependent restrictions such as particulars of vocal tracts, features of auditory and/or visual receptors, the range of motion of gestural muscles, the temporally bounded nature of motion/perception, and so on, which must be largely shared by members of the linguistic community each individual belongs to, for the purpose of more or less successful communication for which there must have been some advantage for SM to be adapted. Among such constraints must be the linearization requirement, namely that structures, generated by syntax in full service of C–I optimization, be mapped to temporal sequences of phones/signs from which the target C–I-interpretations are more or less recoverable. The phonological system does the best it can to meet this, presumably devising various "computational tricks" [34] (Chomsky 1995: 162). The successful phonological system would achieve this task by making use of whatever is readily available in syntactic derivations, a rather likely candidate for which is the set of Agree$_{EF}$-relations, which is generated primarily as a byproduct of Search$_{EF}$ that carves θ-marking at the C–I-interface. Asymmetric labels are correspondingly devisable relatively easily with such

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[33]   Narita (2008) suggests that QR actually creates instances of such primarily unpronounceable ambiguously labeled structures, since a raised QP θ-marks its sister (nuclear scope) as its second argument and hence projects (see Pietroski 2003, 2005 for a neo-Davidsonian analysis of QR semantics; see also Hornstein & Uriagereka 1999, 2002). According to Narita, sentences involving QR are still pronounceable by SM with some tricks, either by pronouncing the lower copy of QP (as in English) or by systematically forgetting one of the ambiguously projected labels (as in Hungarian). If he is right, then some such phonologically problematic structures are still usable in human language, barely satisfying SM-conditions, suggesting the primacy of C–I optimization over SM satisfaction.

[34]   That is, the "features that enable [human language, though designed for elegance, not for use] to be used sufficiently for the purposes of normal life" (Chomsky 1991: 49). See also Fukui (1996) and Ishii (1997) for intriguing discussion in relation to 'discrete optimization problems studied in the field of discrete mathematics/theoretical computer science.

computational tricks as the binary branching constraint on Merge and transitive extension of Agree. So might some label asymmetry-based linearization mechanism(s) be.[35] To the extent that the syntax/C–I mapping is trivial (as the Theta Principle (31) and DFI (50)/(51) suggest), it is conceivable that these are computational tricks primarily in service of SM-optimization matters for the working of externalizable syntax, too.

Thus, there are two pictures presented here as to the relation between syntax and the SM-interface. One sees syntax and its computation as a function of the satisfaction of SM-interface constraints such as the label asymmetry condition (in addition to the C–I constraints). The other sees the relation the other way round, suggesting that the computational properties of syntax in its full service of C–I optimization actually pose a severe constraint on what the 'possible SM-strategies' might be, allowing label asymmetry and others as viable options. The two hypotheses differ in their predictions as to the explanatory burden that syntax can carry. Again, only empirical considerations can eventually advise us on which track is the right one to take. See also Narita (2009d) for some relevant discussion.

5. Concluding Remarks

The current proposal essentially draws a picture that the EF is a key innovation in the evolution of human language. It is this feature which defines the 'computational atoms' for syntax, namely LIs. It is also the EF which allows LIs to be subject to Merge. Further, it can be used as a basis for Search/Agree, which the performance systems can make use of in various ways: C–I utilizes Search$_{EF}$ as the instruction for θ-marking (the Theta Principle (31)), and SM utilizes Agree$_{EF}$ as a necessary component for defining unambiguous labels for each node, feeding phonological linearization. To the extent that this picture has some truth to it, the study of EFs will constitute a major source of insight for future research in the field of comparative ethology (Hauser *et al.* 2002, Fitch *et al.* 2005), addressing the question of what in natural language is distinctively human.

Let us return to the two questions raised at the beginning of this article, (1) and (4). (1) was essentially a methodological, Ockham's razor question:

(1)   Does the theory of human language really need to assume labels/labeling to set an empirically adequate account of the known variety of linguistic phenomena?

We started the whole discussion of the Agree$_{EF}$-based label theory by assuming a moderate *Yes* to (1). In particular, the lack of any empirically successful label-free linearization mechanism in the past led us to make an assumption that phono-

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[35]   This point holds for whatever the correct linearization mechanism might ultimately turn out to be. Note that it is not unreasonable to suppose that all the linearization mechanisms proposed in the past literature (LCA, Kayne 1994), Symmetry Principle (Fukui & Takano 1998), X-bar schema with head-parameter, etc.) might just be one of the several options that human phonological systems can come up with.

logical linearization operations requires asymmetry coded by labels. This investigation reached the definition of labels in (15).

(15) *The Definition of Labels*

For any SO Σ, an LI H is the label of Σ $=_{def}$ H Agrees$_{EF}$ with the rest of the LIs within Σ.

The notion of 'label' here is reduced to just a well-defined shorthand for an LI prominently Agreeing$_{EF}$ with the rest of the LIs in a given SO, nothing more. Since now Agree$_{EF}$-relations generated in syntax can fully derive the effects of labeling, whether the syntax-internal computations really have to refer to labels (instead of Agree$_{EF}$-relations) becomes questionable. It was even hinted that the phonological linearization at the SM-interface might be the sole mechanism in faculty of language ("in the broad sense"; Hauser *et al.* 2002, Fitch *et al.* 2005) that has to refer to labels.[36]

If we can reduce the empirical burden of the notion 'label' along this line of approach, then it constitutes an indirect support for my moderate *No* to the substantial minimalism question in (4).

(4) Does 'label' count as a virtually conceptually necessary part of human language (an optimal C–I-SM linker, insofar as the SMT holds)?

My *No* was, however, only moderate, since I entertained the possibility that the driving force of labeling, namely Search$_{EF}$, is optimally feeding interpretation (θ-marking) at C–I (the Theta Principle).

(31) *Theta Principle*

α Searches$_{EF}$ β in syntax. $\Leftrightarrow$ α θ-marks β at C–I.

I construed the Search$_{EF}$-θ-marking transparency as one instantiation of a more far-reaching principle of (Derivational) Full Interpretation.

(54) *Derivational Full Interpretation* (DFI)

Every syntactic operation correlates with a corresponding interpretation at C–I (or at SM).

To the extent that (54) holds, there is a strong sense in which syntax itself is just a generator of 'language of thought', freely computing symbolic thoughts. Optimality of syntax for C–I would become almost trivial, insofar as syntactic operations *are* C–I-interpretations, and C–I is proposed to be much more interpretive, blindly following the path syntax has carved out (Uriagereka 1999: 275,

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36 Contra Pinker & Jackendoff's (2005: 212) claim that "major characteristics of phonology are specific to language (or to language and music), [and] uniquely human," Samuels (2009a, 2009b) claims that the formal properties of phonology in human language are entirely explainable in terms of the third factor in language design, principles and properties that are not specific to human language.

2002: 64, Hinzen 2006: 250, Chomsky 2007a: 15; see also Narita 2009d for further discussion on 'naturalization of meaning'). It is only when syntax is used to generate 'pronounceable' objects that the SM externalizability conditions such as linearizability would matter, in which case the calculation of asymmetric labels by means of Agree$_{EF}$-relations might be one of the best available (hence close to optimal) "computational tricks" that the syntax-SM mapping can come up with. Thus, although the notion 'label' itself might not count as a virtually conceptually necessary part of syntax, its availability at the close-to-optimal SM externalization mechanism might be not so mysterious, either. Or, it may eventually turn out to be the case that some 'third factor' principles (Chomsky 2005) actually strongly constrain the optimal C–I-SM linking system to utilize 'label', a readily available *definiendum* of Agree$_{EF}$, for externalization purposes, in which case we may be entitled to withdraw our previous *No* to the question (4).

Much work has to be done, and I hope this article will constitute a modest step toward the minimalist goal of understanding how syntax could be shown to satisfy the SMT.

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The Evolution of I-Language: Lexicalization as the Key Evolutionary Novelty

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Comparative psychological research suggests that human and non-human minds comprise an array of encapsulated cognitive systems ('core know-ledge systems'). While most of these cognitive building blocks appear to be shared across species, the cognitive gap between human and non-human minds is nevertheless quite stunning (*Hauser's Paradox*). Following recent ideas concerning the crucial role of human word learning in cognitive development, it is here suggested that lexicalization — the association of concepts with words — is the key evolutionary novelty that allows lingu-istic minds to integrate the various encapsulated conceptual resources into a common mental language.

1.   Introduction: The Object of Inquiry

In this contribution, I want to comment on the present stage of evolutionary linguistics and suggest some lines of research that seem plausible to me. I will neither review all the relevant literature to support my claims nor attempt to provide detailed justification. The tentative and speculative nature of the remarks that follow should be obvious.

When considering questions of 'language evolution', we should ask, first of all, whether the questions are put in the right way. What does it mean to ask how language evolved? Is *language* a well-defined object of inquiry, anyway?

A by-now traditional answer to the latter question is that it is not; 'Language' in the ordinary-language sense of the term is not the object of inquiry of theoretical linguistics. Concerning scientific realism about such common-sensical notions, Noam Chomsky (2000: 20ff.) comments:

> The question is […] whether in studying the natural world […] we view it from the standpoint provided by such [common-sensical] concepts. Surely not. […] [I]n the context of the search for laws of nature, objects are not conceived from the peculiar perspectives provided by concepts of common-sense. […] The concepts of natural language, and common-sense generally, are not even candidates for naturalistic theories.

The argument applies with equal force to questions of 'language' and 'language evolution': If linguistics is a science, it must necessarily abandon a common-sensical understanding of its object of inquiry in favor of a scientific category. The investigation of *language* (the concept) and its conceptually necessary properties has no place in linguistics; it is part of lexicography or ethnoscience, if anything (Chomsky 1980: 29).

This basic tenet of Generative Grammar (or rather, of science in general) is, however, often ignored in professional discussions. The present topic is a dramatic illustration: It seems to me that a good deal of the literature on language evolution is in fact based on unnecessary confusion resulting from the term *language*. The result is aptly summarized by Derek Bickerton (2007: 510) when he concludes that "there is perhaps no other field of human inquiry which has been so vitiated by a failure to get priorities straight."

A necessary first step towards a resolution of the situation is the realization that there can be (as a matter of principle) no unitary answer to the question "How did language evolve?" The question is scientifically irrelevant, since it makes no reference to any scientific category. The alternative is, again, a traditional one, in linguistics and elsewhere — namely, "to try to isolate coherent systems that are amenable to naturalistic inquiry and that interact to yield some aspects of the full complexity" (Chomsky 2000: 29). Once this step is done, questions of evolution can be raised in a meaningful way.

The standard approach takes the object of inquiry to be *Universal Grammar*, a distinct component of human biological endowment. In the course of acquisition, Universal Grammar grows into an *I-language*, the system that yields the specifically linguistic knowledge of the competent speaker (Chomsky 1986). A theory of I-language explicitly characterizes this body of knowledge; it has no procedural implications and abstracts away from extraneous factors that enter into linguistic performance.[1]

Minimally, the I-language must comprise a *generative procedure* (syntax) that operates over a finite *lexicon* of atomic units or words (in the technical sense) and maps the resulting complex objects onto representations that are accessed by performance systems. Since syntactic operations apply recursively to atomic units and combinations thereof, the I-language yields an infinite array of structural descriptions linking 'sound and meaning', that is, representations encoding phonetic, semantic and structural properties (Chomsky 1965).

Selective impairments and developmental dissociations (cf. the Genie case) suggest that the I-language is distinct from the systems that enter into linguistic behavior (cf. Jenkins 2000); comparative evidence supports this distinction on

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[1]   By hypothesis, I-language is distinct from animal communication systems (bee dance and the like), which may well be associated with a common-sensical notion of 'language'.

phylogenetic grounds (more below). The complex of I-language and various performative systems enters into human action (linguistic thought and external-ization).

With this sharpened understanding of what the object of inquiry is, let us now turn to questions of evolution, that is, the emergence of I-language in the human species.

2.   I-Language and Related Systems — Evolution

2.1.   *Some Reasonable Assumptions*

I think it fair to say that virtually nothing is known, in a strict sense of the term, about the evolution of I-language, understood as a distinctly human capacity, in the sense outlined above. In what follows, I will merely mention some of the suggestive observations that are frequently referred to in this connection.

Let us first consider some developments in the history of the species that can be taken to be plausible indications for the evolutionary emergence of I-language. Various lines of research suggest that humans arrived in Europe around 40–50,000 years ago (the transition from Middle to Upper Paleolithic), in a period that saw an explosive emergence of complex tools and art, burials and complex social organization, and symbolic behavior.[2]

As is often pointed out, it is hard to imagine how this extremely rapid development could have taken place in the absence of linguistic communication and complex symbolic thought. It therefore seems reasonable to assume that the human I-language trait is *at most* 100,000 years old (Hurford 2004: 552), its emergence having facilitated the 'Upper Paleolithic Revolution' (see also Berwick & Chomsky, to appear).

At the same time, however, it is known that brain capacity and vocal-tract anatomy of modern humans were in place much earlier (Holden 1998: 1455), predating, as it appears, the crucial cognitive innovations. Thus, by many estimates at least, there is an evolutionary mismatch between the phylogenetic development of anatomical prerequisites and the actual emergence of behavior indicating higher-order, presumably linguistic, cognition. In the words of evolutionary anthropologist Ian Tattersall (1998: 171):

> It is very hard to avoid the conclusion that articulate language is quite intimately tied up with all […] aspects of modern human behavior. Yet we know that effectively modern skull-base anatomy appeared long before we have any convincing archaeological evidence for complex sym-bolic behavior. […] Simultaneous acquisition of both the central and the peripheral apparatuses necessary for language would have been quite a developmental trick to pull off, and a multistage process is certainly easier to envisage in both developmental and evolutionary

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[2]   Juan Uriagereka (p.c.) points out that recent archaeological research casts some doubt on this traditional estimate; the actual cultural revolution may in fact have taken place tens of thousands of years earlier. Whatever the exact time frame, the crucial point is that the emergence of 'higher culture' took place within the blink of an eye, in evolutionary terms.

terms.

The idea of a 'multistage process' in both evolution and development has received ample support from comparative psychological research, to which I turn now.[3]

2.2.   The Comparative Approach and Hauser's Paradox

What happened to the human species that enabled it to make the cognitive leap that was the foundation for the Upper Paleolithic Revolution?

There are essentially two kinds of approaches to explain the striking discrepancies between human and non-human cognitive capacity (cf. Spelke 2003). According to a long-standing view in psychology, the stock of domain-specific cognitive modules or 'core-knowledge systems' in humans is quite different from that found in non-human species. In this view, human and non-human minds are made up of radically different building blocks, and what made the human mind special was, essentially, the evolutionary accumulation of more cognitive systems (see, e.g., the papers collected in Barkow *et al.* 1992).

In the last 15 years, however, an alternative view has grown out of psychological research investigating humans (infant and adult) and animals in a directly comparative setting. Motivated in part by the tension described by Tattersall, this comparative approach has led to the emergence of a novel picture of the evolutionary origins of human cognitive function, according to which many of the building blocks of the human mind are in fact shared with other species, but tied up in a way that yields a cognitive quantum leap.

This is not the place to review the wealth of experimental research that has shaped this newly emerging consensus; for comprehensive surveys of the relevant evidence the reader is referred to Hauser *et al.* (2002), Fitch *et al.* (2005), Fitch (2005), Carruthers (2006), Hurford (2007), and sources cited there. Here, I want to address the relevant question in the context of I-language evolution, which is: "Which systems must be assumed to be part of this evolutionary novelty, and which systems can be assumed to have developed independently?"

It is useful at this point to adopt the terminology of Hauser *et al.* (2002), where those aspects of the human language faculty that are distinctly human innovations are labeled 'FLN', while the language faculty, construed broadly as a complex of FLN and interfacing performance systems, is termed 'FLB'.

As outlined above, a fairly conventional assumption is that the I-language (the generative system) interfaces with systems of sensorimotor control (which in

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[3]   The multistage process is a plausible scenario for FLN, too. Chomsky in particular has emphasized the asymmetry between the two mapping components (SEM and PHON), mapping to C-I and SM systems, respectively. First, PHON introduces a variety of features that are not present in the lexical items of the expression (e.g., stress and intonation contours) and erases information required for semantic interpretation (copy reduction). Second, island phenomena show that externalization of 'thinkable' thoughts can fail (consider a standard CNPC violation such as *Who did Mary believe the claim that John killed t*, which corresponds to a coherent thought but cannot be externalized as such), again indicating that computation to SM is ancillary (Chomsky 2008, Berwick & Chomsky, to appear). On this view, it is plausible (in fact, likely) that the I-language evolved as a thought system, which only later got adapted to the sensorimotor system.

turn provide instructions to the articulators), and with perceptual systems that process linguistic input. Hauser *et al*. (2002) review a wealth of comparative evidence suggesting that many if not all crucial aspects of articulatory and perceptual prerequisites relevant to phonetics are rather widely shared with other species. This includes basic vocal-tract anatomy and well-developed motor control on the physiological side, and categorical and rhythmic perception, and presumably even vocal imitation (in dolphins, whales, and seals), on the cognitive side (see Hauser & Fitch 2003 for detailed review; also the contribution by Samuels 2009). Consequently, Hauser *et al*. (2002) ascribe these systems to FLB, concluding that they are not plausible candidates for being part of the evolutionary innovation that yielded human I-language.

A mounting body of evidence suggests that the same is true with regard to conceptual-intentional systems, that is, those systems that enter into semantic interpretation of the outputs of the I-language. Here, too, many components appear to be shared between humans and other species. Experimental work suggests that many species have some (perhaps rudimentary) theory of mind and sophisticated knowledge in areas like planning, navigation, social relations, and spatial reasoning, among others (see Hurford 2007 and Boeckx 2009: chap. 11 for extensive review).

At least a good deal of the relevant conceptual resources, then, cannot have evolved as part of FLN in the human species. Most of our core-knowledge systems are found in other species and hence predate modern humans; in Hurford's (2007: 87) words, "some (not all) of a human system of common-sense understanding precedes a system of language, both ontogenetically and phylo-genetically."

Hurford's claim is supported by developmental evidence as well. In experiment after experiment probing specific knowledge areas, pre-linguistic infants perform essentially like non-human animals, as Elizabeth Spelke and others have shown (see, e.g., Spelke 2003). The gist of this research is that pre-linguistic infants, like animals, exhibit sophisticated knowledge in specific domains, suggesting that the relevant cognitive systems predate the development of I-language. Nevertheless, like animals, infants fail at more complex tasks that require *conceptual connections across individual modules*.

Summarizing an extensive body of evidence, Spelke (2000: 1240) concludes that "although no young child or non-human animal possesses [the cognitive skills of adults], both exhibit many of the cognitive systems that serve as their building blocks." Concerning I-language, the natural conclusion is that "the early development of semantic categories [in pre-linguistic infants] parallels the development of phonological categories and suggests that natural language semantics, like natural language phonology, evolved so as to capitalize on pre-existing representational capacities" (Hespos & Spelke 2004: 455).

Synthesizing both the developmental and evolutionary evidence alluded to above, Hauser *et al*. (2002) conclude that conceptual systems are largely to be ascribed to FLB, part of the "peripheral apparatus" in Tattersall's terms.[4] In effect,

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[4]   Even on the neuro-functional level, certain areas (such as those accessed during the retrieval of word meanings) appear to be shared with non-human primates (Hurford 2007: 7, 57).

they equate I-language and FLN, speculating that the I-language (syntax and the lexicon) may indeed be the sole evolutionary novelty that allowed humans to cognitively outplay even their closest evolutionary relatives.

As the literature cited above demonstrates, the modular minds of non-linguistic creatures can be taken to comprise a variety of mental languages (or core-knowledge modules) that must allow for rudimentary predicate-argument structures (GIVE[X,Y,Z], SUPERIOR-TO[X,Y], etc.); for extensive arguments to this end, see Carruthers (2006), Hurford (2007). That is, non-human animals must have concepts and means of combining them into non-linguistic thoughts, within the limit of each core-knowledge system the animal possesses; these isolated cognitive systems can yield a high degree of task-specific sophistication, as described in the experimental literature (see the references above, and in particular Spelke 2003). The comparative approach strongly suggests that "Meanings existed in our pre-linguistic ancestors before the application of linguistic labels to them by humans" (Hurford 2007: 57) in just this rudimentary way (basic predicate–argument association). It is indeed hard to see how the relevant representations could be constructed without this fundamental mode of combination.

Overall, the comparative evidence suggests that we share a good deal of our non-linguistic cognitive capacities with other species (Fitch *et al*. 2005: 191). This suggests, in turn, that those systems that interact with the I-language were basically in place when the latter evolved, the scenario assumed by Hauser *et al*. (2002). The cognitive gap between human and non-human minds cannot be the result of the emergence of distinctive conceptual-intentional (let alone articulatory) systems in humans. It appears, then, that we end up with a *paradox*, as stated by Marc Hauser (2008, quoted in Boeckx 2009):

> [A]nimals share many of the building blocks that comprise human thought, but paradoxically, there is a great cognitive gap between humans and animals.

The way out of this paradoxical state of affairs that I will suggest below is based on a conjecture of Paul Bloom's (2000: 242): "Non-humans have no words and a relatively limited mental life; humans have many words and a much richer mental life. This might be no accident." I will elaborate on this idea in the remainder of the article.

3.   Lexicalization as the Key Evolutionary Novelty

3.1.   *Words and Calls*

The comparative approach suggests that while most cognitive building blocks predate the modern human mind, some rapidly-evolved novelty must have led to a dramatic change in overall cognitive capacity. This invites the hypothesis that it is indeed the human I-language that accounts (in large part at least) for the cognitive gap between linguistic and non-linguistic creatures (the evolution of FLN, as suggested by Hauser *et al*. 2002). But how could the emergence of I-language yield this dramatic change? To approach this question, let us consider a

further domain of human uniqueness, namely the lexicon (a component of I-language). In general, animals calls are functionally referential, signaling food or danger to listeners. Alarm calls are unlearned and extremely limited in their application. Calls are not used for intentional acts of reference and usually stimulus-bound (see Hauser 1996 for extensive discussion).

None of these properties are true of human words. This is somewhat unexpected from a point of view that takes animal calls and human words to be stages of the same evolutionary continuum. But in fact, the human lexicon is *not* simply a memory system like those found in other species (its complexities go well beyond a memorized list of items, see below), and there is no evidence suggesting that animal calls are precursors to human words in any way (Fitch 2005: 205), or that human words have any real analogs or homologs in animal communication systems (Hauser *et al*. 2005: 1576). The two systems — calls and words — are radically different, suggesting that words are part of the evolutionary novelty that brought about the I-language.

Indeed, lexicalization — the process that associates concepts with words — is a rather stunning cognitive feat. Consider, for instance, the fact that the number of words that children acquire during the critical period is extremely large compared to anything non-human animals can achieve (Hauser & Fitch 2003, Bloom 2000). There is no evidence for comparable acquisition mechanisms in non-human animals. The rate of vocabulary acquisition clearly suggests that the concepts to which words are linked are already in place (Chomsky 1980: 139), at least to a substantial extent, suggesting again that conceptual resources precede I-language in development. The intricacy of semantic properties of lexical items is enormous (Pustejovsky 1995, Chomsky 2000), and there is no evidence for comparative complexities in animal calls. The same is true with regard to structure: at most, animal calls have linear-sequential structure, but no higher-order hierarchical structure as evidences in human syntax. Non-human primates have serial processing abilities, but seem to lack the capacity of perceiving phrase-structural representations and long-distance relationships between elements (Fitch & Hauser 2004).

Finally, there is no evidence for a complex compositional/propositional semantics in non-human communication systems (Hauser 1996) that would allow calls to be combined in a way that yields a meaningful syntactic object. By contrast, words are the building blocks of syntactic objects, which map onto conceptual representations. The absence of words in animals (and pre-linguistic infants) might be crucial for the limitations their cognitive capacity exhibits.

As reviewed in the previous section, although many animals have rich conceptual resources, they "cannot make the specifically *linguistic connections between concepts* that humans can make" (Hurford 2007: 85, emphasis mine — DO). According to recent ideas, it is lexicalization — the human capacity to turn domain-specific concepts into units of linguistic computation — that allows human minds to make these connections. To sharpen this hypothesis, let us consider the capacity of lexicalization in some more detail.

3.2.   *Lexicalization of Concepts in Humans*

Lexicalization is the process that associates concepts with grammatical units, often in ambiguous ways. During acquisition, humans lexicalize a huge number of concepts within a very short period of time. Lexicalization appears to be largely automatic and independent of experience (cf. the acquisition of color and perception words in blind children discussed by Gleitman & Newport 1995).

A rather uncontroversial assumption is that concepts can be of various adicities; presumably, RUN is monadic, while GIVE is most likely polyadic. THE MAN does not have any adicity, since it is a referential expression. Adicities of predicates allow the formation of complex thoughts, via some kind of saturation: Concepts are 'interlocking' mental objects, and the formation of any meaningful thought requires at least the computation of minimal predicate-argument structure.

In a Fregean *Begriffsschrift*, each combination of two terms is an instance of function-application. This Fregean mode of combination is the minimal symbolic operation necessary for any mental language within a certain module, hence must be present in all animals. Without this kind of 'Fregean thought', it would indeed be inconceivable how animals can perform the intricate cognitive operations that underlie many kinds of behaviors, as argued at length by Hurford (2007) and others.[5]

According to the 'standard theory' in formal semantics (e.g. Heim & Kratzer 1998), the human I-language combines expressions in essentially this Fregean way. That is, combinations of predicates and arguments are interpreted as function-application/type-reduction, plus some theoretical tweaks to cover more complicated cases.

But some have argued that the human I-language does *not* work in this way, i.e. that it does not employ Fregean modes of combination to form complex expressions (Marantz 1997, Borer 2005, Pietroski 2005, forthcoming). Pietroski in particular has argued at length that on a plausible reformulation of semantics, all combination of lexical items signifies conjunction of monadic predicates. On this view, words are essentially adicity-free, while indicating (being linked to) the potentially polyadic concepts they lexicalize (see Pietroski 2005, forthcoming).

The starting point for such non-Fregean approaches to semantics is that once lexicalized, concepts (now words) appear to be extremely *promiscuous* (Pietroski's term). Words can be combined with other words to form expressions that are further combinable. I-languages do not impose any limits in this regard: All expressions are conjoinable, no matter how complex.

One observation that Pietroski capitalizes on is that the promiscuity of words makes it very hard to determine the adicity of a lexicalized concept. That is, words conceal the adicities of the concepts they indicate to a large extent. Consider some examples, borrowed from Pietroski (in press).

Is the concept GIVE triadic? That seems plausible: *X gave Y to Z*. But if the lexicalized counterpart of GIVE inherits this adicity, why are (1c–e) fine

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5   A reviewer rightly points out that inference from behavior to cognitive structures is not innocent, at least in the absence of a developed theory of conceptual/representational capacities. The evidence therefore has a 'best best' character; the conclusions drawn here are hard to avoid, however.

expressions of English?[6]
(1) a.   John gave Peter the money.
     b.   John gave the money to Peter.
     c.   Barry gave the money away.
     d.   Bill donated the painting and everybody gave something.
     e.   Robin Hood steals from the rich and gives to the poor.

Are COOK or SING triadic? This seems unlikely. But once lexicalized, it seems like they can be:

(2)   John cooked Bill an egg, while he sang the baby a lullaby.

What is the adicity of BUY? It seems rather implausible to assume that the concept BUY is, for example, triadic. But once lexicalized, it is free to occur in a ditransitive construction, as in (3c):

(3) a.   Plum bought the knife.
     b.   Plum bought the knife for Scarlet.
     c.   Plum bought Scarlet the knife.
     d.   Plum bought Scarlet the knife for ten dollars.

Further examples of this kind are all-too easy to find, and there is no need to go on here. What is relevant is that these examples are illustrations of the general case: Words do not have inherent adicities, but merely *indicate* (are associated with) concepts of certain adicities.

So, do words really 'take arguments', like concepts do? This is a traditional view in linguistics, but it is in fact all but obvious that argument structure (as a syntactically expressed property of predicates) really exists. There just seems to be no one-to-one relation between the grammatical behavior of a word and the adicity of the concept(s) it indicates, as shown above. But adicity-matching hypotheses assume just this one-to-one relation.

Pietroski's alternative is to deny that words have any adicities at all, and to take all words to be of the same semantic type. This requires demonstration that a single type is compatible with efficient semantic composition — a project that is pursued in Pietroski (2005, forthcoming), the details of which need not concern us here. Adicity effects in I-language expressions ('canonical' number of arguments and the like) must then be reflections of the associated concepts, that is, result from the interaction of the grammar with outside systems. In general, combination of words is so flexible, and the lexicalization process in infants so rapid, that words are apparently freed from any conceptually imposed adicities.

On this view, then, lexicalization is a distinctly human capacity (hence part

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[6]   A reviewer suggests that constructions like (1d) and (1e) might be elliptical, but it is hard to
      see what the elided object would be in these cases. A transparent example (from Chomsky
      1986: 8) is *John ate*, which cannot mean *John ate something* (*something* elided), since *John ate a*
      shoe does not entail that *John ate*. It seems like the meaning of the predicate does indeed vary
      in subtle ways depending on the grammatical context, in this case as well as in those cited in
      the text.

of FLN; see below) that yields the unboundedness of (linguistic) thought without requiring significant changes in mental architecture. In the following section, I will elaborate on this more specific hypothesis.

3.3.   *The Universal-Currency Hypothesis*

Non-human animals can no doubt compute elementary conceptual predicate-argument structures of the 'who did what to whom'-type; see Hurford (2007). As shown by the results in comparative psychology, we do find complex representations in many species, suggesting that (to put it informally), while certain conceptual capacities are present in the absence of I-language, there is comparatively little that nonhuman minds can do with them (Chomsky 2004: 47).

In particular, it seems like combinations of concepts cannot cross the bounds imposed by the various knowledge modules (such as social relations or spatial reasoning); that is, nonhuman conceptual structures are domain-specific in a rather strict way. As mentioned before, in order to generate thoughts, any conceptual system must be at least 'Fregean' in its combinatorial capacities (predicate-argument structure). But apparently, animals have no complex thought beyond that — the conclusion drawn by Hauser & Spelke (2004), Carruthers (2002, 2006), and many other researchers. Presumably, then, a Fregean mode of composition within the bounds of any particular core-knowledge system is all that is available to non-linguistic creatures:[7]

> [T]he cognitive functioning of [human infants, non-human primates, and human adults] can be understood, in part, in terms of the same systems of core knowledge. These systems serve to construct abstract representations of basic features of the world, including objects and numerosities, but they are limited in three respects: They are domain specific, task specific, and largely independent of one another.
>
> (Spelke 2000: 1240)

By contrast, (adult) human minds can integrate concepts from various sources by lexicalizing them, yielding unbounded cross-modular thought. Thus, higher apes may have complex Fregean conceptual structures in various mental domains while lacking the computational capacities provided by the I-language ('free thought'):

> When human adults form and use concepts that no other animal can attain, they do so by assembling a set of building blocks that are shared with other animals. These building blocks are part of core knowledge. Language may be a powerful device for assembling and coordinating the systems of core knowledge.   (Hauser & Spelke 2004: 862)

When lexicalized, human concepts can freely and systematically compose, *regardless of the conceptual subsystem from which they are drawn*. I-language expressions can combine concepts of color, sound, space, time, self, other things, action, habitation, number, etc. as well as theoretical and fictitious concepts

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[7]   See already Chomsky (1980: 57) for similar speculations.

(Spelke 2000, Carruthers 20002, Pietroski, in press).[8] This fact about our linguistic ability is so natural to us that it is hard to appreciate its relevance; but in fact, there is no reason to take this ability of constructing *cross-modular concepts* for granted.

Consider science. Scientific concepts are very different from common-sense concepts, and given that only human minds are capable of naturalistic inquiry, we may well be led to posit a distinct conceptual system for science (the *Science-forming Faculty* of Chomsky 2000: 22). This particular cognitive system allows for the construction of concepts that are assigned determinate meanings, taken to refer to natural kinds, but open to modification as science progresses. Thus, other than common-sensical categories, these terms refer (by stipulation) and do not simply grow in the mind. As argued by Chomsky (2000), the resulting concepts abstract from all the semantic complexities of common-sense concepts, and their meaning is simply defined. *Hydrogen atom*, H_2O, *CP* are products of this particular conceptual capacity, and as such radically different from *language*, *person*, *table*, and other concepts drawn from the conceptual domain we call 'common sense' (Chomsky 2000: 23f.). Thus, both kinds of concepts belong to distinct mental conceptual resources.

However, "[t]he constructed systems [based on concepts of the science faculty] may use resources of the I-language (pronunciation, morphology, sentence structure, etc.)" (Chomsky 2000: 42f.). That is, "language makes science possible" (Hurford 2004: 552). This is because from the point of view of the grammatical system, radically different types of concepts are 'just words', once lexicalized.

Put in a different way, I-languages allow the generation of domain-general thoughts by extracting concepts from their modular bounds, by means of lexicalization. All comparative research suggests that animals and pre-linguistic infants are incapable of representing such multimodal thoughts (see especially Hauser & Spelke 2004, Spelke 2000, Carruthers 2002, 2006, and references cited). In the words of Spelke (2000: 1241):

> By combining representations from these systems, human children [...] and adults may gain new abilities not by creating those abili-ties out of whole cloth, but by bringing together building-block representational systems that have existed in us since infancy.

If this is correct, then the distinctly human capacity of domain-general thought is a direct result of the distinctly human trait of I-language, comprising a lexicalization mechanism and syntax. I submit that this lexicalization mechanism of the I-language is indeed the most plausible candidate for the key evolutionary novelty that brought about the unity of human thought. It is lexicalization that allows a concept to be enter into the construction of syntactic structure, which in turn acts as an instruction to construct a complex concept/'thought' (Boeckx, to appear, Pietroski, in press, forthcoming).

A simple mechanism that associates words and concepts could thus

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8   The resulting expressions may of course be awkward (*The unicorn swallowed the electron*), but that is immaterial to the point at hand.

account for both the unifying function of the I-language and its core computational property, discrete infinity:

> [A lexical item (LI)] has a feature that permits it to be merged. Call this the *edge feature* (EF) of the LI. … The fact that Merge iterates without limit is a property at least of LIs — *and optimally, only of LIs*, as I will assume. EF articulates the fact that Merge is unbounded, that language is a recursive infinite system of a particular kind.
>
> (Chomsky 2008: 139, emphasis mine — DO)

Evidently, if this were true, an evolutionary account of I-language would be significantly simplified, in that syntax itself would follow from lexicalization (assignment of an edge feature).

The proposal, then, is that the lexicalization of a concept effectively demodularizes it. Given this central role of lexicalization, it follows that humans (*qua* lexicalizers) can entertain an unbounded variety of thoughts, many of which are necessarily unavailable to non-linguistic minds. The idea is aptly phrased by Cedric Boeckx (to appear) in recent work:

> We can in fact think of lexicalization as the mental analog of the hypothetical creation of a truly universal currency, allowing transactions to cross formerly impenetrable boundaries.

In this way, humans can go beyond the limited combinatorial possibilities offered by the various encapsulated *Begriffsschriften* in all animal minds. Notice how neatly this proposal dovetails the emerging consensus in comparative psychology:

> [C]ore systems serve as building blocks for the development of new cognitive skills. When children or adults develop new abilities to use tools, to perform symbolic arithmetic calculations, to read, to navigate by maps and landmarks, or to reason about other people's mental states, they do so in large part by assembling in new ways the representations delivered by their core systems.   (Spelke 2000: 1233)

This basic picture (see also Carruthers 2002) explains immediately why animals have striking capabilities in various core-knowledge domains, but why only humans appear to be able to unify all these domains via I-language (i.e., it resolves *Hauser's Paradox*). Bloom's conjecture is vindicated: *Words* are crucial.

Lexicalization is thus likely to have been the key innovation that yielded free, cross-modular thought, accounting for the difference between human and non-human mental life despite largely shared conceptual building blocks. A maximally radical version of this hypothesis holds that little more than the evolution of the edge feature (in Chomsky's sense) was necessary for this cognitive quantum leap.

3.   Conclusion: Lexicalization as the Key Evolutionary Novelty

There is ample empirical evidence for the claim that basic semantic relations as

part of shared conceptual systems predate (and provide a basis for) the emergence of human syntax (Bickerton 2003, Hurford 2007). Plausibly, animals have considerable conceptual capacities but are unable to integrate the various mental languages in the same way humans can. The comparative approach thus leads to *Hauser's Paradox*.

Lexicalization — the human capacity to associate concepts with words — appears to be more than merely a trivial attaching of phonetic labels to concepts. Lexicalized concepts become conjoinable beyond their modular bounds, yielding a recursive system that transcends the boundaries of core-knowledge domains. This suggests that the crucial evolutionary novelty was in fact the mechanism of lexicalization, leading to an increase in both computational and conceptual capacities. If these speculations are on the right track, the significant cognitive gap between humans and non-linguistic animals is not the result of a profound remodeling of the pre-linguistic mind. Rather, the sudden addition of recursive syntax, paired with a capacity for lexicalization, plausibly led to the explosive emergence of symbolic thought that paved the way for modern human behavior.

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Danish Vestigial Case and the Acquisition of Vocabulary in Distributed Morphology

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As Halle & Marantz (2008: 71) acknowledge, "we have no real idea about how a child assigns features to Vocabulary Items" in Distributed Morphology (DM). Stated generally, how do children acquire language-specific (sometimes variable) mappings between morpho-syntactic features and their morpho-phonological exponents? Following Emonds (1986) in a DM framework, this article advances a testable 'morphological transparency' constraint on the acquisition of Vocabulary, and presents supporting results from a pilot observational child-language study in Danish. This constraint explains a significant difference in the mechanisms of Germanic case morphology. By hypothesis, 'vestigial' case forms of English and Danish pronouns are contextual allomorphs, with Vocabulary that do not contain any morpho-syntactic case features. Vestigial-case mechanisms constitute a comprehensive analysis of intra-individually variable case-form mismatches in coordinate Determiner Phrases, predicate nominals, and other syntactic structures. Thus, a principle of language acquisition ultimately explains the distribution of case forms both within and across language varieties.

Keywords: acquisition; case; Danish; Distributed Morphology; Germanic

1.   Introduction

As is well known, a high degree of inter-individual (i.e. cross-linguistic) variation can be observed in the morphosyntax of case (e.g., Blake 1994, Malchukov & Spencer 2009). There is furthermore significant intra-individual (i.e. Labovian sociolinguistic) variation observed in the morphosyntax of case, which is a primary focus of this article. Considering such variation, it seems reasonable to suspect that the underlying mechanisms of case morphosyntax are largely, if not wholly, acquired on the basis of environmental input rather than determined innately by UG. This suspicion deepens upon adopting a general Minimalist

perspective (Chomsky 1995, 2000 *et seq.*, see also Hauser *et al.* 2002), where only the operations of the narrow syntax are genetically endowed; all variation is restricted to 'lexical' features and the interfaces between narrow syntax and language-external cognitive and sensory/motor systems.

The question of case acquisition arises in an especially acute form within the theory of Distributed Morphology (DM; Halle & Marantz 1993, Embick & Noyer 2007). In the architecture of DM and related separationist theories (e.g., Beard 1995), post-syntactic objects and operations determine the morpho-phonological forms taken by sets of morphosyntactic features. Insofar as such morphological mechanisms are explicitly articulated, as they are in DM, it becomes possible, and in fact necessary, to formulate and test hypotheses about how they are learned by children during the process of linguistic development. This imperative strengthens if we adopt any version of more radical proposals, whereby case morphology is divorced from abstract licensing of nominal phrases in the syntax, and the features or properties realized as case are determined or valued solely in a post-syntactic morphological interface component (e.g., Marantz 2000, McFadden 2004, 2007, Sigurðsson 2006, 2009; but cf. Legate 2008 for arguments against such approaches). If such ideas are at all on the right track, then case morphosyntax must be learned. Of course, it is no small matter to discover what the relevant mechanisms are and how exactly they are acquired. Moreover, any moves toward analyzing case as a strictly morphological phenomenon raise the theoretical stakes considerably, given the central role of case in Government and Binding and Minimalist theories of syntax (see Lasnik 2008 for an overview and references).

Accordingly, we might take it as a desideratum for morphosyntactic theory that, in the words of Halle & Marantz (2008: 71), "principles of language acquisition ultimately should explain facts about the distribution of forms across the paradigms generated by the inflectional features of a language." Unfortunately, however, there has been no work on language acquisition specific to the DM framework. "In particular," as Halle & Marantz acknowledge, "we have no real idea about how a child assigns features to Vocabulary Items." In DM Vocabulary are listed 'lexical' entries that provide phonological exponents to abstract morpho-syntactic terminals. Vocabulary insertion of phonological features takes place during the post-syntactic computation to the Phonetic/Perceptual Form (PF) interface. The question is not limited to the DM theoretical framework, but can be stated generally: How exactly do children acquire an inventory of language-specific (and sometimes variable) mappings between morphosyntactic feature bundles and their morphophonological exponents?

This article takes tentative steps toward addressing the kinds of issues raised above. Following Emonds (1986) within a DM framework, I advance a specific and testable 'morphological transparency' constraint on the acquisition of Vocabulary items. Emonds (1986) gives an early, and in my view essentially correct, analysis of English pronominal case-form mismatches in coordinate Determiner Phrases (CoDPs) and other environments. (For alternative analyses, see e.g., Sobin (1994, 1997), Lasnik & Sobin (2000), Johannessen (1998), Schütze (2001), Quinn (2005), and Grano (2006).) The transparency constraint proposed below is intended to explain a significant difference in the mechanisms of Ger-

manic case morphology. As is well known (e.g., König & van der Auwera 1994, Sigurðsson 2006), languages such as German, Icelandic, and Faroese have phonologically distinctive case morphology on elements of open-class nominal phrases, as well as on closed-class pronouns. However, languages such as English, Danish, and varieties of Norwegian and Swedish, among others, have phonologically distinctive case-form allomorphs only within a closed sub-set of personal pronouns. Because morphological acquisition is constrained by transparency, by hypothesis, such 'vestigial' case forms of (at least) English and Danish are contextual allomorphs, with Vocabulary that do not contain any abstract morpho-syntactic case features. This difference between the mechanisms of vestigial and transparent case morphology constitutes the most comprehensive analysis to date for a heretofore puzzling instance of intra-individual variation in English (and, as predicted, in Danish): namely, pronominal case-form mis-matches in CoDPs and other syntactic structures. As desired, then, a principle of language acquisition provides the ultimate explanation for the distribution of case forms both within and across languages.

2.   Emonds's (1986) Analysis of Case Variation in English

2.1.   Pronominal Case-Form Variation

English singular and plural 1st person and 3rd person pronouns have two case-form allomorphs.[1] For the most part, these case forms are in complementary distribution: One appears when the pronoun is the subject of a finite clause, and the other appears when the pronoun is a verbal (direct or indirect) or prepositional object, a subject of a non-finite clause, or in many other heterogeneous positions. These two case allomorphs are hereafter referred to as subject and oblique forms (SFs and OFs).[2]

(1)   *English pronominal case-form allomorphy*

	Subject form (SF)	Oblique form (OF)
1SG	*I*	*me*
3SG	*she (♀) / he (♂)*	*her (♀) / him (♂)*
1pl	*we*	*us*
3pl	*they*	*them*

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[1]   2nd person *you* and 3rd singular neuter *it* are excluded from consideration, since they do not have distinct case forms in English.

[2]   In order to simplify exposition, I do not consider English possessive pronouns in this article. One reason for their omission is that the possessive pronoun forms express a different semantics, and accordingly their distribution is orthogonal and not complementary to the distribution of the case allomorphs. Furthermore, according to the theory developed below, the syntactio-semantic features responsible for possessive semantics — let us refer to them as [±POSSESSIVE] for short — are transparent on DPs in English. That is, [±POSS] is not only phonologically distinctive on closed-class pronouns, but on open-class DPs (e.g., [*the man with the hat*]'s *beer*). As we will see, this means that Vocabulary may contain [±POSS], and therefore that mismatches in CoDP are not predicted.

Emonds (1986) is among the first linguists to provide an explicit mechanistic analysis[3] of phenomena known to virtually every native speaker of English: There is sociolinguistically significant variation in the distribution of case forms when pronouns occur in several heterogeneous syntactic constructions. The following constructed examples are adapted from Emonds, with his terms followed by mine in brackets when different. Emonds refers to these as "deviant prestige constructions" because the prescribed SF seems rare in speech and apparently strikes most native speakers as being marginal or even unacceptable, despite its normatively favored status. Thus, note that by '*', Emonds means 'socially prestigious but grammatically deviant' and not necessarily 'unattested or unacceptable'. (Similar lists are provided by Schütze (2001) and Grano (2006).)

(2) a. *Conjoined Subjects [CoDPs]*
         Mary and him/*he are late.
     b. *Predicate nominals [post-copular nominals]*[4]
         It is just us/*we who John says are late.
     c. *Subjects of understood predicates [objects of comparatives]*
         Students smarter than her/*she get no scholarship.
     d. *1st person demonstratives of subjects*
         Us/*we commuters are often blamed for smog.
     e. *Appositives to subjects*
         Judy thinks the best student, namely her/*she, should win the prize.

There are additional environments where OFs occur categorically in English (with no prescriptive attention). Below and hereafter, '*' means 'unattested or unacceptable' as per convention.

(3) a. *Left-dislocated subjects*
         Me/*I, I truly love beer.
     b. *Isolated pronominal subjects*
         Who truly loves beer? Me/*I!

Most striking among these environments are CoDPs, where variably mismatched pronominal case forms occur with salient frequency.[5,6] The following

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3   Other early but independent analyses include Schwartz (1985) and Parker (1988); see also Jespersen (1933, 1949 [1961]) for perhaps the earliest observations of this phenomenon.
4   For cross-linguistic discussion of predicate nominals, see Schütze (2001), Sigurðsson (2006), or Thráinsson (2007).
5   I would like to emphasize that I intend 'mismatch' as neutral term to describe the appearance of an allomorph outside of its expected distribution, in the environment of its complementary form. Thus, consider the invariant complementary distribution of English case allomorphs in examples (4)–(7) without coordination:

      (i)   a. *Him is fighting.
             b. *I was coming between **they**.
             c. ***Him** was working.
             d. *This is starting to make **I** feel bad.

6   As mentioned in fn. 2 above, mismatched possessive pronouns in CoDPs are not predicted

attestations are from Parrott (2007: chap. 6);[7] CoDP constituents are indicated with brackets and mismatched pronouns with boldface font, a convention followed throughout this article.

(4)   *OF* in finite-clause subject CoDP

    a.   [**Him** and the zombie hunter] are fighting.
    b.   [The zombie hunter and **him**] are fighting.

(5)   *SF* in prepositional object CoDP

    a.   He thought I was coming between [**he** and his wife].
    b.   * He thought I was coming between [his wife and **he**].

(6)   *OF* and SF in finite-clause subject CoDP

    a.   [**Him** and I] were working at the time.
    b.   * [I and **him**] were working at the time.

(7)   *OF and **SF*** in verbal object CoDP

    a.   This is starting to make [him and **I**] both feel really bad.
    b.   * This is starting to make [**he** and me] both feel really bad.

Evidently, mismatched OFs are well attested in finite-clause subject CoDPs (4a); mismatched SFs are well attested in prepositional (5a) and verbal object CoDPs (7a); and CoDPs containing both a SF and an OF pronoun are well attested as both subjects and objects (6a)/(7a). A remarkable fact about this variation is that pronoun-specific linear ordering effects are observed with coordinated SF pronouns, as empirically confirmed by acceptability question-naires (Quinn 2005), corpus studies (Grano 2006), and observational 'specimen collection' (Parrott 2007: chap. 6). OFs are attested and judged acceptable in

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because [±POSS] is transparent on DP in English. Although the topic requires further study (see also Zwicky 2008), at first glance (and with help from Google), this prediction appears to be largely confirmed. Possessive morphology seems possible either on both conjuncts of a CoDP (i), or on the entire CoDP (ii), but (mostly) not otherwise (iii). Note also the ordering effect with case allomorphs is retained in a possessive CoDP (iid).

    (i)   a. Erik's and my brewery
            b. My and Erik's brewery

    (ii)   a. Erik and me's brewery   ["and me's" 34,600 Google results = 8%]
            b. Me and Erik's brewery
            c. Erik and I's brewery   ["and I's" 393,000 Google results = 91%]
            d. *I and Erik's brewery

    (iii)   a. * Erik and my brewery
             b. * My and Erik brewery
             c. [%]Erik and my's brewery   ["and my's" = 4,470 Google results = 1%]
             d. ?Erik's and my's brewery
             e. * Erik's and me brewery
             f. * Erik's and I brewery

[7]   The b-examples are constructed, in order to illustrate ordering effects.

either conjunct (4a-b). 1SG SFs are only attested and acceptable in the second conjunct (6a–b)/(7a); however, 3SG SFs are attested and acceptable only in the first conjunct (5a–b).[8] A CoDP's syntactic structural context is apparently irrelevant to these ordering effects (see (9)–(10) and discussion immediately below). Moreover, there appears to be an implicational hierarchy such that 3SG SFs do not co-occur with 1SG OFs in CoDP (7b).

Every native speaker of English is aware of the often rather extreme normative attitudes toward case-form usage in CoDPs (for surveys of prescriptive literature see Angermeyer & Singler 2003, Grano 2006, for examples, see Honey 1995, O'Conner 1996, Garner 1998, Casagrande 2008). However, normative attitudes regarding case-forms in post-copular nominals are much milder than attitudes toward coordinated pronouns. There are two set expressions where SF pronouns are occasionally used, namely *It is I* or *This is he/she*. But otherwise, post-copular pronouns are always OFs, as illustrated in (8) below. Prescription of SFs in this environment appears to be a lost cause. According to O'Conner (1996: 10, 186), even "some of the stuffiest grammarians" accept that a speaker who uses the prescribed SF in this environment "sounds like a stuffed shirt," that is, pompous or pretentious. It seems clear that essentially categorical OFs should not be regarded as a mismatch in this environment, even though SFs are (or were) the prescribed pronoun, and as we will see below, post-copular nominals are invariantly nominative in languages like German. The a-sentences below are attested, but the b–c-sentences are constructed.

(8)   a.   It really is just him….
       b.   *It really is just he….

When CoDPs occur as post-copular nominals, pronoun-specific ordering and implication effects are evident, just as for coordinated pronouns in any other syntactic environment. OFs can appear in either conjunct, as in ((9), cf. (4) above).

(9)   a.   My time with C. and F. is strictly [me and them].
       b.   My time with C. and F. is strictly [them and me].

1SG SFs appear only in the second conjunct of a post-copular CoDP ((10a–b), cf. (6) above). 3SG SFs are not coordinated with 1SG OFs ((10a,c), cf. (7) above).

(10)   a.   We often dream of the days when it is just [him and I].
       b.   *We often dream of the days when it is just [I and him].
       c.   *We often dream of the days when it is just [he and me].

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8   Coordinated plural pronouns are extremely rare, probably for pragmatic reasons, so nothing will be concluded about them here. The few attestations in my collection all have OFs in the second conjunct (Parrott 2007, 2008).

       (i)   a.   [Her brothers and **them**] was standing over there.
              b.   [Bush and **them**] spend more money in one week in Iraq than it would take to fix up all our homes.

       For what it is worth, my intuition is that coordinated SFs sound extremely marginal in either conjunct, and would only be used in writing.

For reasons of space, and in view of the pilot-study results to follow below, subsequent discussion is limited to CoDPs and post-copular nominals. For more details about the other constructions, see the literature cited above and Parrott (2007: chap. 6).

2.2.   *Emonds (1986) a là DM*

The core of Emonds's (1986) analysis, updated into modern theoretical terminology, is that English pronominal allomorphy does not involve abstract case features at all. Instead, SF and OF pronouns are contextual allomorphs: They are exponents of a pronoun's structural context, but not exponents of a pronoun's case features. The morphology of vestigial-case pronouns is presented informally below.

(11)   a.   SF exponent when a pronoun is the subject of a finite clause
         b.   OF exponent when a pronoun is in any other structural context.

Emonds's analysis merely states that the morphology of English pronouns does not refer to case features. The analysis does not entail any position on whether abstract case features are checked/assigned in the narrow syntax (e.g., Chomsky 2000 *et seq.*, Adger 2003, Hornstein *et al.* 2005) or determined in a post-syntactic morphological component (Marantz 2000, McFadden 2004, Sigurðsson 2009). Although it is consistent with the standard view that all English DPs have unpronounced syntactic Case features, the present approach is also consistent with a more radical morphological analysis whereby English lacks abstract case features altogether. The matter cannot be settled here, but remains in the background. I return to the question briefly in the conclusion.

Implemented in a DM framework, the analysis holds that English pronominal Vocabulary do not contain any case features whatsoever. Vocabulary are listed lexical items that formally resemble generative phonological rules (e.g., Chomsky & Halle 1968). Each Vocabulary item contains a set of phonological features (inside phonemic slash brackets on the right side of the double arrow) that are post-syntactically inserted into a terminal node identified by an underspecified set of morphosyntactic features (inside square brackets on the left side of the double arrow). Vocabulary may also include information (following a slash on the right side of the double arrow) that specifies a structural or other context where the target terminal must appear in order to receive exponence. Because more than one item may be inserted in the same terminal, Vocabulary must 'compete' for insertion according to the Elsewhere condition (Kiparsky 1977, Halle & Marantz 1993, Halle 1997). Thus, the Vocabulary item with the most highly specified features is inserted first, less-specified items later, and the least specified last, by default.

The schematic Vocabulary for English pronouns in (12) state that the phonological features of a SF exponent are inserted into a terminal containing a categorical determiner feature (D) and person/number agreement features[9] (φ)

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9   Following Halle (1997) among others, the φ features adopted here are [±AUTHOR], [±PARTICI-

— that is, a pronoun — whenever the target D terminal is itself the specifier of finite Tense (T[±PAST]). The OF is an elsewhere exponent, inserted by default when the target D terminal occurs in any other structural context.[10]

(12)   [D, φ]   ⇔   /SF/   / [$_{TP}$ __ [T[±PAST] …]]
       [D, φ]   ⇔   /OF/   *elsewhere*

The Vocabulary for English 1SG and 3SG pronouns are given in (13)–(14):

(13)   [D, +AUTH, +PART, −PL]   ⇔   /ai/   / [$_{TP}$ __ [T[±PAST] …]]
       [D, +AUTH, +PART, −PL]   ⇔   /mi/   *elsewhere*

(14)   [D, −AUTH, −PART, −PL, ♂]   ⇔   /hi/   / [$_{TP}$ __ [T[±PAST] …]]
       [D, −AUTH, −PART, −PL, ♂]   ⇔   /hɪm/   *elsewhere*
       [D, −AUTH, −PART, −PL, ♀]   ⇔   /ʃi/   / [$_{TP}$ __ [T[±PAST] …]]
       [D, −AUTH, −PART, −PL, ♀]   ⇔   /həɹ/   *elsewhere*

This analysis explains why (variably mismatched) OF pronouns are attested in such diverse syntactic structures (examples (2)–(3) above), whose only common property is not being the specifier of finite T. Pronouns in any of these constructions cannot receive SF exponence, so only elsewhere OFs can be inserted.

2.3. *Case-Form Variation in CoDPs*

What about CoDPs? These are certainly the most crucial structures to explain. It is not at all obvious why coordination should be a default environment on a standard theory of abstract syntactic Case (cf. Schütze 2001). Why should coordination interfere with Case-feature checking/assignment (cf. Parker *et al.* 1988, Johannessen 1998)? Indeed, case mismatches inside CoDPs appear to be completely unattested and unacceptable in languages with 'rich' morphological case, as we will see immediately below for German. If there is in fact some special

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PANT], and [±PLURAL], where 1st person = [+AUTH, +PART], 2nd person = [−AUTH, +PART], and 3rd person = [−AUTH, −PART]. See also Nevins (2007b, 2007a) and Nevins & Parrott (in press) for more discussion of φ features.

[10] Again, possessive pronouns are excluded for simplicity's sake (see fnn. 2 and 6 above). As mentioned, the feature(s) [±POSS] is transparent by hypothesis, and therefore may be contained in Vocabulary. As a first approximation then, let us assume that a more complete set of Vocabulary for English [D, φ] includes something like the item schematized in (ia), with a 1SG example given in (ib):

    (i)   a.   [D, φ, +POSS]   ⇔ /possessive form/
            b.   [D, +AUTH, +PART, −PL, +POSS] ⇔ /mai/

Note that because the possessive Vocabulary sketched in (i) contain a [+POSS] feature, they will not compete for insertion with the case-form Vocabulary given in (12)–(13), which lack any [±POSS] feature (Halle 1997). Of course, I have not attempted to address the distribution of so-called 'weak/strong' (Quirk & Greenbaum 1973) possessive pronoun forms (i.e. *my/ mine, your/yours, her/hers, our/ours, their/theirs*).

property of coordination that causes interference, and if the syntactic mechanisms of Case are the same in both languages, then why should case-form mismatches be possible in English CoDPs but impossible in German CoDPs?

Following Munn (1994) and Johannessen (1998), the phrase structure of CoDP is now relatively uncontroversial (but see Goodall 1987 for an alternative analysis).

(15)   CoDP

DP   Co′

Co^0 DP
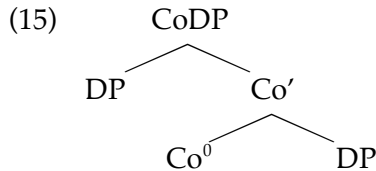
Notice that a pronoun inside of a CoDP is either the specifier or the complement of the coordinate head (Co^0). It follows that pronouns inside of a CoDP cannot themselves be the specifier of T[±PAST]: Only the CoDP itself can be the specifier of T[±PAST]. A CoDP subject of finite T is diagrammed below.

(16)   TP

CoDP   T′

DP *and* DP T[±PAST]   *v*P

…
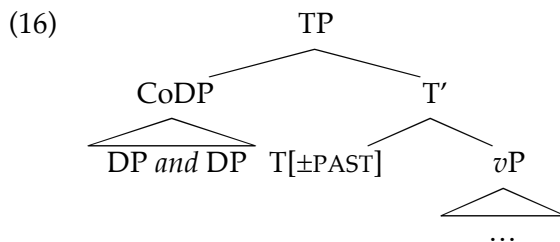
Therefore, on the present analysis of English case-forms as contextual allomorphs, pronouns in either conjunct of a CoDPs (examples (2a)/(4a)/(6a)/(7a)) receive elsewhere OF exponence for the same reason as post-copular pronouns (2b)/(5), pronoun objects of comparatives (2c), 1st person demonstrative pronouns (2d), appositive pronouns (2e), left-dislocated pronouns (3a–b), and isolated pronouns (3c–d). Simply put, none of these pronouns are the specifier of finite T.

Of course, any analysis of English case must also be able to account for the variable occurrence of (mismatched) SF pronouns in CoDPs (examples (5a)/(6a)/(7a)/(10a) above). Emonds (1986: 115–116) states that these are produced by 'ad hoc local transformations,' but does not go into detail about the mechanisms involved. Thus, I have introduced a novel element to Emonds's analysis by proposing that individuals may (but need not) learn 'supplemental' Vocabulary items in response to normative pressures. English supplemental pronoun Vocabulary insert a specific SF exponent only when the target D terminal is linearly adjacent to the CoDP head (indicated in the diagrams below with '*' following Embick 2007). Supplemental Vocabulary items for 1SG and 3SG pronouns are given below.

(17) a.   [D, +AUTH, +PART, –PL]   ⇔   /ai/ / [$_{CoDP}$ … [Co^0] * __ …]

      b.   [D, –AUTH, –PART, –PL, ♀]   ⇔   /ʃi/ / [$_{CoDP}$ … __ * [Co^0] …]

      c.   [D, –AUTH, –PART, –PL, ♂]   ⇔   /hi/ / [$_{CoDP}$ … __ * [Co^0] …]

Normative pressure is the most plausible reason that linear adjacency is part of the contextual information contained in supplemental Vocabulary. As most native speakers of English will recall, explicit instruction during elementary education prescribes that it is polite to put oneself 'last' — in other words, a 1SG pronoun must be the final conjunct in a CoDP (see Angermeyer & Singler 2003). In fact, most English speakers are not taught to use SFs in finite-subject CoDPs, but rather just to say *and I*.[11] Even if an individual is not herself the recipient of instruction, she will still be frequently exposed to this socially salient variant (see Grano 2006 for discussion of the relationship between frequency, salience, and prescription).

An individual whose Vocabulary inventory includes (17a), but contains no other supplementary Vocabulary items, will be able to produce 'mixed' OF/SF CoDPs (as in (6a)/(7a)/(10a)). Such a Vocabulary inventory is diagrammed in (18) below. The dotted/dashed line indicates that supplemental Vocabulary items do not compete for insertion. This is due to the Elsewhere condition mentioned above: The supplemental Vocabulary in (17) contain exactly the same amount of features and contextual information as the ordinary Vocabulary for SF pronouns in (13)–(14). Such non-competition between Vocabulary items is one of the hypothesized mechanisms of intra-individual variation, though not the only mechanism (e.g., Adger & Smith 2005, Adger 2006, 2007, and Nevins & Parrott, in press). Consequently, an individual with the pronominal Vocabulary inventory in (18) can variably produce *him and I, him and me*, or *me and him*, but not **he and I, *he and me, *I and him*, or **me and he*.

(18) [D, +AUTH, +PART, −PL]   ⇔   /ai/   / [$_{CoDP}$ … [Co^0] * __ …]

    [D, +AUTH, +PART, −PL]   ⇔   /ai/   / [$_{TP}$ __ [T[±PAST] …]]
    [D, +AUTH, +PART, −PL]   ⇔   /mi/   *elsewhere*

    [D, −AUTH, −PART, −PL, ♂]   ⇔   /hi/   / [$_{TP}$ __ [T[±PAST] …]]
    [D, −AUTH, −PART, −PL, ♂]   ⇔   /hɪm/   *elsewhere*
    [D, −AUTH, −PART, −PL, ♀]   ⇔   /ʃi/   / [$_{TP}$ __ [T[±PAST] …]]
    [D, −AUTH, −PART, −PL, ♀]   ⇔   /həɹ/   *elsewhere*

Although other individual inventories are possible on this theory, supplemental Vocabulary for 1SG pronouns are apparently much more common than supplemental Vocabulary for 3SG among English speaking populations.[12] The implication mentioned above (3SG SFs in CoDPs → 1SG SFs in CoDPs) has a social explanation. If an individual is sufficiently motivated by prescription to learn supplemental Vocabulary for 3SG pronouns, she will have also learned the

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[11]   See Quattlebaum (1994) for an interesting experiment with pedagogical methods and pronoun usage in CoDPs.

[12]   The large majority of English speakers appear not to learn supplementary Vocabulary for plural pronouns; see fn. 8 above. It seems likely that those who do have also learned supplemental Vocabulary for 1SG and 3SG. Thus, we can make another implicational prediction: Individuals who have supplemental Vocabulary for plural pronouns will also have supplemental Vocabulary for both 3rd and 1st singular pronouns (1/3PL SFs in CoDPs → 3SG SFs in CoDPs → 1SG SFs in CoDPs). See Parrott (2007: chap. 6) for some elaboration.

supplemental Vocabulary for 1SG pronouns.

Further elaboration of the present analysis would exceed the scope of this article (for additional details see references cited above and Parrott 2007: chap. 6).

3. Transparent and Vestigial Case

The previous section outlined Emonds's (1986) analysis of English pronominal case as implemented in DM. We now proceed to take a cross-linguistic perspective. Are pronominal case-form mismatches in CoDPs attested in other languages, or is this an English-specific anomaly?

3.1. German CoDPs

Emonds specifically predicts that CoDP case variation will be unattested in German. And in fact, numerous native speakers of German, both linguists and 'civilians,' have informed me that case mismatches inside CoDPs are not only unattested but completely unacceptable. This is illustrated for nominative/ accusative phrasal and pronominal CoDPs in (19)–(22), using masculine-gender nouns because these have distinct case forms.[13] Conjunct ordering permutations show that this factor is irrelevant to the unacceptability of case mismatch in German, unlike in English.

(19)   *German nominative CoDPs*

    a. * [**Den**   Mann und der   Hund] haben die   Katze gebissen.
         the.ACC man   and the.NOM dog   have   the.ACC cat   bitten

    b. * [Der   Mann und **den**   Hund] haben die   Katze gebissen.
         the.NOM man   and the.ACC dog   have   the.ACC cat   bitten
         'The man and the dog bit the cat.'

(20)   *German accusative CoDPs*

    a. * Die   Katze hat [**der**   Mann und den   Hund] gebissen.
         the.NOM cat   has the.NOM man   and the.ACC dog   bitten

    b. * Die   Katze hat [den   Mann und **der**   Hund] gebissen.
         the.NOM cat   has the.ACC man   and the.NOM dog   bitten
         'The cat bit the man and the dog.'

(21)   *German nominative CoDPs (pronouns)*

    a. * [**Mich**   und Stefan] haben Bier getrunken.
         me.ACC and Stefan have   beer drunk

    b. * [Stefan und **mich**] haben Bier getrunken.
         Stefan and me.ACC have   beer drunk.
         'Me and Stefan/Stefan and me drank beer.'

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[13]   Using masculine nouns allows us to abstract away from gender/case syncretisms in modern German, which are not numerous enough to reduce the transparency of case below the threshold necessary for acquisition. This situation could change over time, or in independently developing varieties of German, if the number of syncretisms increases sufficiently.

(22)   *German accusative CoDPs (pronouns)*

a. *Die   Polizei hat [Stefan und **ich**]   verhaftet.
    the.NOM police has Stefan and I.NOM   arrested

b. *Die   Polizei hat [**ich**   and Stefan] verhaftet.
    the.NOM police has I.NOM   and Stefan arrested
    'The police arrested Stefan and I/I and Stefan.'

(23) illustrates that post-copular nominals, whether full DPs or pronouns, occur with invariant nominative case in German. This shows German to be unlike English, where post-copular pronouns always occur as OFs, notwithstanding a very slight remnant of prescriptively induced variation, as mentioned above.

(23)   *German post-copular nominals*

a. Das   ist der   Hund.
    that is the.NOM dog

b. *Das   ist **den**   Hund
    that is the.ACC dog
    'That is the dog.'

c. Das   bin   ich.
    that am   I.NOM

d. *Das   bin   **mich**.
    that am   me.ACC
    'That/it is me.'

3.2.   *Transparent Case in German and Beyond*

If we accept the standard premise that mechanisms of case are the same in both languages, even granting special properties to coordination, it is not clear why variable mismatches in CoDPs are impossible in German but well attested in English. Of course, there is another obvious difference between these two languages. In German, phonologically distinctive case morphology (syncretisms notwithstanding, see fn. 13) appears not only on closed-class pronouns but on various elements that constitute open-class DPs. These elements include, *inter alia*, definite articles and pre-nominal adjectives. Nominative and accusative cases are exemplified below on masculine-gender DPs (24) and pronouns (25).

(24)   *German masculine DPs*

a. Der   knurrende   Hund   hat   den   Mann   gebissen.
    the.NOM snarling.NOM dog   has   the.ACC man   bitten
    'The snarling dog bit the man.'

b. Der   Mann hat   den   zitternden Hund   gebissen.
    the.NOM man   has   the.ACC trembling.ACC dog   bitten
    'The man bit the trembling dog.'

(25)   *German masculine pronouns*

Er   hat   ihn   gebissen.
he.NOM   has   him.ACC   bitten
'He bit him.'

Henceforth, I refer to German as having 'transparent' case, adopting Emonds's terminology in anticipation of the acquisition principle discussed in the next section. Case morphology can be called transparent if it is phonologically distinctive on relevant open-class categories, hence productive in the sense that all new nominals will have to express case. Transparent-case languages would thus include Icelandic and Faroese in the Germanic family,[14] as well as Greek, Czech, and other languages in various families.

Recall that on the present analysis, English pronominal case forms are allomorphs of contextual structure, with Vocabulary that do not contain any case features. Well-attested and otherwise mysterious variable mismatches in CoDPs, along with variation or default OFs in other structures like post-copular nominals, constitute strong evidence for the analysis. It is exactly this kind of variation that is unattested in German. Thus, we might draw the perhaps unsurprising conclusion that transparent case morphology, in German and relevantly similar languages, is in fact the exponence of (morpho)syntactic case features. Again, it is not necessary to take any position on whether these case features are checked/assigned in the narrow syntax, or determined in a post-syntactic morphological component as advocated by McFadden (2004) among others.

For concreteness, let us adopt the following case features for German (adapted from McFadden 2004, where they are assigned by post-syntactic morphological rules).

(26)   *Case features of German*
        a.   [+CASE, +GENITIVE, +OBLIQUE, +INFERIOR]   = Genitive
        b.   [+CASE, +OBLIQUE, +INFERIOR]   = Dative
        c.   [+CASE, +INFERIOR]   = Accusative
        d.   [+CASE]   = Nominative

These case features are contained in Vocabulary that provide exponence both to German masculine singular definite articles (27) and pronouns (28) (adapted from McFadden 2004: 221-223).

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[14]   The endangered variety Oevdalian, which is spoken by approximately 3000 people in one province of central Sweden, may have, or have had, case on open-class DPs (Sapir 2005, Dahl & Koptjevskaja-Tamm 2006, Svenonius 2008). Evidently, however, transparent case is dying or dead in the modern language (Piotr Garbacz, p.c.). Further research is underway to address this and other questions about case in Oevdalian.

(27)   *German Vocabulary for D[+definite], masculine singular*

[+CASE, +GENITIVE, +OBLIQUE, +INFERIOR, −FEM]	⇔	/des/
[+CASE, +OBLIQUE, +INFERIOR, −FEM]	⇔	/dem/
[+CASE, +INFERIOR, −FEM, −NEUT]	⇔	/den/
[+CASE, −FEM, −NEUT]	⇔	/dɛɹ/

(28)   *German Vocabulary for pronominal D, masculine 3rd person singular*

[+CASE, +OBLIQUE, +INFERIOR, −FEM, −AUTH, −PART, −PL]	⇔	/im/
[+CASE, +INFERIOR, −FEM, −NEUT, −AUTH, −PART, −PL]	⇔	/in/
[+CASE, −FEM, −NEUT, −AUTH, −PART, −PL]	⇔	/ɛɹ/

English pronominal case-form allomorphs are the exponence of structural context; for that reason, mismatches occur in structures such as CoDP. German case forms are the exponence of (morpho)syntactic case features; these case features are checked/assigned normally inside CoDP and thus mismatches do not occur. In other words, it is not that underlying mechanisms of case are the same in German and English, but special case-interfering properties of coordination are parametrically different. Rather, it is the other way around. Coordination is the same in both languages, but the mechanisms that produce morphological case are significantly different. Predictions based on German can be extended to all other transparent-case languages, where mismatched case forms in CoDPs should be completely unattested. This prediction appears to be robustly supported. For Icelandic and Faroese, there are no reports of such variation in the literature (e.g., Thráinsson 2007, Thráinsson *et al.* 2004). Several linguists who are native speakers of Icelandic have confirmed for me that case mismatch in CoDPs is impossible. Fieldwork with non-linguist native speakers in the Faroe Islands provides further corroboration (Parrott to appear).

Before proceeding, it should be emphasized that although it does not occur in CoDPs and the other syntactic environments relevant for English (and Danish, below), intra-individual case variation is in fact observed in transparent-case languages. Two types are well known. The first is variation between dative and accusative case on objects of certain prepositions. The second is variation in the case of non-nominative finite-clause subjects of certain (typically experiencer or similarly themed) verbs. In Icelandic, the latter type is quite common and is associated with normative attitudes. Because it involves dative case on subjects of verbs for which other cases are prescribed, this variation is popularly known as 'dative sickness' (see, e.g., Jónsson & Eythorsson 2005, Thráinsson 2007: 224).[15] Non-nominative finite-clause subjects are simply impossible with experiencer or any other verbs in modern English (and Danish, below). These facts constitute further support for the theory being argued for in this article. If mechanisms of case are the same in German (or Icelandic, etc.) and English (or Danish, etc.) then why could there not be variation in CoDPs in the former, or OF finite-clause subjects in the latter? Further consideration of case variation in transparent-case languages would take us too far afield; for more discussion see references cited.

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[15]   Linguists may prefer the somewhat more neutral term 'dative substitution'.

3.3. Vestigial Case in Danish

English used to be a transparent-case language like German *et al.* (van Kemenade 1994). However, independent phonological changes 'erased' case morphology on open-class nominal phrases in English (Allen 1995, Quinn 2005). The only case-like remnants left behind were suppletive allomorphs within a closed subset of pronouns. The present analysis of English holds that pronominal allomorphs are the exponence of syntactic structural context and that their Vocabulary do not include any case features. Hereafter, this state of morphological affairs will be referred to as 'vestigial' case. Typologically speaking, of course, English is not the only Germanic vestigial-case language. In addition to Dutch, Afrikaans, and Frisian (König & van der Auwera 1994), we find the so-called 'mainland Scandinavian' varieties, comprising Norwegian, Swedish, and the focus of this article, Danish. All of these languages have pronominal case-form allomorphs but lack case morphology on open-class nominal phrases.[16]

Above, it was predicted that CoDP case variation will be unattested in transparent-case languages like German. A converse prediction is that pronominal case-form mismatches in CoDPs, and perhaps additional environments, will be attested in vestigial-case languages other than English. This prediction is robustly supported for Danish, whose pronominal case-form allomorphs are given in (29) below.[17] Danish has distinctive case forms for 2nd person pronouns in both singular and plural (but like English, there is no distinction for 3SG *det/den* 'it').[18]

(29)   *Danish pronominal case-form allomorphy*

	Subject Form (SF)	Oblique Form (OF)
1SG	*jeg*	*mig*
2SG	*du*	*dig*
3SG	*hun* (♀) / *han* (♂)	*hende* (♀) / *ham* (♂)
1pl	*vi*	*os*
2pl	*I*	*jer*
3pl	*de*	*dem*

Pronominal case-form variation in CoDPs and other structures is salient to

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[16]   Yiddish has lost most (but perhaps not all) traces of case on nominal phrases, but retains dative pronouns (König & van der Auwera 1994), as do certain varieties of Swedish and Norwegian (Jørgensen 2000). The status of such dative-retaining vestigial-case languages is an open and intriguing research question on the current approach.

[17]   As above for English (see fnn. 2, 6, and 10), I do not discuss Danish possessive pronouns here. Interestingly, Danish also has a possessive DP clitic *–s*.

[18]   Danish has a transparent 2-gender system with agreement on articles and both pre-nominal and predicate adjectives. Masculine and feminine have been syncretized to a common gender, which contrasts with a neutral gender. There are some Danish varieties that still maintain a three-gender system, but these may be in decline because of dialect leveling to the Copenhagen 'standard'. *Det* is the neutral form of the 3SG pronoun 'it' and *den* is the common-gender form, but their distribution differs in other ways (Allan *et al.* 1995: 154f., 157–160). As in English, the other 3SG pronouns (*hun/hende* and *han/ham*) refer to semantic (biological sex of humans) rather than grammatical gender.

native speakers, and has been pointed out by Danish scholars (Jørgensen 2000, Hansen & Heltoft 2007, Pedersen 2008). Despite remarkably exact parallels to English, however, case variation in Danish has not been discussed or analyzed in the linguistic literature to my knowledge. One very concise exception is Allan *et al.*'s grammar of Danish, which reports the following (1995: 145):

> In colloquial language, the objective form *mig* is sometimes used as subject […]. This happens mostly in coordination with a noun phrase, irrespective of the order of the two (or more) coordinated elements, though it is felt to be even more informal when the personal pronoun appears in first place […].

(30)   *Adapted from Allan* et al. *(1995: 145)*

   a.   [Min bror   og   **mig]** er   gode venner.   *Danish*
        my   brother   and   me.OF   are   good   friends

   b.   [**Mig**   og   min   bror]   er   gode venner.
        me.OF   and   my   brother   are   good   friends

We can infer the existence of intra-individual variation from normative attitudes. After all, it not possible to prescribe against forms that are never used. The following examples of mismatch in CoDPs (31) are adapted from Hansen (1988), in the section titled "*They* or *them, she* or *her*?" Such examples are taken as representative, among many other similar examples from the Danish prescriptive literature (e.g., Oxenvad 1976).

(31)   a.   Kun [min sekretær   og   **mig**] kender adressen.   *Danish*
            only   my   secretary   and   me   know   address.DEF

       b.   Adressen   kendes   kun   af   [min sekretær   og   **jeg**].
            address.DEF known.PASS only   by   my   secretary   and   I

As predicted — both by the present theory, and by inference from prescriptive literature — pronominal case-form mismatches in CoDPs appear to be very well attested in both written and spoken Danish. (32) is attested from an email,[19] and is comparable to English (4) above.

(32)   **OFs** *in finite-clause subject CoDP*   *Danish*

       … [**mig** og   **dig**   og   F.]   går   ind   i   det   nye   udvalg.
          me   and   you.OF   and   F.   go   into   in   the   new   committee

The attestations in (33) were collected from a corpus of written Danish,[20] and are comparable to English (5a) and (7a), respectively.

(33)   **SFs** *in prepositional and verbal object CoDPs*   *Danish*

   a.   En terapi   med [hende   og   **jeg**] ville have været
        a   therapy   with her   and   I   would have been

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b.   … (at,) jeg ikke er ked af, at   det ikke blev [ham og   **jeg**].
          that I   not am sad of INF it   not   become him and I
   '… (that) I am not sad (we didn't become a couple).'

(34) was spoken aloud.[21] Note that the coordinate head is *eller* 'or' rather than *og* 'and'. This fact supports the theoretical claim (made in section 2.3 above) that supplemental Vocabulary refer to the coordinate head itself, and not to the phonological or other features that specifically distinguish *and/og* from *or/eller*.

(34)   **SF** *in prepositional object CoDP*   *Danish*

De   problemer kan   loses   af [L. eller **jeg**].
they   problems   can   solved.PASS by L. or   I

The attestations in (35) were all spoken aloud by adults and recorded as part of the pilot child-language study to be discussed in the next section.[22] (35a–b) are comparable to English (4) above, and (35c) to (6a).

(35)   **OFs** *(and SF) in finite-clause subject CoDPs*   *Danish*

a.   … [far   og   **mig**] blev gift.
          father   and   me   got   married

b.   … at [**dig**   og   far]   ligner   hinanden   lidt.
          that   you.OF and   father   look-like each-other little

c.   [**Ham**   og   jeg] var faktisk   sammen.
          him   and   I   were in-fact   together

There also appear to be pronoun-specific ordering and implication effects in Danish, similar to those observed in English. While OFs appear in either conjunct (compare (32) and (35a–b) above), *jeg* seems to be restricted to the second conjunct, regardless of whether the CoDP is a subject or object, as illustrated in (36).

(36)   a.   * [Jeg og **ham**] var faktisk sammen.   *Danish*
         b.   * En terapi med [**jeg** og hende] …

Moreover, a 3SG SF is apparently not acceptable with a 1SG OF in either subject or object CoDPs, as illustrated in (37).

(37)   a.   * [Han og **mig**] var faktisk sammen.   *Danish*
         b.   * En terapi med [**hun** og mig] …

Preliminary consultation with native speakers confirms the unacceptability of (36-37), but should be corroborated with empirical studies utilizing question-naires and/or interviews to elicit reliable 'acceptability reactions' (Schütze 1996)

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21   Uttered by a student at the University of Copenhagen; overheard and documented by Jacob
       Thøgersen.
22   Transcribed by René Staustrup.

to these and other constructions.[23]

There do not appear to be normative attitudes regarding post-copular nominals in Danish, and pronouns in this environment are categorically OFs. The attestations in (38) were spoken by adults in the same recording mentioned directly above, and are comparable to English (8-10).

(38) a.   Det   er   J.,   det   er   dig   og   det   er   mig.   *Danish*
            it   *is*   *J.*   *it*   *is*   *you.OF*   *and*   *it*   *is*   *me*

      b.   Det   er   [K.   og   dig].
            it   *is*   *K.*   *and*   *you.OF*

      c.   Det   er   [dig   og   K.   og   C.]
            it   *is*   *you.OF*   *and*   *K.*   *and*   *C.*

      d.   Det   er   også   [dig   og   S.]   der   kører.
            it   *is*   *also*   *you.OF*   *and*   *S.*   *there*   *driving*

SFs as post-copular nominals are unacceptable and unattested, probably due to the lack of prescription for this environment.

(39) *Det er du og det er jeg.   *Danish*

Danish looks remarkably similar to English with respect to variation and the distribution of case-forms in syntactic structures such as CoDP and post-copular nominals.[24] We can conclude, then, that the morphological mechanisms for case forms of Danish pronouns are the same as in English — that is, the pronoun Vocabulary do not contain any case features. Danish pronominal case forms are allomorphs of structural context: SFs are inserted when a pronoun is the specifier of finite T, and OFs are elsewhere items inserted in all other structural contexts (see (11) and (12) above). For concreteness, Vocabulary for Danish 1SG and 2SG pronouns are given in (40)–(41); Vocabulary for the other pronouns follows the same schema.

(40) [D, +AUTH, +PART, −PL]   ⇔   /jai/   / [$_{TP}$ __ [T[±PAST] …]]
      [D, +AUTH, +PART, −PL]   ⇔   /mai/   *elsewhere*

(41) [D, −AUTH, +PART, −PL]   ⇔   /du/   / [$_{TP}$ __ [T[±PAST] …]]
      [D, −AUTH, +PART, −PL]   ⇔   /dai/   *elsewhere*

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[23]   On the present analysis, we also might expect to find variably mismatched pronouns in CoDPs and other structures when Danes are speaking English. And indeed, I have overheard the following attestation:

          (i)   Him and I played Hendrix together.

[24]   Pedersen (2008) states that for certain Danish varieties, for example those spoken in southern Jutland and the island of Bornholm, 'nominative is dominant' in subject CoDPs, post-copular pronouns, and the other relevant structures. Pedersen attributes this to the influence of Swedish (see below), especially for Bornholm. However, she concedes that such SF usage is characteristic of 'older' varieties. Further empirical research is necessary in order to determine what the current situation is and whether there is any change in progress that might be observable in apparent time (e.g., Bailey 2002).

Danish pronoun-headed relative clauses (PhRCs) offer additional support for the present analysis. PhRCs are rarely used in modern spoken English, with the exception of a few set expressions (e.g., *He who must not be named* from the Harry Potter book and film series). But pronominal case-form variation in this construction is known to Danish scholars (Jørgensen 2000, Hansen & Heltoft 2007, Pedersen 2008) and appears to be a chief concern in the prescriptive literature, as exemplified in (40) with constructed examples modified from Hansen (1988). Below, the relative clause is bracketed and the mismatched pronoun is in bold.

(42) a. ***OF** in finite-clause subject PhRC*
          [**Ham**,   der   står   derovre],   er   min nabo.
          him   who   stands   there-over   is   my   neighbor

      b. ***SF** in prepositional object PhRC*
          Blandt [**de**, der hjalp familien], var især   naboerne.
          among   they who help family.DEF were especially neighbors.DEF

Case-form variation in this construction is predicted directly by the theory under discussion. A pronoun heading a relative clause is embedded in a DP structure, so it cannot receive SF exponence and an elsewhere OF will be inserted by default (42a).[25] Supplemental Vocabulary, learned in response to normative

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[25] Danish is a matrix V2 language, but a full consideration of the issues raised thereby would take us far beyond the scope of this article. Very briefly, consider (43) in its complete sentential context (i). Following standard analyses, the PhRC DP has raised to the specifier of CP from its Merged position inside VP, with an intermediate stop in the spec of TP to satisfy EPP; copies left by phrasal movement are indicated below with angled brackets (Chomsky 1995, 2000 *et seq.*, Hornstein 2001).

(i)   *Structure of (42a); T = T[−PAST]*
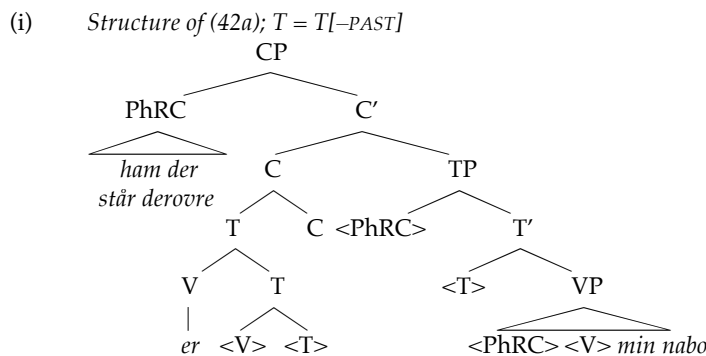
Now we must grapple with a more difficult question: What is the status of head movement? If it is an instance of generalized syntactic movement, the standard view, it will leave copies as shown in (i) above. Then we might say that pronominal Vocabulary like those in (41)–(42) can 'see' copies, so that SFs can be inserted in V2 subjects because their copies are in the specifier of finite T. But what if head movement is a (wholly or partially) post-syntactic operation (Chomsky 2001, Boeckx & Stjepanović 2001, Parrott 2001, Matushansky 2006)? Will morphological head movement still leave copies? If not, we might postulate that pronominal Vocabulary do not in fact refer to the finite T head itself, but only to the specifier position of finite T. I leave must leave the matter here, but see Parrott (2007: chap. 6, 2008) for a more elaborated discussion with regard to English pronouns and T–to–C

attitudes, will account for mismatched SFs in PhRCs (42b).

(43) *Structure of Danish PhRC (42a)*

```
            DP
           /  \
          /    \
        D       CP
        |      /  \
      ham   der står derovre
```
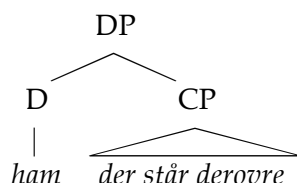
Further empirical research on Danish pronoun case variation is currently underway, utilizing a large corpus of sociolinguistic interviews collected in several locations across Denmark from the 1970s to the present. The LANCHART corpus (Gregersen 2007, in press) will be first be searched for coordinated and post-copular pronouns, followed by PhRCs and other structures. The long-term goal is to code every pronoun for its syntactic context, making possible an exhaustive analysis of pronominal case-form distribution and variation in Danish. Initial results from the LANCHART corpora are reported in Hilton & Parrott (2009). We extracted 513 coordinated pronouns from a subsection of the corpus consisting of about 2.58 million 'words'[26] (about 1 coordinated pronoun per 5000 'words'). Of these, 92 (about 18%) contained mismatched case forms, with all mismatch types attested (OF in a subject CoDP, SF in an object CoDP, mixed SFs/OFs in subject and object CoDPs). Extrapolating based on this sample, we estimate that around 1400 coordinated pronouns will be found in the entire LANCHART corpus (approximately 7 million 'words'), with about 280 (20%) of these containing mismatches.

3.4. *Vestigial Case in Mainland Scandinavian and Beyond*

On the theory developed in this article, one possible prediction is that Norwegian and Swedish, the other mainland-Scandinavian vestigial-case languages, should also have pronominal case-form variation in CoDPs. However, this prediction is evidently much too strong: it is contradicted by the facts of Swedish and to some extent Norwegian. In general, it must be said that matters look quite a bit more complicated for Swedish and Norwegian than for Danish. The linguistic situation in Denmark could be described as mono-centric: regional dialect diversity has been reduced in favor of a supra-local 'standard' based on varieties spoken in the capital, Copenhagen. In contrast, Norway has not one but two official written standards (Bokmål and Nynorsk), along with a plethora of regional varieties whose use is typically sanctioned rather than stigmatized by popular and normative attitudes. Sweden also has a remarkable variety of regional dialects, in addition to Swedish varieties spoken in Finland. Adding to the complexity of this picture, certain varieties of both Norwegian and Swedish retain dative pronouns, or have (variable) syncretism of the SF/OF distinction for particular pronouns

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raising in questions.

[26] 'Words' are defined as non-empty intervals, and thus include hesitation noises, false starts, repetitions, etc.

(Jørgensen 2000), for example Norwegian 3pl *de/dem* 'they/them' (Hilton 2009).

Keeping in mind these complicating factors, Swedish nevertheless looks quite unlike either Danish or English with respect to case variation. Thráinsson (2007: 185) reports that mismatched OFs in subject CoDPs are unattested and unacceptable. I have confirmed this with several native speakers of Swedish, including linguists and 'laypeople.' Moreover, both isolated and post-copular pronouns are invariantly SFs (Sigurðsson 2006, Thráinsson 2007). Thus at first glance, contrary to the morphological transparency hypothesis presented below, Swedish seems to behave like a transparent-case language. However, this conclusion cannot be maintained after a closer look. For one thing, unlike Icelandic, German, or other transparent-case languages, Swedish does not allow non-nominative finite-clause subjects. Moreover, there is at least one kind of case-form variation that may be unique to Swedish, yet seems unlike anything found in transparent-case languages. Holmberg (1986) reports that in one northern dialect, SFs occur variably as verbal and prepositional objects. To my knowledge, Holmberg provides the only English-language discussion and analysis of this phenomenon. Such a pattern of variation is not predicted to occur in a transparent-case language, and no such variation has been reported in one, to my knowledge.

Thus, it could be maintained that Swedish pronouns are allomorphs of structural context, but that their morphology is nonetheless different than Danish and English. As a very preliminary sketch, suppose that Swedish Vocabulary insert OF exponents when the pronoun is an object — say, when it is the complement of a head — and that SFs are elsewhere items inserted for pronouns in any other context. Important questions remain. Why is there no case-form variation in Swedish CoDPs? How did Swedish develop such a different pronominal morphology than Danish, a closely related language? More empirical research will be required to establish what patterns of case variation are (not) found in Swedish.

Turning to Norwegian, we find remarkable dialect diversity and (variable) case syncretisms, as noted above. However, unlike Swedish, the predicted mismatches in CoDPs have been attested in varieties of Norwegian.[27] Johannessen (1998, see also Schütze 2001: 226 for a summary and discussion) provides several examples from dialects spoken in Bergen, Stavanger, and Tromsø, but cites only older sources (Berntsen & Larsen 1925, Larsen & Stoltz 1912). On this basis (and presumably also as a native speaker), Johannessen concludes that when mismatched pronouns occur in CoDPs, either the first conjunct must be a SF, or both conjuncts must be OFs. In two corpora of sociolinguistic interviews conducted in Oslo and Hønefoss, consisting of one million 'words' in total, Hilton & Parrott (2009) report only three attestations of unambiguous pronominal case mismatch in CoDPs. Because none of these attestations have pronouns in both conjuncts, it

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[27] According to Sigurðsson (2006), post-copular pronouns are OFs in "most varieties of Norwegian," but in a footnote he seems to suggest that there is intra-individual, sociolinguistic variation: "[M]ost speakers can apply only the accusative [OF], while other speakers can apply either the everyday accusative [OF] or the more 'conscious' nominative [SF] (perhaps due to the influence of language planners)" (p. 15, fn. 16, of the pre-print manuscript from http://person.sol.lu.se/HalldorSigurdsson/HS/TheNomAcc.pdf). Unfortunately, Sigurðsson does not discuss case variation in Norwegian CoDPs.

is not possible to determine whether in fact they conform to Johannessen's alleged pattern. One example is given below.

(44)   *OF in subject CoDP*   *Norwegian*
    [**Meg**   og   M.]   er   jo   hva   skal   vi gjøre.
    me   *and*   *M.*   *are*   *like*   *what*   *should*   *we do*

Thus, from this incomplete and very preliminary inquiry, Norwegian apparently shows patterns of case variation that are similar to those found in both Danish and Swedish. It is not clear at this point whether the phenomena are limited to inter-individual variation between different Norwegian varieties, whether there is evidence of intra-individual variation with associated sociolinguistic attitudes, or both. Further empirical investigation will be required to resolve these and other outstanding questions.

Finally, pronominal case-form variation needs more empirical investigation in the remaining Germanic vestigial-case languages, namely Afrikaans, Dutch, and Frisian. According to Sigurðsson (2006), post-copular nominals are OFs in North Frisian, but SFs in Afrikaans, Dutch, and West Frisian. CoDPs in those languages are not discussed. If Sigurðsson's facts are correct, and if the analysis presented in this article is on the right track, then variable case-form mismatches would be predicted to occur in North Frisian CoDPs. In Afrikaans, Dutch, and West Frisian, we might expect to find patterns of case-form variation similar to Swedish, for example variably mismatched SF objects, as mentioned above (Holmberg 1986).

4.   Case and the Acquisition of Vocabulary

Why are the morphological mechanisms of pronominal case-form allomorphy in English and Danish different from those in transparent case languages? Why can't English and Danish pronoun Vocabulary simply contain Case/case features, as in German (cf. (6)–(8) above)? Emonds's (1986) important insight is to explain both the inter- and intra-individual variation in Germanic case morphology with a principle of language acquisition.

4.1.   *Morphological Transparency and the Acquisition of Vocabulary*

Simply put, Emonds hypothesized that the acquisition of morphosyntactic exponence is limited by what is phonologically distinctive in the child's environmental linguistic input. This basic idea is quite consistent with a Minimalist-DM theoretical architecture. Plausibly, Vocabulary items and all other objects and operations of the post-syntactic morphological PF interface component constitute the exclusive loci of inter-individual variation; it follows that patterns of intra-individual variation have the same loci. Such morphological objects or operations, the loci of all variation, are not provided by UG and therefore must be learned on the basis of perceptually distinctive linguistic stimuli. As Chomsky (1993: 3, emphasis mine — JKP) states:

Variation must be determined by what is 'visible' to the child acquiring language […]. **It is not surprising** […] **to find a degree of variation in the PF component, and in aspects of the lexicon** […]. Variation in the overt syntax or LF component would be more problematic, since [acquisition] evidence could only be quite indirect. A narrow conjecture is that there is no such variation: [B]eyond PF options and lexical arbitrariness […], **variation is limited to nonsubstantive parts of the lexicon and general properties of lexical items.**

Emonds (1986: 106f.) formalizes the notion that morphosyntactic features must be phonologically 'visible' for acquisition.

(45) *Morphological transparency*

Definition. A syntactic category C is "morphologically transparent" on B if and only if a productive number of pairs of simple B which contrast with respect to C also differ phonologically.

(46) *Morphological transparency as a constraint on acquisition* (Emonds 1986)

Morphological Transparency. An abstract (e.g., case) feature C of a category B is realized on the lexical head of B in a language if and only if the C is morphologically transparent on B.

Implementing Emonds's Morphological Transparency hypothesis in DM yields the following.

(47) *Morphological transparency in DM*

A morphosyntactic feature F (e.g., [±inferior]) is morphologically transparent on an abstract terminal morpheme M (e.g., [D^0]) if and only if a productive number of pairs of simple M which contrast with respect to F also differ phonologically.

(48) *Transparency constraint on acquisition of morphology*

A morphological operation or object (e.g., Vocabulary item) that modifies M may contain a morphosyntactic feature F if and only if F is morphologically transparent on M.

Emonds's formulation of the transparency hypothesis raises numerous questions, all of which cannot be resolved here. For instance, what definition of 'productive' is pertinent for transparency? Emonds in fact defines 'productive' in a footnote (1986: 106, fn. 6).

Productivity. A linguistic construction is 'productive' if the number of different forms that the construction may take is not limited by virtue of linguistic rules or principles. For example, the category ADJECTIVE is productive in English, but the category of TENSE endings on verbs is not.

Although the concept of productivity is somewhat intuitive, Emonds's definition is not straightforward from the theoretical perspective adopted here. In DM

theory (Embick & Noyer 2007), the category 'adjective' consists of a root (i.e. 'lexical') morpheme with relevant semantic features that is adjoined to an adjectival categorizing morpheme during the morphosyntactic derivation. The category 'tense' consists of an abstract (i.e. 'functional') morpheme with semantic features such as [±PAST]. Although roots have inherent phonological feature content, abstract morphemes must be supplied with phonological features by post-syntactic Vocabulary insertion. It cannot be the case that all abstract morphemes are defined as non-productive, otherwise no feature could be morphologically transparent on any abstract morpheme. And indeed, the definite article (D[+DEFINITE]) is a primary locus of case exponence in German. Definite articles are a closed class (i.e. non-productive), and D is an abstract morpheme. But case features are clearly transparent on all German determiners. Intuitively, of course, productivity results when D is combined with open-class NPs. But this still leaves the question of how some 'number of pairs of simple' D could be productive for transparency.

Perhaps the problem here is not with productivity, but rather with 'pairs of simple' morphemes. Are only pair comparisons relevant for transparency? And must the pairs consist, for example, of simple Ds, or could they be pairs of DPs? It does seem clear that the threshold 'number of pairs' required for transparency is an empirical question to be settled by examining specific cases.[28] However, if a relevant category is productive, then there are, in principle, an infinite number of possible pair comparisons. Surely this means that when contrastive features are being compared for phonological distinctiveness, productivity will suffice to exceed the necessary threshold for transparency.

Whatever the precise answers turn out to be, a meager four contrastive SF/OF pairs among the closed set of pronouns clearly do not constitute an adequately "productive number of pairs" to make case features transparent on D in English. Nor do six contrastive pairs suffice for Danish. Thus, by hypothesis, no child with English or Danish as her environmental linguistic input will be able to acquire a morphological case system like that learned by her German or Faroese counterpart. She must learn a different morphological system that will account for the allomorphic distribution of pronominal case forms. As evidenced by variation and mismatch in CoDPs and other structures, a child exposed to English or Danish (and possibly varieties of Norwegian or Frisian) will acquire pronominal Vocabulary that are sensitive to structural context, such that SFs are the exponents of finite-subject pronouns and OFs are elsewhere items. Keeping to the transparency hypothesis, a child exposed to Swedish (and possibly Afrikaans, Dutch, or varieties of Frisian) also should not be able to acquire case features in her Vocabulary. However, it is not necessary that she acquire the same pronominal morphology as her Danish (and so on) counterpart. Evidently, SFs are the elsewhere pronoun exponents in Swedish. It remains to be discovered why this difference exists.

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[28] This is not dissimilar to Lightfoot's (1999) idea that a child must be exposed to environmental structural 'cues' at some statistical threshold of frequency in order to set a parameter.

4.2.   A Pilot Study on Danish Child Language

The morphological transparency hypothesis for case can be directly tested by observing children's production of pronominal case-forms in CoDPs and the entire range of syntactic structures discussed above and elsewhere (Schütze 2001, Grano 2006). The prediction is that young children acquiring a vestigial-case language like English will not use SF pronouns in post-copular nominals, nor in any CoDPs, even (and especially) in finite-clause subject CoDPs. Unfortunately, coordinated pronouns are evidently rare in child speech. But initial inquiry suggests that the prediction will be confirmed in Danish.

The following attestations of mismatched OFs in finite-subject CoDPs come from an article titled "7 days with Clara Suhr, 6 years old," which was published on 8 December 2000 in the Danish newspaper *Politiken*.[29] Of course, there is no way to be absolutely certain that these were actually uttered by the child or recorded accurately by the journalist. Even if they are not accurate, however, these examples would at least indicate that OFs in subject CoDPs are regarded as 'childish' usage.

(49)   ***OFs*** *in finite-subject CoDPs*   *Danish*

    a.   [Cille og **mig**] legede ved vandet.
        Cille and me played by water-the

    b.   [Cille og **mig**] har næsten lige været med hende.
        Cille and me have almost just been with her

    c.   [L. og L. og J. og M. og **mig**] lavede pigebord og fjollede.
        L. and L. and J. and M. and me made girl-table and fooled-around

    d.   Nu skal [Cille og **mig**] se Pokémon
        now will Cille and me watch Pokémon

I will now report the results of an observational pilot study of Petra,[30] a Danish child aged 3;1 years at the time of recording. Petra was recorded in conversations with her father and mother while working in the kitchen, eating a meal, playing with toys, and looking at photos. The parents were aware of the broad research objective and did attempt to elicit coordinated pronouns by asking Petra questions about photos and other topics.

First of all, it is important to observe that Petra consistently uses the 'correct' (i.e. adult-like) pronoun case-forms as the non-CoDP subjects of tensed clauses. This shows that Petra has already acquired pronominal allomorphy.

(50) Blev jeg også gift?[31]   *Danish*
     get I also married

As predicted, Petra invariantly uses OFs as post-copular nominals. This is illustrated in (51) below; see the appendix below for additional tokens.

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29   "7 døgn med Clara Suhr, seks år," from 'Korpus 2000' (http://korpus.dsl.dk), collected with Jacob Thøgersen.
30   This is a pseudonym.
31   For context, see (35a) above.

(51) a.   Det   var   **os**.
             it   was   us

      b.   Det   er **mig**!
             it   is me

As predicted, she also uses OFs invariantly in post-copular CoDPs (52). Again, this shows that Petra has already acquired coordination. Notice that OFs occur in both first and second conjuncts.

(52) a.   Det   var   [**mig** og   far].
             it   was   me   and   father

      b.   Det' [min far   og   **mig**].[32]
             it [is] my   father and   me

There is one example of Petra using an OF *os* 'us' as a demonstrative within a post-copular nominal phrase *os to* 'us two' (53).

(53) Det   er **os** to.
      it   is us two

In one example, she uses an OF as an isolate pronoun, in response to a question formed from a post-copular nominal (see A3 in the Appendix).

(54) Ja   også   **mig**.
      yes   also   me

Finally, in another example, Petra uses an OF in an isolate CoDP, in response to an object *wh*-question (see A17 in the Appendix).

(55) [**Mig** og   morfar].
      me   and   grandfather

Unfortunately, no attestations of the most crucial kind of mismatch — OFs in subject CoDPs — were recorded in this pilot study. But even though the results are not conclusive, they are still suggestive and completely consistent with the theory advocated in this article.

Future research on the acquisition of Danish pronominal case forms will utilize both observational and experimental methods. It may be possible to elicit coordinated pronouns, especially as finite subjects, with a number of different designs. For example, children might look at a picture book that depicts a family outing without text, and then explain to their parents what is happening. Additionally, children might be asked to talk about what they did with their friends at school.

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[32]   The apostrophe transcribed here (*det' os*) indicates a phonologically reduced form of the Danish copula, where the /r/ of *er* or *var* is glottalized. This occurs not only in child language, but also in adult speech.

5.   Concluding Remark

As mentioned above, the theory presented so far is compatible with the standard view that semantically uninterpretable abstract Case features are checked/valued in the narrow syntax (as in, e.g., Chomsky 2000 *et seq.*, Adger 2003, Hornstein *et al.* 2005), or with emerging proposals that case features are only assigned/realized in the post-syntactic morphological component (McFadden 2004, Sigurðsson 2009). Either way, the Case/case features must be mapped onto their phonological exponents: that is, the child must learn Vocabulary. However, if this story is at all on the right track, it would seem to favor a theory of post-syntactic case. If Case features are checked in the narrow syntax, then Case is endowed by UG and available to the child without any need for learning from environmental input. If that were the case, it is hard to see why anything like the transparency constraint would be operative. Even a small set of pronoun allomorphs ought to be sufficient to signal the correct mappings of phonological features to Case features. But if case features are only assigned/realized post-syntactically, say by morphological rules that refer to syntactic structures (McFadden 2004), then these rules too must be learned on the sole basis of environmental input and would thus be subject to transparency.

Appendix: Complete List of Tokens

The following comprise all Petra's tokens of coordinated, post-copular, and isolated pronouns extracted from an approximately one-hour-long recording. They are presented in order of occurrence, and with some discourse context. All the child's pronouns are in boldface font, and CoDPs are bracketed.

(A1)   *OF as post-copular nominal*

Father:	Hvem	var	det	der	gik	ind	i	hulen?
	who	*was*	*it*	*who*	*went*	*into*	*in*	*cave-the*
Petra:	Det'	**os.**[33]
	it [was]	*us*

(A2)   *OF in post-copular CoDP*

Mother:	Hvem	var	det?
	who	*was*	*it?*
Petra:	Det	var	[**mig**	og	far].
	it	*was*	*me*	*and*	*father*

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[33]   See fn. 32 above.

(A3) *OF as isolate, from post-copular nominal*

Mother: Hvem   var   med   på   Christiania?
who   was   with   at   Christiania

Petra: Det   var   far.
it   was   father
[…]

Mother: Og? Far   og mor?   Far? Var det bare mor   og far?
and   father and mother   father was it   just mother   and father

Petra: Ja   også   **mig**.
yes   also   me

(A4) *OF as post-copular nominal*

Father: [in a funny voice] Hvem   slukkede   lyset?
who   turned-out light-the

Petra: Det   er **mig** Barbarpappa. […] Det var **mig** Barbarpappa.
it   is me   Barbapappa   it   was me   Barbapappa

(A5) *OF as post-copular nominal*

Father: Så spørger Barbapappa   hvem   slukkede   lyset?
so asks   Barbapappa   who   turned-out light-the

Petra: Det   var   **os**.
it   was   us

(A6) *OF in post-copular CoDP*

Father: Hvem   er os?
who   is us

Petra: Det   er   [far   og   **mig**].
it   is   father   and   me

(A7) *OF as post-copular nominal*

Father: Hvem   bor   i   hytten?
who   lives   in   cabin-the

Petra: Det   er **os**.   Det' **os**.[34]
it   is   us   it [is] us

(A8) *OF in post-copular nominal*

Father: Hvem   slukkede   lyset?
who   turned-out light-the

Petra: Det   er **mig**! [laughs] Det   er **mig**!
it   is me   it   is   me

---

[34] See fn. 32 above.

(A9) **OF** *as post-copular nominal*

    Father: Hvem slukkede nu lyset?
                 who turned-out now light-the

    Petra: Det er **os**.
               it is us

(A10) **OF** *in post-copular CoDP*

    Father: Hvem er os?
               who is us

    Petra: Det' [min far og **mig**].[35]
               it my father and me

(A11) **OF** *as post-copular nominal*

    Father: Hvem tændte lyset?
               who turned-on light-the

    Petra: Det er **mig**, det var **mig** Barbapappa.
               it is me it was me Barbapappa

(A12) **OF** *as a demonstrative in a post-copular nominal phrase*

    Mother: Du var i zoologisk have, Petra, hvordan var det.
               you_SF were in zoological garden Petra how was it

    Petra: Det er **os** to.
               it is us two

(A13) **OF** *as post-copular nominal*

    Mother: Se der er en love.
               see there is a lion

    Petra: Det er **os**.
               it is us

(A14) **OF**s *as post-copular nominals*

    Mother: Hvem er så det der?
                who is so it there

    Petra: Det er **mig**.
               it is me

    Mother: Og hvem er du sammen med?
               and who are you.SF together with

    Petra: C.

    Mother: Det er dig og K., og C. Hov! Hvem er det der […]?
               it is you and K. and C. hey who is it there

    Petra: Det er **mig**.
               it is me

---

[35]   See fn. 32 above.

(A15) *OF as post-copular nominal*

Father:	Hvem	er	det	der	sidder	og	smiler?
	who	*is*	*that*	*there*	*sitting*	*and*	*smiling*
Petra:	Det	er	**mig**.				
	it	*is*	*me*				

(A16) *OF as post-copular nominal*

Mother:	Se	Petra,	hvem	er det?
	see	*Petra*	*who*	*is it*
Petra:	Ja,	det	er **mig**.	
	yes	*it*	*is me*	

(A17) *OF in isolate CoDP*

Father:	Hvem	er	det	man	kan	se	på det	der	billede?
	who	*is*	*it*	*one*	*can*	*see*	*in that*	*there*	*picture*
Petra:	[**Mig** og	morfar].							
	me	*and*	*grandfather*						

(A18) *OF as post-copular nominal*

Father:	Hvem	er	det	der	spiser	is.
	who	*is that*	*there*		*eats*	*ice-cream*
Petra:	Det'	**os**.[36]				
	it [is]	*us*				

(A19) *OF in post-copular CoDP*

Father:	(Skal vi lige)	kigge	på det	der?
	shall we just	*look*	*at it*	*there*
Petra:	Det	er	[**mig** og	M.]
	it	*is*	*me*	*and*	*M.*

(A20) *OF as post-copular nominal*

Father:	Hvem	var	det!
	who	*was*	*it*
Petra:	Det	var	**mig**.
	it	*was*	*me*

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[36] See fn. 32 above.

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Sex and Syntax: Subjacency Revisited

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Despite the sustained effort of about forty years to analyze Subjacency, to date, there has been no principled account, with the most recent attempts faring not much better than the initial proposals. It is also significant that the seeming arbitrariness of Subjacency has been used to argue that syntax could not have evolved gradually: One does not see why evolution would target a grammar with Subjacency, when its contribution to grammar is not transparent, let alone its contribution to survival. As put in Lightfoot (1991), "Subjacency has many virtues, but […] it could not have increased the chances of having fruitful sex". This article turns the argument around, and proposes that subjecting syntax to a gradualist evolutionary approach can in fact shed light on the existence of Subjacency effects. It thereby offers a reconstruction of how communicative benefits may have been involved in shaping the formal design of language.

Keywords: adjunction; co-ordination; evolution; proto-syntax; subjacency

1.   What is Subjacency?

Move(ment) plays a crucial role in the mainstream theory of syntax, Minimalism (e.g., Chomsky 1995) and its predecessors alike. So, for example, *wh*-question formation is considered to involve movement of the *wh*-word or phrase from its thematic (underlying) position to the left periphery of the sentence (in English). The following examples illustrate:

(1)   What do penguins eat <what>?

(2)   What does Peter think [_CP_ penguins eat <what>]?

(3)   Who(m) did Peter walk with <who(m)>?

(4)   Who(m) did you say [_CP_ Peter walked with <who(m)>]?

In (1)–(2) it is assumed that the *wh*-word *what* originates after *eat* as a complement/theme/object of *eat* (cf. echo questions such as *Penguins eat what?*), and that it subsequently moves to the front of the sentence, to the position of the specifier of CP. (The '<>' notation is used here to represent the original (pre-Move) copy of the *wh*-word.) Similar considerations hold of the *wh*-word 'who(m)' in the examples (3)–(4). It is important to note here that *wh*-movement conceived in this way can sometimes apply long-distance, that is, it can cross clause (CP) boundaries, as is the case in (2) and (4).

    In his dissertation, Ross (1967) noted that there are many types of syntactic islands, that is, constructions out of which it is not possible to move.[1] One such island is coordination — as illustrated with the minimal pairs below, it is not possible to move a *wh*-word out of a conjunct:

(5)   What did Peter eat ham with <what>?

(6)   *What did Peter eat ham and <what>?

(7)   Who(m) did Peter see Richard with <who(m)> yesterday?

(8)   *Who(m) did Peter see Richard and <who(m)> yesterday?

Notice that the echo versions below are grammatical, suggesting that the problem lies with the movement itself, rather than with the semantics.

(9)   Peter ate ham and what?

(10)   Peter saw Richard and who(m)?

In addition, movement out of subjects (11) is less acceptable than movement out of objects (12), and subjects are for that reason also regarded as islands:

(11) ?*Who did [_NP_ your loyalty to <who>] appeal to Mary?

(12)   Who did Bill question [_NP_ your loyalty to <who>]?

The following examples introduce some additional islands: *Wh*-Island, where *wh*-extraction is prohibited out of another *wh*-clause (13); Complex NP Constraint, where Move is prohibited out of a noun phrase which includes a clause, either a nominal complement clause (14), or a relative clause (15); and Adjunct Island, where Move is prohibited out of an adjunct/adverbial (16):

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[1]   "We say that a phrase is an 'island' if it is immune to the application of rules that relate its parts to a position outside of the island" (Chomsky 1980: 194).

(13) ?*Which book did you ask John [$_{CP}$ where Bill bought <which book>]?

(14) ?*What did Bill reject [$_{NP}$ the accusation [$_{CP}$ that John stole <what>]]?

(15) ?*Which book did Bill visit [$_{NP}$ the store [$_{CP}$ that had <which book> in stock]]?

(16) ?*What did Peter retire [$_{CP}$ after Mary said <what>?]

One of the central questions of syntactic theory, if not the central question, has been what differentiates constructions that allow Move from those that do not. Typically, the assumption among syntacticians is that islandhood, that is, restrictions on Move, is the marked case, in need of explanation. This assumption has led to the expectation that there is some (abstract) principle of syntax, such as Subjacency, which accounts for all or most of the island effects. Syntacticians have thus concentrated on characterizing and defining the principles that are taken to constrain Move, including Subjacency.[2] About forty years after Ross' dissertation, no real progress has been made on this front, however: There is still no principled characterization of islandhood.[3]

Most accounts stipulate which syntactic nodes (S, NP, CP, DP, etc.), and/or which combination of nodes, and/or nodes in which syntactic positions, constitute obstacles to Move (barriers/bounding nodes/phases). The classic accounts are Huang (1982), Lasnik & Saito (1984), and Chomsky (1986). To take one example, very roughly speaking, one can account for the Complex NP constraint (14)–(15) by assuming that the NP is an obstacle to Move, to use neutral terminology. But the NP proves an obstacle only in conjunction with a clause, given that movement is otherwise possible either out of a clause as in (2) and (4) or out of an NP as in (12). Very roughly speaking again, one needs to assume that clauses and NPs are both obstacles, but that the *wh*-phrase can jump over one obstacle (at a time), even though not over two. So far, so good. But then

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[2]   Some more recent accounts (e.g., Boeckx 2008), adopt a pluralistic view of islandhood, that is, a view that islandhood is a result of the application of various principles, not just one unified principle such as Subjacency. Under this view, a unification of all islandhood is not pursued or expected (see fn. 7 for further discussion). In fact, Boeckx considers that the result of each Merge is an island, although typically not an absolute island. For him, island-hood results if too much checking affects a single item. If features to be checked can be distributed over more than one item, such as may be the case with movement leaving a resumptive pronoun, then islandhood is voided or weakened (p. 208). In other words, the islands are relativized to the amount of checking relations established and their configurations. Boeckx does acknowledge, however, that adjoined structures "have a freezing effect" on movement (p. 233), as well as that the islandhood of coordination is not captured by his, or any other syntactic theory (p. 237). Napoli (1993: 401, 409) likewise notes that "while Subjacency accounts for the Complex NP Constraint, […] the Subject Condition, and the *wh*-islands, it cannot account for the ungrammaticality of movement out of coordinate structures and out of adverbial clauses". The islandhood of coordination and adjunction is the central focus of my article.

[3]   This is not meant, in any way, to denigrate the quality of research done within this approach. For even when one follows an ill-fated hypotheses, one gathers invaluable data and insights along the way. But however fine and ingenious this research may have been otherwise, and however great its contributions, in my view, it has not yielded progress on this particular front, that is, it has not provided a principled account of islandhood.

this analysis does not really carry over to other islands. When it comes to the Subject island, how does one explain why movement out of the subject NP is illicit, while movement out of the object NP is licit? In both cases, the *wh*-phrases seem to be crossing the same number of obstacles. According to Huang (1982), this is because the subjects (and adjuncts) are not 'properly governed', while objects are. In Chomsky's (1986) version, this is because subjects (and adjuncts) are not L-marked, while objects are. The appeal to either proper government or L-marking only serves to render objects/complements privileged in this respect, implicating the importance of the structural position, in addition to the nature and number of nodes crossed. But there is now no real unification of the Complex NP island, on the one hand, and subject or adjunct islands, on the other. And the problems multiply as one considers additional islands (see, e.g., Postal 1997, 1998).[4]

Within the Minimalist framework, in which proper government and L-marking are not available as theoretical postulates, Chomsky (2001, 2008) attempts to capture some of the island effects by invoking new Minimalist constructs, phases (impenetrable domains), again stipulating that CPs and DPs (former NPs) are phases. Boeckx & Grohmann (2007) note that these most recent phase-based approaches to islandhood do not improve upon the previous approaches, and that "phases are in many ways reincarnations of bounding nodes and barriers" (p. 216). Belletti & Rizzi (2000) report an interview with Chomsky, in which he says that "there is no really principled account of many island conditions".

2.   Why There Is No Principled Account of Islandhood

The persistent view of Islandhood/Subjacency (in Minimalism and predecessors) considers Move to be a default option, while Subjacency (and other restrictions on Move) is treated as a marked option, in need of explanation (Ross 1967, Huang 1982, Lasnik & Saito 1984, Chomsky 1986, 2001, Stepanov 2007). To be more precise, Move in Minimalism is never completely free but is taken to apply only if motivated by a need to check certain (strong uninterpretable) features. But once such features are present in the derivation, it is considered that Move applies freely, in the sense that it applies unless blocked by some specific principle like Subjacency.

Significantly, this view fuels the influential language evolution hypothesis, according to which Merge (which subsumes Move) was the only evolutionary breakthrough for syntax: Once it emerged, it was able to apply freely and recursively (Hauser *et al.* 2002, Chomsky 2005, Fitch *et al.* 2005). In an attempt to reconcile this view with a gradualist approach to syntax, Newmeyer (1991) proposes that a grammar with Subjacency was specifically targeted by natural/

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[4]   Not only does one have to invoke the nature and number of obstacles, and the syntactic position in which they occur, but it is often necessary to characterize some obstacles as weak and some as strong, in order to explain variation in grammaticality (see e.g., the discussion in Stepanov 2007). See also fn. 2.

sexual selection, over a previous stage of grammar, which presumably had no Subjacency. This implies that this previous stage was characterized by a much freer Move, and that the ungrammatical examples discussed in Section 1 were grammatical in this stage. However, Lightfoot (1991: 69) counters that "Subjacency has many virtues, but […] it could not have increased the chances of having fruitful sex". In other words, it is not clear how or why a grammar with Subjacency would have been naturally/sexually selected over a grammar without Subjacency. Given these and similar considerations, Berwick (1998: 338–339) concludes that "there is no possibility of an 'intermediate' *syntax* between a non-combinatorial one and full natural language — one either has Merge in all its generative glory, or one has no combinatorial syntax at all" (see also Bickerton 1990, 1998, 2007). This kind of reasoning has led many syntacticians to believe that syntax is an all-or-nothing package, which could not have evolved gradually, and which must have been, in its entirety, a product of one single sudden event, possibly one single gigantic mutation.

But there is no need for this drastic conclusion. In fact, there is an alternative possibility to consider regarding Subjacency (mentioned in, e.g., Cinque 1978, Postal 1997, Boeckx & Grohmann 2007, Progovac 2009b, in press), that islandhood is the default state of syntax. Given this view, permitting Move would be a special/marked option. In fact, the constructions that prohibit Move are much more numerous and diverse than those that allow it (for a long inventory of additional island constructions, see, e.g., Postal 1997, 1998). Consider, again, the constructions which constitute islands to Move:

Subjects
(17) [?] Who did [NP your loyalty to <who>] appeal to Mary?

Wh-Clauses
(18) [?]*Which book did you ask John [CP where Bill bought <which book>]?

Complex NPs
(19) [?]*What did Bill reject [NP the accusation [CP that John stole <what>]]?

(20) [?]*Which book did Bill visit [NP the store [CP that had <which book> in stock]]?

Adjuncts
(21) [?]*What did Peter retire [CP after Mary said <what>?]

Conjuncts
(22) *What did Peter retire and [CP Mary said <what>?]

Basically, Move is possible only out of (a subset of) complements/objects, for example, verbal (non-*wh-*)complements, whether clausal (23) or nominal (24):

(23) Which book did you tell John [CP that Bill bought <which book>]?

(24) Who did Bill question [NP your loyalty to <who>]?

What this means is that constructions which disallow Move (islands) do not form a natural class, while those that allow Move, do. If so, then any attempt to characterize islandhood/Subjacency in unified terms is doomed to fail. On the other hand, it should be possible to formulate a general characterization of non-island constituents, as suggested in Postal (1997). [5]

Furthermore, there are additional cases where Move is illicit, and I list them here to anticipate the discussion in sections 3 and 4. For example, Move does not occur across sentential boundaries, as is well-known, but not discussed in the context of Subjacency:

(25) *Who did Mary see the movie. It featured <who>?

The idea is that the principles of syntax do not extend across sentence boundaries, but it is worth noting here that some sentence-internal boundaries resemble the sentential boundaries in this respect.

Move is also prohibited from paratactically (loosely) attached (small) clauses (26), and from attached bare small clauses (27), the latter example subsumable under Adjunct Islandhood:[6]

(26) *What nothing ventured, <what> gained?
      (cf. Nothing ventured, nothing gained.)

(27) *Where can her having retired from <where>, we finally relax?
      (cf. Her having retired from MIT, we can finally relax.)

Finally, Move is also prohibited from the so-called Root Small Clauses, that is, small clauses used in root contexts, to be discussed further in sections 3 and 4.

(28)   *Where her retire from <where>? / *Who(m) retire from MIT?!
       (cf. Her retire from MIT?!)

With these additional examples, it becomes even clearer that constructions that prohibit Move (islands) have no syntactic property in common. It is thus not surprising that in spite of all the effort, to date, there has been no principled analysis of islandhood/Subjacency, as pointed out in section 1 (see also Belletti & Rizzi 2000, Szabolcsi & den Dikken 2003, Boeckx & Grohmann 2007).

For all these reasons, it would be prudent to explore an alternative track, an approach that takes islandhood to be the default state of syntax, and Move a special option, available only in certain privileged constructions.[7] In this view,

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[5] According to Postal (1997), *every* English constituent is an island and it is especially difficult to provide an account for the Coordinate Structure Constraint, as also noted by Boeckx (see fn. 2). Boeckx (2008: 250) embraces Postal's idea that domains are islands by default.

[6] A 'bare' small clause can be characterized as an embedded small clause whose subject does not check structural case (see section 4.1. for various types of small clauses with respect to case properties).

[7] As mentioned in fn. 2, a reviewer points out that another angle is possible, namely, to adopt a pluralistic view in which islandhood is a result of several independent principles that constrain Move (see, e.g., Boeckx 2008). In addition to not being able to capture the

the question is no longer why Move is impossible out of islands, but rather why Move is possible out of certain complements, and indeed why Move is possible at all. But, first, one needs to wonder why No Move would be the default state of syntax. The next section attempts an answer.

3.   An Evolutionary Explanation[8]

I propose that proto-syntax, based on small clauses and one-word utterances, did not have Move or subordination (Progovac 2007, 2008, in press).[9] Initial clausal combinations may have looked like paratactic, impenetrable constructions such as the following concatenations of two small clauses:[10]

(29) a.   Nothing ventured, nothing gained.
      b.   Easy come, easy go.
      c.   Monkey see, monkey do.
      d.   Card laid, card played.

Recall from section 2 that such concatenations do not sanction Move (26). The following are some additional examples that illustrate the same point:

(30) a.   * How easy come, <how> go?
      b.   * Who monkey see, <who> do?

In this view, the kind of syntax illustrated in (29)–(30) was primary, while Move was an evolutionary innovation. While, in agreement with Newmeyer (1991), this proposal advocates a gradualist approach to the evolution of syntax, notice that it is the direct opposite of what Newmeyer proposed, which is that the previous stage(s) of grammar had no restrictions on Move, and that Subjacency was an in-

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    islandhood of coordination and adjunction, the central topic of this article, this view, as noted by the reviewer himself, is also not able to account for the generalization that non-islands seem to form a natural class. Even though the correlation is not perfect, it still holds that if a constituent is *not* a complement, then it is highly likely to be an island. There are many subtleties regarding islandhood, including distinguishing weak from strong islands, that my approach does not address, as rightly pointed out by the reviewer. I hope that future research will address this question within an evolutionary framework. In the meantime, I would like to submit that an evolutionary approach which assumes gradual development of syntax is well-equipped to deal with graded stages. In this respect, one would need to consider the three stages explored in this article, Adjunction, Coordination, and Subordination (see also section 4), as just three idealized points in the evolution of language, with transitional stages certainly a possibility.

[8]   As pointed out by Cedric Boeckx, syntactic theories of Subjacency, and locality in general, should be compatible with findings in neuroscience and evolutionary biology: "Up to now, compatibility with neuroscience and evolutionary biology has been a rather weak constraint on theory construction in linguistics" (Boeckx 2008: 4).

[9]   I use the term 'subordination' here in a rather narrow sense, to refer to the embedding of one clause within another, where the embedded clause serves as the complement of the main verb.

[10] A reviewer points out that analyzing examples in (29) as simple concatenations may be problematic. I return to this issue below.

novation (section 1).[11] In my proposal, islandhood, or lack of Move, characterized the previous stage(s) of grammar, while Move was introduced later, probably in conjunction with more complex, layered, hierarchical syntax, as well as in conjunction with specialized functional categories and projections, such as TP and CP (section 4).

This scenario meshes quite well with Kiparsky's (1995) account of Indo-European proto-language clause structure (see also Hale 1987).[12] According to Kiparsky (1995: 155), a major characteristic of Indo-European syntax, best preserved in Sanskrit, Hittite, and Old Latin, was that finite 'subordinate' clauses were not embedded but adjoined (Watkins 1976, Hock 1989). According to Kiparsky, Indo-European proto-language lacked the category of complementizer and had no CP or any syntactically embedded sentences. What looked like finite subordinate clauses, including relative clauses and sentential complements, were syntactically adjoined to the main clause, still exhibiting main clause properties, such as topicalization of constituents to clause-initial position. Kiparsky (1995: 145) calls these adjoined finite clauses 'embedded root clauses', for they exhibit properties of main clauses, and yet seem to be interpreted as embedded. This is exactly the transitional scenario toward developing subordination that I am proposing worked for language evolution. Kiparsky further claims that the introduction of complementizers coincided with the shift from adjunction to embedded subordination, which is in line with Kayne's (1982) assumption that only CPs can function as sentential arguments (see also Holmberg 1986 and Taraldsen 1986).

A reviewer points out that my analysis of (29) as simple concatenation/ parataxis (see also section 4) may be problematic, given some recent analyses of correlative constructions of the type illustrated in (31) below:

(31)   The more you read, the less you understand.

Culicover & Jackendoff (2005: 508) argue that such correlative constructions involve a paratactic (quasi-coordinate) syntax with conditional semantics. However, den Dikken (2005: 503) counters that their approach "condone(s) a mismatch between syntax and semantics" and proposes a syntactically more complex derivation. The conditional semantics, however, does not follow even from den Dikken's treatment of correlatives, as he himself acknowledges. But, at any rate, this same friction between syntax and semantics seems to carry over to my examples in (29).

First of all, I would like to suggest that, at least in the case of examples such as (29), one is not dealing with a mismatch, but rather with underspecification/ vagueness. The paratactic attachment only signals that there is a relationship between the two clauses, but it does not specify what that relationship is exactly. According to Culicover & Jackendoff (2005: 528), parataxis is "grammatically the most primitive way to combine linguistic elements, one that leaves the semantic

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[11]   A similar idea can be found in, for example, Boeckx's (2008: 244) statement that bounding nodes are solutions that the language faculty has developed to ensure that syntactic objects are unambiguous.

[12]   Thanks to an anonymous reviewer for pointing me to Kiparsky's paper.

relations among the elements to be determined by their inherent semantic possibilities or by pragmatic considerations". As further discussed in section 4, concatenations such as (29) typically rely on iconicity of word order to express temporal and/or causal relations, rather than on any syntactic devices (see also Stump 1985: 307, Deutscher 2000).

Furthermore, the correlative structures in (31) are clearly more complex than the paratactic attachment of small clauses in (29), both clause-internally and clause-externally. Internally, both clauses in (31) are finite, showing tense and agreement, as well as a left-peripheral position before the subject, implicating Move, or at least a(n additional) functional projection above TP. In contrast, the small clauses in (29) are just that — small clauses which show no tense, no agreement, and no Move. Externally, each of the small clauses in (29) can be a root construct on its own, not requiring another clause to complete it (e.g., *Nothing ventured!*). This is in contrast to correlative constructions in (31), whose individual clauses are clearly dependent (*The more you read*), possibly suggesting some additional external mechanism of clause cohesion, not available in (29). In addition, as pointed out by a reviewer, external ATB movement out of correlatives such as (31) is possible:

(32)   This is a book that the more you read, the less you understand.

This is not to deny the obvious similarities between the constructions in (29) and the correlatives in (31). The correlatives in (31) may represent modern complications of ancient correlatives, the latter more closely approximated by the examples in (29).

In his detailed consideration of absolute constructions, such as the underlined string in (33) below, which also seem to involve parataxis, Stump (1985: 302) concludes that the logical relation between an absolute and its superordinate clause is often determined inferentially. He defines 'inference' as "anything which is not part of the literal meaning of some expression but which language users judge to be part of the intended meaning of that expression" (304).

(33)   She clapped her hands like a child, <u>her lucid eyes sparkling</u>.

(Stump 1985: 332)

The issue of vagueness and underspecification deserves special attention in an evolutionary framework. If language developed gradually, then it is to be expected that not all the grammatical tools that we use today to express logical relations with some precision were available in the previous stages of grammar. This should not have prevented our ancestors from speaking in however imprecise and underspecified ways. It is also important to keep in mind that, however precise we may believe that our language is today, it is still vastly underspecified with respect to so many distinctions that could in principle be made. The ever increasing precision in what we can express with language, and the increasing match between syntax and semantics, may have marked *one* of the directions in which language evolved. But there is no reason to believe that a

perfect syntax–semantics match has been achieved, or that it is even desirable to achieve.

Going back to islands, we can now envision an answer to the question of why some constructions still disallow Move (e.g., coordination or adjunction), while others facilitate it (subordination). My claim is that our grammars, courtesy of gradual evolutionary development, show a range of constructions that fall between the two opposites, (i) two completely separate utterances/sentences and (ii) syntactically fully integrated expressions. The intermediate possibility is to be loosely attached (semi-integrated) into sentential fabric, and this is arguably the case with, for example, clausal adjuncts and conjuncts, on which I focus in this article (see also the concatenation of small clauses discussed above (29)).[13] Only the most integrated of constructions (subordination) allow Move across clause boundaries.[14]

Clausal conjuncts and adjuncts have been repeatedly noted not to be fully integrated into syntactic fabric.[15] First, they are often parsed as separate intonation phrases (Selkirk 1978, Stowell 1981, Nespor & Vogel 1986, Zec & Inkelas 1990), which is consistent with them sitting in semi-integrated, 'non-canonical', syntactic positions, as put in An (2007). Next, adjuncts have been analyzed in syntax as merging in a different plane (e.g. Chomsky 2001; see also Chomsky 2004), and conjuncts as sitting on parallel planes (Goodall 1987). According to Lebeaux (1988), adjuncts can be merged into structure acyclically, that is, independently of the main cycle of the derivation. According to Stepanov (2001), adjuncts are necessarily Merged post-cyclically, that is, after the cyclic portion of the derivation is complete. According to Adger (2003), adjuncts do not even involve Merge, but rather an operation distinct from Merge, which can be called Adjoin. Chomsky (2004: 117) acknowledges that "there has never […] been a satisfactory theory of adjunction". It is very clear that adjunction and coordination are not fully integrated into the fabric of syntax.[16]

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[13] Even though I will not discuss subject islands in this article, it is worth noting that syntactic theory recognizes that subjects/specifiers are more loosely integrated than objects/complements in various ways. While their objects/complements are merged directly with the verbs (First Merge), subjects/specifiers are typically introduced as sisters to intermediate projections (Second Merge). In addition, subjects typically undergo Move out of verbal projections, further contributing to their syntactic instability.

[14] This is not to say that subordination was necessarily one big solid monolithic stage — as pointed out repeatedly in this article, sub-stages and transitions may well have existed, and may account for a number of present-day constructions which are ambivalent in this respect (see, e.g., section 4).

[15] Note also that both adjuncts (i) and conjuncts (ii) can be instantiated by bare small clauses (see also fn. 6, as well as the discussion above regarding absolute constructions). Such bare small clauses exhibit subjects without structural case, bearing resemblance to concatenated small clauses in (29):

  (i)   He reverted to his old ways, us having left.   (Jackendoff 2002)

  (ii)   I am not going to have any woman rummaging about my house, and me in bed.
                                                                                  (Jespersen 1954)

[16] Note also that c-command, the central postulate of syntax, does not seem to extend into conjuncts or adjuncts in all cases (see Progovac 2003 for some discussion). With negative polarity licensing, it is possible to license the negative polarity item *ever* in an embedded subordinate clause (i), but not in a conjunct clause (ii) or an adjunct clause (iii):

Once again, the question is why human grammars should avail themselves of this range of possibilities for clause combination, and moreover such 'imperfect' possibilities, as are coordination and adjunction. According to, for example, Traugott & Heine (1991) and Deutscher (2000), grammaticalization of subordination (36) typically proceeds through exactly these three stages, including parataxis (adjunction) (34) and coordination (35), from least syntactically integrated to most integrated:[17]

(i)	Mary did not say [that she ever met Peter].	*Subordination*
(ii)	* Mary did not say it, [but she ever met Peter]. (cf. Mary did not say it but she never met Peter.)	*Coordination*
(iii)	* Mary did not say it, [after she ever met Peter].	*Adjunction*

In a similar fashion, Principle C effects, clearly visible with subordination (iv), do not seem to extend into conjuncts (v): While *she* and *Mary* cannot co-refer in (iv), co-reference is possible in (v). The judgment is less clear with an adjoined clause in (vi). To me, as well as a native speaker I consulted, it seems that (vi) is slightly better than (iv), but the reviewer, as well as an informant of his, ranks it equally ungrammatical as (iv).

(iv)	* She_i never mentioned [that $Mary_i$ is a bartender].	*Subordination*
(v)	She_i never mentioned it, [but $Mary_i$ is a bartender].	*Coordination*
(vi)	?* She_i never mentioned it, [after $Mary_i$ became a bartender].	*Adjunction*

Given that both negative polarity and Principle C extend across clause boundaries, I use clausal coordination and adjunction here to illustrate c-command effects, just as I use clausal subordination. These should thus constitute good minimal pairs.

However, there are processes dependent on c-command which nonetheless seem to extend into conjuncts, as pointed out by a reviewer: ATB extraction (vii), Principle C into a coordinated embedded clause (viii), and bound anaphora (ix):

(vii)   Which book does Peter like and Mary hate?

(viii) She_i said that John saw $Mary_{*i}$ and Bill saw Sue_{*i}.

(ix)   Every boy_i said that he_i is going to play football and there's nothing you can do to stop him_i.

To complicate matters further, some Principle C effects seem to overlap with the effects of the pragmatic precedence principle, which operates across independent sentences (x), and can thus not be reduced to c-command (see Progovac 2003 for some discussion):

(x)   ??? He_i finally arrived. $John_i$'s cousin accompanied him.

Given this, one is not clear if it is syntactic c-command or pragmatic precedence that excludes co-reference in either (viii) or (iv) and (vi). Clearly, this issue deserves further investigation. It may be that the (un)grammaticality of these various examples is due to a curious interplay of more than one factor, including syntactic c-command and pragmatic precedence, whose domains seem to partly overlap. Could it be that an ancient, pragmatic principle of precedence got grammaticalized into c-command, which can realize its full potential only in the subordination stage?

[17]   The following example may also be seen as involving parataxis, but in a clause-internal position:

(i)   He, as you know, is a linguist.

(34)   He is a linguist — (as) you know.   *Parataxis*

(35)   He is a linguist, and you know it.   *Coordination*

(36)   You know that he is a linguist.   *Subordination*

If comparable stages characterized language evolution, with adjunction and coordination constituting intermediate steps between separate utterances (no syntactic integration, no Move) and subordination (full integration, free(er) Move), then such evolutionary 'tinkering' left us with multiple possibilities which partly overlap in function, that is, with redundant means for expressing similar meanings (34)–(36).[18,19] In the spirit of Charles Darwin (e.g., Darwin 1964), and as elaborated in Jacob (1977), evolution is taken to be a 'tinkerer', rather than an engineer. Unlike engineering, which designs from scratch, with foresight and plan, and with perfection, tinkering involves cobbling together, out of bits and pieces that happen to be available, clumsily, with no long-term foresight. Evolution is also taken not to throw a good thing away, but to build upon it, or to add to it. So, if adjunction and conjunction proved to be useful syntactic mechanisms in a proto-syntactic stage (see also section 4), the later stages did not have to discard them, but could continue to use them in specialized functions. This is also the case with grammaticalization of subordination. As put in Carroll (2005: 170f.), "multifunctionality and redundancy create the opportunity for the evolution of specialization through the division of labor". Overlap and (partial) specialization are properties of evolutionary tinkering, rather than of optimal design.

Still, one would like to know what might have been the advantages of each stage, which assured its survival? I propose in section 4 that the conjunction stage has a clear advantage over the adjunction stage in that it provides more robust evidence for Merge, including segmental. But what about subordination — does it provide any concrete advantage over either conjunction or adjunction, and moreover an advantage that could have been targeted by natural or sexual selection?

As it turns out, in addition to facilitating Move, subordination also provides a recursive mechanism for embedding multiple viewpoints one within another, unavailable with either coordination or adjunction, privileging in this

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[18]   This is not meant to imply that there were exactly three syntactic steps in the evolution of language, or that the subordination stage was a single solid stage. Finer sub-stages are very likely to have existed, and even modern languages show constructions that are transitional in nature, as pointed out throughout this article (see especially section 4). For the purposes of this article, it is sufficient to identify these three rough stages.

[19]   Notice that my claim here is *not* that subordination automatically licenses Move. I am only saying that subordination is a necessary condition for Move, not sufficient. Other conditions clearly need to be met to allow Move, including the existence of the landing site for Move (e.g., CP for *wh*-movement). Given this, the fact that not all subordinate constructions allow Move, but only a subset of them do, is not directly a problem for my analysis, even though it raises the question why. The analysis proposed here posits a different question than the traditional analyses: The question here is not what non-complements and complement islands have in common, the question pursued by Subjacency accounts, but rather how complement islands differ from complement non-islands. Exploring this question further may give new insights into the nature of Move.

respect (39) over (37)–(38):

(37)   [As you know,] [as Mary knows,] he is a linguist.

(38)   He is a linguist, [and you know it,] [and Mary knows it].

(39)   You know [that Mary knows [that he is a linguist]].

Only in (39) is it possible to report on one person's knowledge about another person's knowledge. Thus, if subordination (as well as Move) is an innovation resulting from evolutionary tinkering, then subordination would have significantly increased the expressive power of language, in a concrete and tangible manner, and thus, unlike Subjacency, constitutes a plausible target for natural/sexual selection.

     In this evolutionary perspective, rather than a system designed from scratch in an optimal way, syntax is seen as a patchwork of structures incorporating various stages of its evolution, giving an impression, or an illusion, of Subjacency. It follows that Subjacency is not a principle of syntax, or a principle of any kind, but rather just an epiphenomenon. Subjacency or islandhood can be seen as the default, primary state of language, due to an evolutionary base of language which was without Move. This default state can be overridden in certain evolutionarily novel constructions, such as subordination.

     While this article does not answer the question of how exactly complex syntax evolved, or how exactly humans proceeded from an adjunction or conjunction stage to the subordination stage (see section 4 for the characterization of these stages), it at least provides a framework in which these questions can be asked, and eventually answered. It also identifies concrete communicative advantages that subordination has over its more primary counterparts: The increase in the expressive power of language, making possible, with syntactic means, the recursive embedding of multiple viewpoints one within another. The next section characterizes in more detail the three postulated stages, as well as possible advantages of the coordination stage over the adjunction stage.

     As kindly put by a reviewer, this article is not only about looking for evolutionary fossils and postulating possible paths of language evolution, but this particular evolutionary scenario offers a reconstruction of how communicative/functional benefits may have been involved in the shaping of the formal design of language itself (see also Progovac 2008, 2009a).

4.   Excursus: Hypothesized Evolutionary Stages of Syntax

Based on some present-day constructions, as well as based on the trends in grammaticalization processes, one can reconstruct the following three rough stages in the evolution of syntax, each of which could have, of course, proceeded through sub-stages:

(A)   Parataxis/Adjunction stage, with no hierarchical structure, where prosody/suprasegmentals provide the only glue for merger (Jackendoff 1999, 2002).

(B)   Proto-coordination stage, where, in addition to prosody, the conjunction provides all-purpose *segmental* glue to hold the utterance together.

(C)   Specific functional category stage, where, in addition to prosody, specific functional categories provide specialized syntactic glue for clause cohesion, including tense elements and subordinators/complementizers. It is in this stage that Move seems to become available.

The following subsections explore each of these postulated stages.

4.1.   *Parataxis/Adjunction Stage*

This stage can be characterized by flat concatenation, where the merger of two constituents (e.g. two words or two clauses) to form a single constituent does not build hierarchical structure, and where it is only intonation and prosody (suprasegmentals) that hold the constituents together. Some version of this proposal can be found in, for example, Dwyer (1986), Bickerton (1990), Jackendoff (1999, 2002), Burling (2005), Deutscher (2005), Progovac (2007, 2009b).[20] According to Jackendoff (1999, 2002), adjunction has proto-linguistic flavor, and it can be seen as an evolutionary fossil. Adjunction in present-day languages is typically taken to involve flat/non-hierarchical structure (cf. Merge vs. Adjoin of Adger 2003, as discussed in section 3).

   Furthermore, Progovac (2007, 2008, 2009a) argues that this adjunction/parataxis stage not only can be found fossilized in several constructions used today (such as *Nothing ventured, nothing gained*, as introduced in section 3, or root small clauses discussed below), but that such paratactic constructions provide a foundation upon which hierarchical syntax is built.

   Consider the following instances of Root Small Clauses (RootSCs), that is, small clauses used in root contexts:

(40)   Me first! Everybody out! Him apologize?! Me worry? Case closed. Problem solved. Point taken.

While RootSCs of (40) are hardly ever a subject of syntactic inquiry, having been relegated to the 'periphery', similar (but not identical) small clauses (SCs), which

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[20]   Note in this respect that Jordens (2002) argues that there is a stage in the acquisition of Dutch where all constituents are attached by adjunction, and where certain modal verbs and negation serve as proto-functional categories. According to Jordens, the stage lacks evidence for functional categories, the properties of finiteness are absent, and the ordering in this stage is driven by pragmatic/conceptual factors. Thanks to an anonymous reviewer for pointing me to this reference. See section 5 for more discussion regarding language acquisition.

occur in embedded contexts, have been recognized and studied in syntax.[21]

(41)   He wants [$_{SC}$ everybody out]. He imagined [$_{SC}$ the problem solved].

There are competing analyses of the bracketed SCs in (41), including some that ascribe quite complex structure to them (see, e.g., Cardinaletti & Guasti 1995). However, the tendency is still, overwhelmingly, to label them 'SCs', suggesting hesitance to commit to an analysis that renders them projections of their predicate, or of something else. In fact, they may be paratactic creations, in which subject and predicate are loosely concatenated/adjoined. Uriagereka (2008) looks at the embedded SCs such as the ones in (41), and concludes that their structure is rather basic, and may involve finite-state syntax, the simplest type of syntax in Chomsky's hierarchy. One of the arguments Uriagereka invokes for the primitive nature of (embedded) small clauses is the long-noted observation that these clauses do not have an internal source of structural case, and are thus assigned case by an external element, the verbs *want* or *imagine* in (41).

According to Progovac (2006), RootSCs in (40) do not have a structural mechanism for assigning case to their subjects, providing another argument that they are creations similar to embedded SCs. Since with RootSCs there is no external source of case either (they are not embedded under a verb), their subjects surface with what can be analyzed as default case (in the sense of, e.g., Schütze 2001) — witness the accusative on the pronominal subjects in (40). The evolutionary perspective sheds light on the existence of both embedded SCs and RootSCs — both can be seen as 'living fossils' of a proto-syntactic stage in which, presumably, clauses were put together by a process akin to adjunction.[22]

What is of interest for the considerations of this article is the fact that these RootSCs cannot be manipulated by Move, as already pointed out in section 2.[23]

(42)   a.   * Who(m) first?
       b.   * Where everybody?
       c.   * To whom him apologize?
       d.   * What solved?

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[21]   See Progovac (2006) for surface, structural, and semantic differences between embedded ECM small clauses and RootSCs. Structurally, RootSCs are akin to 'bare' small clauses in that neither has any source of structural case for their subject position (fn. 6; see also section 3). As Progovac (2006) argues, constructs such as (40) are true RootSCs, rather than TPs/IPs which have undergone selective ellipsis/deletion. The arguments against analyzing such clauses as TPs/IPs include the (default) accusative case on the subject, the lack of agreement/tense marking, the possible lack of articles even with singular count nouns (e.g., *Case closed*), and the marked interpretation possibilities (such as irrealis, formulaic).

[22]   In biological literature, 'living fossils' are defined as species that have changed little from their fossil ancestors in the distant past, such as, for example, lungfish (Ridley 1993). Bickerton (1990) and Jackendoff (1999, 2002) introduced the idea of language fossils. In syntax, one can define living fossils as constructions which exhibit rudimentary syntax/semantics, not accounted for by the principles of modern (morpho)syntax, but which nonetheless show some continuity with it. One postulated syntactic fossil would be RootSCs, as discussed above.

[23]   A reviewer points out that (i) is acceptable to him/her, but that it may be elliptical:

       (i)   O.K. — who first?

If these are indeed proto-syntactic fossils, then this is consistent with my claim that proto-syntax did not have Move. It is also consistent with the more specific claim that Move is unavailable in the paratactic stage of grammar, which these RootSCs arguably instantiate.

Even though they do not permit Move, these small clauses can concatenate, where intonation, rather than any functional category, provides the glue holding the two clauses together. These kinds of concatenations occur cross-linguistically, and are typically preserved in formulaic, proverb-like sayings:[24]

(43) a.   Nothing ventured, nothing gained.
      b.   Easy come, easy go.
      c.   Monkey see, monkey do.
      d.   Card laid, card played.

(44) a.   Na psu rana,   na psu i   zarasla.   *Serbian*
            on dog wound on dog   and healed
            'No big deal!'

      b.   Preko preče,   naokolo bliže.
            across shorter   around   closer
            'Shortcuts are not always best.'

What I am proposing here is that both clausal combinations such as (43)–(44), and predicate–argument combinations in (40), are created by the same type of grammar — paratactic grammar. This grammar is exocentric, lacks functional categories (e.g., TP, CP), and lacks Move. Also, as pointed out in the following sub-section, the same proto-coordinator can sometimes be used both clause internally and externally, suggesting that the two followed a similar evolutionary path. That is why I believe that the internal structure of these clauses is (indirectly) relevant for understanding the structure of clause combination: Arguably, both are products of the same exocentric, paratactic grammar.

To appreciate the role of prosody/intonation, consider (45) as a report from a business trip, with falling intonation rendering these two clauses as two separate utterances.

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[24]   Comparable concatenations are more productive in pidgin languages, such as in *No money, no come* (e.g., Winford 2006). Bickerton (1990) in fact considers that pidgin languages are indicative of our ability to tap into the proto-linguistic stage. However, in his view, pidgin languages (or child language) have no syntax, and in fact do not count as real language. In my view, the proto-syntactic stages clearly show continuity with the more innovative stages of syntax. Not only that — my argument is that proto-syntax provides a foundation, a necessary stepping stone into more complex, hierarchical syntax (see especially Progovac 2008, 2009a). Notice in this respect that Culicover & Jackendoff (2005) argue that there is continuity between what are typically considered to be 'core' syntactic phenomena and the 'periphery', quirky-looking syntactic constructions, which include at least some of the root small clauses in (40). A reviewer wonders if non-hierarchical structures can be considered as syntax. My view on this is that parataxis is an important aspect/layer of syntax, upon which hierarchical syntax rests. For example, the wide-spread view regarding sentence building is that a sentence starts to unfold from a small clause, which essentially can be analyzed as a paratactic/exocentric structure (see discussion in the text above). This initial paratactic structure gets integrated into layered syntax by various syntactic processes, including Move.

(45)   Nothing ventured. Nothing gained.

The interpretation in (45) is that nothing was ventured, and that nothing was gained. Cross-linguistically, falling intonation implies assertion/certainty/completion, while rising intonation signals uncertainty/incompleteness (e.g., Burling 2005: 170). In contrast, (43) combines the two clauses into a single utterance, using rising intonation as glue. In addition to intonation, concatenations such as (43) and (44) typically rely on iconicity of word order to express temporal and/or causal relations, as mentioned in section 3. Deutscher (2000) argues that the development of finite subordination (CP complementation) had an adaptive advantage of breaking away from such iconicity. Prosody and intonation are still used cross-linguistically to signal grammatical functions, such as interrogative mood in (46). When they are used in conjunction with syntactic processes, such as Subject–Auxiliary Inversion in (47), the result is substantial redundancy and robustness, hallmarks of evolutionary tinkering.

(46)   Mary is already at home?

(47)   Is Mary already at home?

Intonation and prosody, which are modulated analogically, rather than discretely, have been proposed by many to have been available before syntax, given that they have significant analogs in other species (e.g., Deacon 1997, Piattelli–Palmarini & Uriagereka 2004, and Burling 2005). Also, intonation and prosody may remain intact even in cases of severe lexico-syntactic deficits (confabulatory paraphasia and jargon aphasia: Wernicke 1874, Broca 1878, Joanette *et al.* 1990, Brain & Bannister 1992, Pulvermüller 2002). According to Deacon (1997), speech prosody is essentially a mode of communication that provides a parallel channel to speech; it is recruited from ancestral call functions. Like these systems, prosodic features are primarily produced by the larynx and lungs, and not articulated by the mouth and tongue. But unlike calls of other species, prosodic vocal modification is continuous and highly correlated with the speech process (Deacon 1997: 418); the human larynx must be controlled from higher brain systems involved in skeletal muscle control, not just visceral control (243). It is as though we have not so much shifted control from visceral to voluntary means but superimposed intentional cortical motor behaviors over autonomous subcortical vocal behaviors.

Of note here is also that many RootSC types, in particular the incredulity RootSCs such as *Me worry?!* (see (40)) are characterized by exaggerated intonation, possibly compensating for the lack of functional categories, and tapping into the proto-linguistic ability to create clauses using prosody/intonation as the only glue.

In conclusion, postulating a paratactic stage in the evolution of syntax is consistent with, and supported by, the 'living fossils' of this stage found in modern languages (RootSCs and their paratactic combinations, as discussed above), as well as by the neurological and comparative studies of intonation and prosody. Section 5 provides some further corroborating evidence. As illustrated

above, neither RootSCs nor their combinations can be manipulated by Move. This is consistent with my proposal that constructions that do not allow Move are evolutionarily primary, and those that allow Move evolutionary innovations.

4.2. *Proto-Coordination Stage*

As pointed out in the previous section, paratactic combinations rely solely on *supra-segmental* information to provide evidence of merger. Following this argument, one can see conjunctions as segments providing all-purpose *segmental* glue to hold an utterance together. This would be a stage in which a functional category emerged, a proto-coordinator, whose sole purpose was to hold the utterance together by more than just prosodic means, consolidating Merge. The proto-coordination stage most probably built upon the paratactic stage by adding a segment (conjunction) to the already existing intonation, providing now two indicators of Merge, one segmental and one supra-segmental.[25] It would have been only later that such proto-coordinators differentiated into specific functional categories, such as aspect markers, tense markers, or complementizers (section 4.3). Needless to say, the proposal in this section is speculative in nature, and the data presented below are merely suggestive of this possibility.

A reviewer points out that there may have been other advantages to the emergence of (proto-)conjunctions, such as the ability to now use different types of conjunctions, not just the neutral connective 'and'. As pointed out in Payne (1985: 9) and references cited there, in languages such as Vietnamese and Japanese, a coordinator is used for the adversative conjunction (e.g., 'but'), even though in non-adversatives the strategy involves simple juxtaposition of the conjuncts with no intervening conjunction.[26] This state of affairs also points to the continuity/fluidity between adjunction and conjunction. According to Payne, the paratactic strategy, where the conjuncts are simply juxtaposed, with no additional markers of conjunction, is probably available to all languages. In many

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[25] My initial formulation was that segmental glue (conjunction) provides a more robust cohesive mechanism than intonation. A reviewer takes issue with this and suggests that intonation can be considered as a morpheme, and a conjunction is also just a morpheme. Even if the reviewer's suggestion is correct, the point remains that the proto-coordination stage utilizes two mechanisms for identifying Merge, intonation and conjunction, and two mechanisms will necessarily yield more robust evidence for Merge than just one of them alone. Present-day *yes–no* questions tend to keep the rising intonation even in the presence of segmental/syntactic evidence for interrogative mood (see examples (46)–(47) in the text.) But I also doubt that intonation/prosody in paratactic constructions (e.g., (43)), or in yes-no questions such as (46) in the text, can be considered as morphemes. For one thing, this kind of paratactic/interrogative intonation is not language specific, but seems to occur as a device across unrelated languages; if it is a morpheme, it would be some kind of universal morpheme. Second, if prosody signaling e.g. paratactic attachment were a morpheme, then this morpheme would have a rather unspecified meaning, given the range of interpretation possibilities it can have (see section 3). Moreover, prosody/intonation typically stretches over the whole utterance, which would also not be typical of a morpheme. But, clearly, intonation/prosody of the kind used in parataxis/question formation shows some continuity with modern cases of suprasegmental morphemes, such as tone in tone languages.

[26] One also finds combination of both the neutral conjunction ('and') and an adversative conjunction (e.g., English *and yet* and Standard Arabic *wa lakin* 'and but', as noted in Payne (1985: 15), suggesting that the neutral coordinator can serve as a mere connector, without a specified meaning (see section 3 for similar conclusions regarding parataxis).

languages, such as for example, Turkish, it is a normal alternative, existing side by side with other strategies. The classical languages, including Sanskrit and Latin (cf. *Veni, vidi, vici* 'I came, I saw, I conquered') also widely permit the juxta-position strategy for coordination (Payne 1985: 25).

Notice also that several recent accounts of coordination invoke adjunction as an integral part of the analysis. For example, Munn (1993) proposes that the second conjunct right adjoins (with its &P) to the first conjunct, while Kayne (1994) argues that the first conjunct left adjoins to the &P containing the second conjunct. Coordination as adjunction has also been explored by, for example, Schwartz (1989a, 1989b) for comitative/asymmetric conjunct (second conjunct in asymmetric coordination).[27] In several other respects as well, the conjunction is a head unlike other functional heads, falling somewhere between adjunction and subordination (see Progovac 2003 for an overview of various analyses that reduce coordination to adjunction, or vice versa). Considerations like this give credence to the gradualist evolutionary approach, for they provide evidence of continuity and overlap between stages.

Relevant to this discussion is also the existence of the so-called 'fillers' in language acquisition. Some children acquiring various languages use such fillers in their first multi-word utterances, typically in places where one would expect functional categories (e.g., auxiliaries or determiners). These fillers are reported to be closely tied to prosody, particularly rhythm and melody, although there is no unified approach to describing fillers (see Braine 1963, 1976, Bloom 1970, Dressler & Karpf 1995, Peters 1999, and Veneziano & Sinclair 2000). Initially, such fillers may be undifferentiated in form and occur in various positions, but later they become more specialized for the position (from Peters & Menn 1993):

(48)   [m] pick [ə] flowers   (*English*, age 1;6)

Very tentatively — these fillers might correspond to proto-conjunctions/proto-functional categories (see section 5 for the relationship between EVO (evolution in species) and DEVO (development in children)).

Predication may have also gone through a proto-coordination stage. German incredulity RootSCs take an optional conjunction (Potts & Roeper 2006, Progovac 2006, 2009b):

(49)   Ich   (und) Angst   haben?   *German*
       I   (and) fear   have.INF
       'Me afraid?!'

Akkadian, a Semitic language spoken between c. 2,500 to 500 BC, used the co-ordinative particle –*ma* in predicative functions (50) (Deutscher 2000: 33f.). The

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27   One example of such a transitional comitative construction is the following Russian sentence from Crockett (1976):

      (i)   [my   s   Petej]   poedem   segodnja   za   gorod.
            1PL-NOM with Peter-INSTR will-go   today   beyond   city
            'I and Peter will go to the country today.'

absence of verbal copula possibly suggests the use of RootSCs:

(50)   'napišti   māt-im   eql-um-<u>ma</u>   *Akkadian*
         soul.of   land.GEN field.NOM.CONJ
         'The soul of the land is the field.'

In addition, Bowers (1993) analyzes English *as* as a realization of the head of PrP (Predication Phrase):

(51)   She regards [_{SC} Mary <u>as</u> a fool/crazy.]

Of note here is that English *as* (and Akkadian *–ma*) can serve as glue for both predication (interclausally, as in (51)) and to connect clauses (extraclausally, as in (52)):

(52)   a.   Peter will be late, as will John.
         b.   As she was approaching, the door opened.

Note also that *as* is used to solidify/cement predication only in small clauses, where, arguably, there are no other functional projections that can do the job.

   As pointed out in fn. 20, Jordens (2002: 741, 750) has argued that Dutch children pass through a stage which is characterized by the use of proto-functional categories, which are syntactically adjoined, rather than integrated into the head/complement structure (his 'conceptual-ordering' stage). These proto-functional categories, according to Jordens, are linking elements between the topic and the predicate (p. 744). In the next, 'finite-linking stage', these proto-functional words are grammaticalized into auxiliaries, which now serve as heads in head-complement structures (p. 750). This would mark the beginning of the specific functional category stage, as discussed in the following subsection.

   In sum, this section hypothesizes that (proto-)conjunctions may have been the first functional categories to emerge, for the primary purpose of solidifying Merge, that is, of providing more robust (duplicated) evidence of Merge than just supra-segmentals can do. If 'and' emerged as a default connector, as a proto-functional category, then it is not surprising that 'and' exhibits exceptional behavior in comparison to the other functional categories. Finally, if Merge was advantageous to our ancestors, then providing robust and unambiguous evidence of Merge would have constituted a clear and concrete advantage, which could have been targeted by natural/sexual selection.

4.3.   *Specific Functional Category Stage*

Finally, such particles/conjunctions could have grammaticalized into specific functional categories, such as predication head or tense head or complementizer — another syntactic breakthrough and the beginning of modern syntax, which can now not only use functional words as glue to connect words/phrases/ clauses, but which can also use them to build specialized functional projections, such as TP/IP or CP, which now both motivate and facilitate Move. A modern

functional category such as a modal verb, or a complementizer, can be seen as providing not only segmental glue/evidence of Merge, but also, simultaneously, an expanded structural space, as well as additional nuances of meaning. It may well be that the innovation of Move coincided with the introduction of specialized functional categories, which both serve as landing sites for Move, and as 'probes' (triggers) for Move.

Given this view, one can expect to find transitional constructions, those straddling the boundary between coordination and subordination, and such are not difficult to find. To take one example, the most neutral, prototypical of conjunctions, 'and', can express subordinating relationships, such as consequence:

(53) a.   Give him an inch, and he will take an ell.

(Oxford English Dictionary)

 b.   Speak one word, and you are a dead man!

(Oxford English Dictionary)

 c.   One more can of beer and I am leaving.

(Culicover & Jackendoff 2005: 474)

In (53) above, the relationship between the two clauses is best paraphrased as involving a conditional, *if–then* sequence. Culicover & Jackendoff (2005: 474) call this use of *and* 'left-subordinating and'.

Of course, the functional category stage may have divided into finer-grained sub-stages, as pointed out by a reviewer. Perhaps there was a stage in which aspect was grammaticalized, but not tense yet. Perhaps there was a stage in which TP could be built, but not CP yet (see also the discussion regarding proto-Indo-European in section 3). Perhaps gender/number agreement (e.g., on participles) emerged before person agreement (see Progovac 2008).[28] Perhaps a stage where the verb takes only one argument (intransitive stage) emerged before a transitive stage. But my primary focus in this article is on the first two hypothetical stages, paratactic and coordination stages, on envisioning what our grammars might have looked like in those initial stages, and how these initial stages may have provided the foundation for building layered syntax. My purpose was also to show how postulating these hypothetical stages can shed light on the quirks and complexities of present-day syntax.

Another question that arises is whether the advent of functional categories automatically leads to a hierarchical, subordination stage. In this respect, Kiparsky (1968) has argued convincingly that proto-Indo-European syntax was characterized by optional adverbial temporal particles, which did not build TPs. It is really the emergence of functional heads which take complements, and which build their own functional projections, that constitutes the hierarchical breakthrough. This leads to layers and layers of hierarchical structure, which can now be connected by Move. In other words, it is not a temporal adverbial particle adjoined to a SC that creates hierarchical syntax; it is a TP superimposed over the SC, which moreover may interact with the subject of the small clause by attracting it to its own specifier by Move (Progovac 2008).

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[28]   Boeckx (2008: 119) suggests that Agree may have emerged after Merge.

The gradualist approach to the development of syntax sketched in this section, which assumes a progression in stages, guided by natural/sexual selection, is in the spirit of the general vision outlined in Pinker & Bloom (1990). Pinker & Bloom assume the Baldwin Effect for syntax, the process whereby environmentally-induced responses set up selection pressures for such responses to become innate, triggering conventional Darwinian evolution (see also Hinton & Nowlan 1987 and Deacon 1997).[29] Tiny selective advantages are sufficient for evolutionary change: According to Haldane (1927), a variant that produces on average 1 per cent more offspring than its alternative allele would increase in frequency from 0.1 per cent to 99.9 per cent of the population in just over 4,000 generations. This would still leave plenty of time for language to have evolved: 3.5–5 million years, if early *Australopithecines* were the first talkers, or, as an absolute minimum, several hundred thousand years, in the unlikely event that early *Homo Sapiens* was the first (Stringer & Andrews 1988). In addition, sexual selection can trigger a runaway effect, which can speed the process up significantly (Fisher 1930; see also Miller 2000 and Hurford 2007). Also, fixations of different genes can go in parallel.

A reviewer wonders if my third, subordination stage may not have coincided with the postulation of the 'Middle to Upper Paleolithic transition/revolution', around 43–35,000 BP (before present), based on some recent archeological findings. According to Mellars (2002) and others, this period was characterized by major changes, all reflecting shifts in many different dimensions of human culture and adaptation: New forms and complexity of stone, bone and other tools; explosion of explicitly decorative or ornamental items; representational art carving of animal and human figures; increase in human population densities.[30] To many this 'symbolic explosion' is exactly what one might anticipate from a major shift in the structure of complexity of language patterns, possibly associated with corresponding shifts in the neurological structure of the human brain (Mellars 1991: 35, Bickerton 1995, Pinker 1995, Mithen 1996). Many see this explosion as potentially indicative of the emergence of relatively complex language patterns (Mellars 1991: 41). Klein (2000) has pointed out that there is no way that we can exclude the possibility of relatively sudden punctuational

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[29]   Many evolutionists have adopted the Baldwin effect as an evolutionary force, including Dawkins (1999) and Deacon (1997). Pinker & Bloom (1990) and Briscoe (e.g., 2000) have applied it to language evolution. However, as pointed out by a reviewer, Longa (2006) questions the Baldwin effect for language evolution, as well as for evolution in general. He argues that the effect lacks empirical support, and that some authors who invoke it conflate it with the Waddington's effect, wrongly. Longa's point is that the Baldwin effect cannot be conflated with Waddington's effect because the two differ with respect to the timing at which a mutation occurs. With the Baldwin effect, he argues, the mutation necessarily occurs after the environmental change, while with Waddington's effect, the mutation was present even before the environmental effect. As far as I can see, my data and analysis are consistent with the preexisting mutation scenario. Suppose that an innovation occurs in a community: For example, one or two people begin to merge words by using (proto-) conjunctions. Suppose, further, that this innovation becomes useful for survival, or perhaps attractive to the opposite sex. Those who have a pre-existing mutation which facilitates the use of language in this way will leave more offspring than the others, contributing to the spread of the mutation.

[30]   In fact, an increase in population size may have itself accelerated sexual selection with respect to language, due to more competition.

developments in human behavior and mentality, potentially as a result of either major population bottlenecks or of genetic mutations influencing the structure of the brain.

These findings are often interpreted to mean that language (or syntax) *in its entirety* arose through one single gigantic event, such as a mutation (Hauser *et al*. 2002, Chomsky 2005, Fitch *et al*. 2005). However, even in Klein's and Mellar's work, the suggestion is only that *more complex* forms of language might have coincided with this event. So, in a gradualist evolutionary framework, it could have been the stage which introduced the first proto-coordinator, or it could have been the emergence of the subordination stage, which includes the CP projection. In this scenario, protolanguage could have existed, and could have been evolving, for a long time before that. But it is also difficult to exclude even the possibility that complex language, comparable to present-day languages, was in use for a long time before any cultural revolution took place. Definitive conclusions in this regard are especially difficult to draw given the common assumption, based on present-day cultures, that it is possible to have a highly complex language in the absence of any complex culture.[31]

5.   Some Corroborating Evidence

Language acquisition arguably likewise begins with a root small clause stage (or root infinitive stage) (e.g., Radford 1988, 1990, Lebeaux 1989, Platzak 1990, Ouhalla 1991, Jordens 2002; see Guasti 2002 for many more references and also for opposing views). Also, subordination and CP, as well as Move, seem to emerge later in children. According to Studdert–Kennedy (1991) and Rolfe (1996), present-day views of ontogeny/phylogeny warrant the use of ontogeny, development in children, to *corroborate* hypotheses about phylogeny, development in species.[32] The emergence of TP/IP and CP in phylogeny, just as it does in ontogeny, would have created opportunities for specialization and division of labor among small clauses, TPs/IPs, and CPs, leading to many complexities of syntax.

Agrammatism is another potential source of corroborating evidence. Kolk (2006, and many references there) argues that with Dutch and German agrammatic speakers, preventive adaptation results in a bias to select simpler types of

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[31]   In addition, the significance of one of the archeological findings, the use of decorative/ornamental items, needs to be reevaluated in the context of some non-humans, such as bower birds, who build intricate homes, and decorate them extensively, most probably in order to attract females. As mentioned in Miller (2000), males that build superior bowers can mate up to ten times a day with different females. It is possible that decorating in humans was also done for sexual attraction purposes (see fn. 30). In any event, human language and symbolic thought, which is often associated with it, do not seem to be necessary prerequisites for elaborate decorating.

[32]   For some old and some recent views on the relationship between ontogeny/DEVO (development in children) and phylogeny/EVO (development in species), see also Ridley (1993), Fitch (1997), Carroll (2005), Locke & Bogin (2006), Locke (2009). As mentioned in Ridley, the relationship between ontogeny and phylogeny has been a classic question in evolutionary studies, even though strict recapitulationist views are no longer held. Given these considerations, I am assuming here that any parallelism provides some corroborating evidence, even though clearly not decisive proof.

constructions, often sub-sentential (including root small clauses and root infinitives). These clauses show morphology and basic word order (with no Move), distinct from what one finds in finite clauses:

(54)   a.   Koffie   drinken.   *Dutch*
             coffee   drink.INF
       b.   Portemonnai   verloren
             wallet   lost.PAST-PART
       c.   Tedereen   naar buiten
             everybody   to   outside

Whereas control speakers produced 10% non-finite clauses, aphasics produced about 60%. In children, the overuse of non-finite clauses decreased with age: from 83% in the 2-year-olds, to 60% in the 2.5-year-olds, to 40% in the 3-year-olds. Recent computational and brain-imaging work indicates that the selection of these sub-sentential forms is task dependent, arguably used to prevent computational overload. A PET study by Indefrey *et al*. (2001) shows that non-finite clauses require less grammatical work. The use of subordination/CP is also affected in agrammatic patients (see, e.g., the 'tree-pruning approach' of Friedmann & Grodzinsky 1997 and Friedmann 2002).

The data introduced in the previous sections, the 'living fossils' of syntax, are characteristically formulaic/stereotypical expressions (e.g., *Case closed; Me first; Nothing ventured, nothing gained*). According to, for example, Code (2005: 317), non-propositional, stereotypical/formulaic uses of language might represent fossilized clues to the evolutionary origins of human communication, given that their processing involves more ancient processing patterns, including more involvement of the basal ganglia, thalamus, limbic structures, and the right hemisphere (see, e.g., Lieberman 2000 for an extensive argument that subcortical structures, basal ganglia in particular, play a crucial role in syntax). Basal-limbic structures are phylogentically old and the aspects of human communication associated with them are considered to be ancient, too (van Lancker & Cummings 1999, Bradshaw 2001). For example, a stroke to the right basal ganglia can lead to the loss of overlearned/formulaic speech, including swearwords, prayers, and counting (Speedie *et al*. 1993, van Lancker & Cummings 1999). Robinson (1972) proposed that two levels of the human nervous system are responsible for speech/language: An older system, and a newer cortical system. These considerations are consistent with the gradualist approach to syntax explored in this article.

6.   Conclusion: Back to Subjacency

This article has concluded that syntactic islands do not form a natural class, but that non-islands do, and that, for this reason, there can never be a principled account of islandhood/Subjacency. My proposal is that Subjacency is not a specific principle of syntax, but rather the default state of syntax, dating back in time to the evolutionary beginnings of language, in which Move was unavailable. I have hypothesized two intermediate stages in the development of syntax,

in which Move (e.g., across clause boundaries) was unavailable: The adjunction/parataxis stage (à la Jackendoff 1999, 2002), and the coordination stage. In this view, Move and subordination are later innovations, probably made possible by the emergence of specialized functional categories and their projections, such as TP and CP. Various syntactic constructions in present-day use still preserve the state of syntax lacking Move, giving an illusion of Subjacency.

My proposal reverses the direction of syntactic evolution hypothesized in Newmeyer (1991), who also explores a gradualist approach. While Newmeyer assumes that the initial stages of syntax were characterized by Move free of Subjacency, I propose exactly the opposite, that islandhood (or the state with No Move) was the norm in the previous stages, and that Move was an innovation. This reversal allows me to kill three birds with one stone. First, it provides some rationale for characterizing islandhood/Subjacency as the default state of grammar, rather than as a constraint on grammars. Second, this allows me to explain the existence of various fossilized expressions (arguably 'living fossils' of this proto-syntax stage), which cannot be manipulated by Move.

Third, and most importantly, this allows me to address the question of how or why the progression took place from the proto-syntactic stages with no Move and no subordination, to the stage(s) with Move and subordination. Instead of targeting the abstract and obscure Subjacency by natural/sexual selection, as in Newmeyer's (1991) proposal, my proposal targets the emergence of subordination (Move emerging in conjunction with it). In comparison to its more primary counterparts, adjunction and coordination, subordination provides a clear and concrete advantage in the expressive power. One such advantage is that subordination, and only subordination (57), affords the possibility to recursively embed multiple viewpoints one within another, as seen in these repeated examples:

(55)   [As you know,] [as Mary knows,] he is a linguist.   *Adjunction*

(56)   He is a linguist, [and you know it,] [and Mary knows it].   *Coordination*

(57)   You know [that Mary knows [that he is a linguist]].   *Subordination*

This communicative advantage is concrete enough that it could have been targeted by natural or sexual selection.

This article offers a hypothesis which is consistent with a lot of language data, with how grammaticalization processes work, as well as with many studies in language acquisition and processing. Moreover, this proposal offers a reconstruction of how communicative/functional benefits may have been involved in shaping the formal design of language itself. Finally, an important advantage of this proposal is that it does not force us into conclusion that syntax is all or nothing, and that the evolution of syntax as a whole had to have been a sudden, passive, and inexplicable event, inexplicable in the sense that the nature of its evolution has nothing to do with its design. The approach explored here leaves open the possibility that syntax played an active role in evolving human beings. If we do not explore this kind of approach, in order to prove it or disprove it, we will never know.

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The Urge to Merge:
Ritual Insult and the Evolution of Syntax

Ljiljana Progovac & John L. Locke

Throughout recorded history, sexually mature males have issued humorous insults in public. These 'verbal duels' are thought to discharge aggressive dispositions, and to provide a way to compete for status and mating opportunities without risking physical altercations. But, is there evidence that such verbal duels, and sexual selection in general, played any role in the evolution of specific principles of language, syntax in particular? In this paper, concrete linguistic data and analysis will be presented which indeed point to that conclusion. The prospect will be examined that an intermediate form of 'proto-syntax', involving 'proto-Merge', evolved in a context of ritual insult. This form, referred to as exocentric compound, can be seen as a 'living fossil' of this stage of proto-syntax — providing evidence not only of ancient structure (syntax/semantics), but also arguably of sexual selection.

Keywords: evolution; exocentric compounds; proto-syntax; ritual insult; sexual selection

1.   The Problem with Syntax

Doubt has been expressed that complex syntactic patterns conferred communicative benefits on our evolutionary ancestors and, therefore, evolved by way of natural or sexual selection (e.g., Bickerton 1990, 1998, Lightfoot 1991, Berwick 1998, Newmeyer 2003). This doubt comes primarily from the observation that the principles of grammar, especially syntax, seem rather abstract and arbitrary, and are thus not easily amenable to evolutionary forces such as selection. Under the circumstances, it makes sense to look elsewhere for an explanation, such as verbal complexity or display (Locke 2008, 2009), or to consider the most basic (proto-)syntactic combinations (Progovac 2006, 2008, 2009). Here, we consider the possibility that a specific form of 'proto-syntax' evolved in a context that included a particular type of verbal display — ritual insult. We present a type of compound, the *exocentric* compound, which can be seen as a 'living fossil' of this stage of proto-syntax. While our paper cannot provide physical proof that sexual selection played a role in reinforcing proto-syntax, it points to places where such

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proof can be sought, and it opens up new ways in which linguistic data can be used to raise questions, and formulate hypotheses, about language evolution.

2.   Verbal Dueling

Throughout recorded history, sexually mature males have issued humorous insults in public. An ancient form, "flyting," occurred in the *Iliad, Beowulf, Canterbury Tales*, and many other early texts (Parks 1990). In Old Norse, one type of flyting was called *mannjafnaðr* 'man-matching' (or 'man-comparison'). The term derives from a legal procedure used by surviving relatives to "assess the cash value of slain men" (Clover 1980: 445). In man-comparison flyting, the winner was "the better man" at boasting and insulting (Harris 1979). Many of the insults had been generated, or heard, prior to the engagement in which they were used — an important feature if men are to engage, and demonstrate their cognitive and linguistic skills, without getting hurt. Given that verbal rituals have persisted throughout recorded history, there is no reason to believe that they were not operative also at the very dawn of language.

Ritual insulting continues today in a wide range of cultures around the world (see references in Locke & Bogin 2006 and Locke 2009). In the typical case, two familiar males direct alternating remarks at each other competitively, before a spontaneously assembled audience. Success in these contests rests on humor, fluency, timing, and, since much of the best material is 'prepackaged', memory.

These 'verbal duels' are thought to discharge aggressive dispositions (Marsh 1978), and to provide a way to compete for status and mating opportunities without risking physical altercations (Locke 2008). Aspects of verbal duels resemble the vocal duels of some avian species which are also performed primarily or exclusively by males (Vallet & Kreutzer 1995, Leboucher & Pallot 2004, Rogers *et al.* 2006). There are additional similarities to the loud calls of orangutans and baboons (Fischer *et al.* 2004, Delgado 2006), which — as in the human and avian cases — are issued primarily or exclusively by males and carry information about competitive ability and physical stamina as well as rank (Seyfarth & Cheney 2003, Fischer *et al.* 2004, Kitchen *et al.* 2004).[1]

Linguists have observed that it is difficult to derive human syntax from primate calls and grunts (e.g., Newmeyer 2003), but it may be easier to detect continuity when viewing intermediate forms of language (see, e.g., Jackendoff 1999, 2002). To us, the theoretical significance of the aggressive vocal displays of male apes and the ritual insults of male humans is that the former may have intergraded into the latter at an early stage in linguistic evolution. This is more easily seen when one considers the format of a specific type of insult, one that reflects a combination of expressive and generative, or 'proto-syntactic', power — the exocentric compound.

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[1]   As pointed out by a reviewer, the vocal duels by other species are of course in many respects different from the insults that humans can generate by using and combining meaningful language units. Nonetheless, there is a clear similarity of purpose. Humans have evolved another strategy, vocal and verbal indices to fecundity, temperament, and certain cognitive abilities, thus to fitness.

3.   Compounding the Insult

In the transition from infancy to childhood, when syntax emerges, developments occur in three other areas that are relevant to our claim: (1) the ability to spontaneously generate compounds (Becker 1994), (2) the tendency to tease and insult (McGhee 1976, Apte 1985), and (3) the onset of agonistic verbal engagement or verbal dueling (Gossen 1976, Wyatt 1995, 1999). Both teasing and insulting, and verbal dueling, are predominately male behaviors, even at the time of their appearance in late infancy or early childhood. It is also relevant in this respect that syntactically simpler structures emerge before their more complex counterparts, and this is also true of compounds. There is evidence that verb–noun (VN) compounds that are exocentric, that is, not headed (section 4.1), are used for naming purposes long before children are able to create well-behaved (headed) compound types. In this respect, Clark *et al*. (1986) found that children initially produce compounds such as *grate-cheese*/*rip-paper* in lieu of *cheese-grater*/*paper-ripper*.

When it comes to insults, single words clearly suffice, but combining two words into a meaningful unit greatly expands expressive power. Consider exocentric VN compounds that are primarily used in derogatory references, e.g., English *dare-devil*, *kill-joy*, *pick-pocket*, *scatter-brain*, *turn-coat*; Serbian *cepi-dlaka* 'split-hair' (hair-splitter), *guli-koža* 'peel-skin' (who rips you off), *vrti-guz* 'spin-butt' (restless person, fidget).[2] (See the appendices for many more examples.) These compounds used to be productive and plentiful across languages, numbering in the thousands, but they are now reduced to a few survivors. While some of these compounds (less vulgar ones) have survived as common nouns, they all originated as appellations.

It is thought that naming was among the first uses of language for referential purposes, preceding the so-called epistemic stage, in which language is used to express propositions or statements.[3] It has also been speculated by, for example, Rolfe (1996) that humans initially used verbs to issue commands (cf. imperative), even in the one-word (pre-syntactic stage), and much before they used verbs to make statements.[4] It is thus intriguing that these compounds in

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[2]   A reviewer points out that a label such as *daredevil* is not necessarily derogatory, but that it may even invite admiration. As also discussed in fn. 5, there are general desiderata for VN compounds, and the large majority of them, although not every single one of them, conform to such desiderata, which include humorousness, playfulness, and derogatory nature. As for *daredevil*, while it may be perceived as a label for someone who is daring, it can also be perceived as a label for someone who is unrealistically daring. To put it another way, while daredevil-ish acts by themselves may attract positive attention and admiration, calling somebody a daredevil can constitute a warning that the display is pushing the limits of actual capabilities, and that the daredevil may not be around for a long time.

[3]   A reviewer wonders what the purpose of naming would have been without being able to create propositions. Naming could have been used for endearment/intimacy purposes (perhaps equivalent to grooming), for summoning, as it is still used today, as well as for insulting purposes, if our hypothesis in this paper is correct.

[4]   Some indirect support for this hypothesis comes from the observation that imperative forms tend to be the least marked verbal forms across languages, and/or that they tend to preserve archaic patterns (e.g., Kurylowicz 1964: 137, Dixon 1994: 189). In addition, imperative has been reported to be among the first productive verbal forms used by young children (see, e.g., Bar-Shalom & Snyder 1999).

Serbian, as well as in other languages, feature a verb form that coincides with the imperative (section 4.2). In addition, many of these compounds operate with a very basic vocabulary, consisting mainly of concrete nouns and verbs, frequently referring to body parts or functions, but the combinations are striking metaphors that can express abstract human traits succinctly, creatively, and humorously.[5] We suggest that at some point following the evolution of proto-language, combinations comparable to exocentric compounds were used for ritual insults, or for naming purposes more generally, and that those who used them successfully contributed to the underlying syntactic principle of Merge, which is necessary to create these compounds. Clearly, the ability to Merge in this case would have provided an enormous expressive advantage over just using single-word utterances for naming or insulting purposes, especially in this stage in which the vocabulary must have been very limited.[6]

It is important to keep in mind that we only claim that ritual insult in the form of compounding was *one* of the factors contributing to the consolidation of Merge; we are certainly not claiming that it was the only factor. As pointed out by a reviewer, the emergence of (proto-)Merge would have brought about a host of other communicative advantages. The reason why we are exploring ritual insult and sexual selection here is because the particular data we are considering, exocentric compounds, find the best explanation in these terms. These compounds, unlike any morpho-syntactic form we are aware of, specialize for derogatory reference.

4.   Exocentric Compounds and Proto-Syntax

Jackendoff (1999, 2002) proposed that the relatively flat (non-hierarchical) structure of adjuncts, as well as raw concatenation of compounds, still retain a bit of proto-linguistic flavor, and can be analyzed as syntactic 'fossils' of a previous stage of syntax (see also Bickerton 1990, 1998, for the notion of linguistic 'fossil').

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[5]   Not all acts of compounding are equally successful in achieving these general desiderata. As pointed out by a reviewer, compounds such as *pickpocket* do not sound particularly humorous. But enough of them exhibit these properties to suggest that these were the general desiderata. Those who have studied VN compounds in various languages were impressed by their artistic richness (see also section 4.3). According, for example, to Darmesteter (1934: 443), who studied Romance VN compounds, this kind of composition "may attain Homeric breadth; [… it] belongs to the popular language, to that of arts and crafts, and to poetry. Its richness is inexhaustible." As with any artistic enterprise, some creations are more impressive than others, and this is obvious even among the compounds that have survived. In fact, these varying degrees of success are exactly what is needed for sexual selection to have been able to operate.

[6]   A reviewer is worried that our claim may be characterized as a just-so-story. First of all, our proposal is based on solid and robust linguistic data, available cross-linguistically. At the very least, then, our proposal is a hypothesis about these data, which remain unaccounted otherwise. This is clearly also a proposal that connects linguistic theory to biological theory, in a most direct fashion. Moreover, at this point, there is little that has been proven about language evolution, and any attempts at this point are bound to be speculative to some extent. We believe that, under the circumstances, it is necessary to explore various tacks, and especially those that challenge the status quo in the field, and which promise to open new and original lines of discussion.

Progovac (2006, 2007) has argued that specifically VN exocentric compounds represent 'living fossils', that is, constructions dating back to a proto-syntactic stage, now co-existing with more complex syntactic constructions; according to Ridley (1993: 525), 'living fossils' are species that have changed little from their fossil ancestors in the distant past (e.g., lungfish).[7] While these compounds violate several rules and principles of modern syntax (see below), their structure, as well as their persistence, do provide some continuity with modern syntax. If so, then the syntax that supports their formation (proto-syntax) may have facilitated a transition from a pre-syntactic (one-word) stage to modern syntax.

4.1. Exocentricity and Proto-Merge

It is routinely reported in texts on morphology that verb-noun (VN) compounds introduced earlier (section 3; see also the appendices) are exceptional in that they are exocentric, in contrast to compounds such as *bedroom*, *navy-blue* (also *cheese-grater*), which seem to be headed by the second/rightmost element (e.g., Selkirk 1982, Spencer 1991). Thus, a *bedroom* is a kind of *room*, and *navy-blue* is a kind of *blue*, but *pickpocket* is neither a kind of *pocket* nor a kind of *pick*, but rather a person (who picks pockets/steals). Modern syntactic theory, including Minimalism (e.g., Chomsky 1995), considers that a syntactic combination of two elements (Merge) creates a phrase, the nature of which is determined by one of the merged elements acting as a head.[8] The headedness principle is central to syntactic theory, and is taken to apply to complex words as well, including compounds (e.g., Williams 1981).[9] It is obvious that Merge does not apply in the typical fashion in exocentric compounds, and our argument is that these compounds involve a proto-Merge, that is, Merge that does not create hierarchical structure, but rather just involves flat concatenation/adjunction, as will be further explicated in the following sub-sections. This is just one of many ways in which VN compounds are surprising.

4.2. Ancient Verb Forms

Due to the conservative morphology of certain languages (e.g., Serbian), it is

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[7] Progovac also explores the fossil analysis with some semi-productive, marginal root small clauses in English and Serbian, such as *Me worry*?!, *Family first*!, *Problem solved* (see e.g., Progovac 2008, 2009, and other papers cited there).

[8] A reviewer points out that it may be sufficient for Merge that the last element in exocentric compounds is a noun — that renders the whole compound a noun, which would then render this application of Merge unexceptional. This may indeed have been enough for proto-Merge, or whatever process it is which applies in exocentric compounds, but it is not enough for modern Merge. In productive compounds such as *toothbrush*, it is not just enough for the head of the compound to be a noun, but the syntactic head of the compound also has to be the semantic head, necessarily rendering *toothbrush* a kind of brush, and never a kind of tooth. Similarly, *drive-truck* is no longer a viable way in, for example, English or Serbian for expressing the notion of a truck-driver (even though it is in child speech; see section 3).

[9] Many researchers have established the parallelisms between clause formation and formation of certain compounds, including the application of (equivalents of) Merge and Move. These include Roeper & Siegel (1978), Fabb (1984), Sproat (1985), and many others (see also Spencer 1991).

possible to tell that the form of the verb used in these compounds is an ancient unmarked form, the form which is best approximated in many present-day languages by the imperative (Progovac 2006, 2007). The imperative analysis of VN compounds has been explored by many traditional grammarians and researchers, not only for Serbian (e.g., Stevanović 1956, Mihajlović 1992), but also for English (e.g., Weekley 1916, Jespersen 1954) and Romance languages (e.g., Darmesteter 1934, Lloyd 1968). The imperative morphology in VN compounds highlights not only their exotic and ancient nature, but also points to their essentially clausal derivation.

Verb–Noun compounds are used productively in some Romance languages of today, including Spanish and Italian, although not as derogatory labels for people, but primarily as names for instruments or plants (cf. Spanish *lava-platos* 'wash-dishes, dishwasher', *para-caídas* 'stop-falls, parachute'). Lloyd (1968) claims that Romance VN compounds originated from nicknames, usually playful and humorous, and then spread to the other areas, around the 12th/13th century, possibly due to the lack of a competing pattern, such as English *-er* compound type (e.g., *dishwasher*). The latter use of VN compounds in Romance is not common in Latin texts, and is also marginal in Rumanian, where VN compounds "belong to affective and familiar language," and where they are "exclusively epithets applied to persons in a contemptuous fashion, as are the earliest examples in the other Romance languages" (Lloyd 1968: 7). According to Lloyd, many of the original VN compounds were coarse and humorous, and because of that did not enter the texts and reference books.[10]

4.3.   Proto-Predication and Expressive Power

There is another reason to believe that verbal compounds resemble mini-clauses, and that Merge does not apply to them in the usual fashion. In addition to featuring the (imperative) verb, VN compounds also involve basic predication: The noun is interpreted as an argument of the verb, but whether it is an internal

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[10]   It may be that in these modern-day Romance languages, even though not all, VN compounds have re-emerged in a slightly more complex form than original VN compounds. First of all, unlike in English or Serbian, it is common to use plural nouns inside these compounds in Romance, as the Spanish data in the text illustrate (see e.g., Ferrari 2005). Second, Italian and French VN compounds also differ from their Serbian counterparts with respect to gender specification. Ferrari reports that Italian VN compounds are uniformly M(asculine), suggesting that they might have a null head with an M feature (for similar arguments for a null suffix in such compounds in French, see Rohrer 1977 and Lieber 1992). In Serbian, the gender of the VN compound is often not uniquely determined. For example, if the noun merging with the imperative verbal form is F(eminine) (e.g., in *ispi-čutura*, *čutura* is F), the demonstrative for the whole compound can be either F or M, even if a compound is used to refer to a male (see Progovac 2007 for details). Third, Spanish VN compounds are recursive (e.g., *limpia-para-brisas* 'wipe-stop-wind, windshield wiper'), unlike English or Serbian VN compounds, which are not recursive (English **scare-pick-pocket* 'one who scares pickpockets' or Serbian **muti-ispi-čutura* 'one who confuses drunkards'). All this points to the conclusion that the productive VN compounds in some Romance languages may be structurally more complex creations than the original ones, creations that better conform to the rules of modern morpho-syntax. The discussion in this paper focuses solely on the simpler and more ancient compound type, which is no longer productive, at least not in the languages under consideration here, English and Serbian.

argument (object) or external argument (subject) does not seem to be structurally determined. For example, a *killjoy* is somebody who kills joy, with the noun *joy* acting as the internal (object) argument of the verb. Most VN compounds are of this type. However, in a compound such as *crybaby* the noun is clearly an external argument (subject). Compounds like *crybaby* may give an impression of well-behaved, endocentric compounds, but their morphological make-up is identical to that of *killjoy* type compounds, including the imperative verb form in Serbian equivalents (Progovac 2007).

There is further evidence pointing to the conclusion that we are not dealing here with two distinct compound types, one endocentric (*crybaby*), and one exocentric (*killjoy*). The evidence comes from VN compounds which can be dually interpreted. For example, a *daredevil* is someone who dares a devil (internal/object argument interpretation) and can also be a devil who dares (external/subject argument interpretation). In other words, *devil* in *daredevil* can be interpreted as both the subject and the object of the verb, simultaneously, showing that predication/thematic structure is not as uniquely and precisely specified in VN compounds as it is with typical syntactic constructs which involve modern Merge (see Progovac 2007 for more discussion). Another example is the Serbian compound *pali-drvce* (lit. 'ignite-stick', i.e. matches), where *drvce* 'stick' is both what gets ignited and what ignites. This gives further credence to our claim that the syntax behind VN compounds is basic, non-hierarchical, and that it involves a more basic type of Merge.

One might be tempted to say here that the ambivalence in interpretation suggests more structure, rather than less, as pointed out by a reviewer. However, the assignment of theta roles in these compounds is a matter of vagueness, rather than ambiguity. It is usually taken that vagueness allows for two (or more) interpretations at the same time, while ambiguity allows for only one interpretation at a time (see, e.g., Kempson 1977). The vagueness analysis favors the lack of hierarchical structure in these compounds, in contrast to ambiguity, which would favor multiple hierarchical possibilities. Consider the following example, uncontroversially ambiguous:

(1)   The boy saw the teacher with the binoculars.

Either the boy used the binoculars to see the teacher, or the boy saw the teacher who had the binoculars. These two interpretations will receive completely distinct syntactic representations. Importantly, this sentence cannot be interpreted to mean both that the boy used the binoculars to see the teacher and that the teacher had the binoculars, although this dual interpretation is pragmatically plausible. When it comes to VN compounds, the possibility of dual interpretation points in the direction of vagueness, which, unlike ambiguity, is not tied to distinct syntactic structures. In fact, as pointed out above, what suffices in the case of VN compounds is the analysis which posits proto-predication, where the verb merges with one argument, and where there is no further structure to specify the nature of that argument. The rest is accomplished by pragmatics. In other words, there is no evidence for any differentiation in these compounds between external and internal arguments, the behavior which

resembles absolute arguments in ergative/absolutive languages. In an ergative language, the subject of an intransitive verb is morpho-syntactically equivalent to the object in a transitive construction (see, e.g., Dixon 1994).

A reviewer points out that this possibility for dual interpretation with compounds such as *daredevil* is an exception, rather than a rule, among VN compounds. As discussed above, these compounds *in principle* can be interpreted as either involving an internal argument, or the external argument, as far as their (proto-)syntax goes. The choice is largely due to pragmatics, and, if they are especially witty, such compounds can even be assigned both interpretations at the same time. Take the compound *rattlesnake* as an example which does not allow dual interpretation. In the absence of an accessible concept of a person/animal/instrument that rattles snakes, the only plausible interpretation here is that of a snake who rattles (external argument). For this compound to be interpreted dually, it would take there being snakes that rattle, and that are at the same time rattlers of other snakes, a highly unlikely pragmatic scenario. Thus, for the vast majority of VN compounds it is true that they are interpreted as taking either an internal argument (typical scenario), or an external argument. However, those few that pragmatically allow both interpretations simultaneously show that their (proto-)syntax does not stand in the way of such dual interpretations, the way present-day syntax does (see the example in (1) above). Importantly, the syntactically more complex compound *snake-rattler* cannot be dually interpreted, or interpreted to mean a *rattlesnake*, regardless of the pragmatics, because the syntax here specifies that *snake* is necessarily the internal (object) argument. As pointed out by a reviewer, minimal syntactic specification, and extensive involvement of pragmatics, are the hallmarks of what have been proposed to be syntactic fossils by, for example, Jackendoff (1999, 2002).

It is in fact those compounds that can be doubly interpreted in this way (e.g., *daredevil*) that seem to be most expressive, their expressiveness unmatched by any syntactically well-behaved paraphrase (e.g., *daring person*). By introducing more precision, a more complex syntax precludes exactly this type of double interpretation. This great and unique expressive power of VN compounds may be part of the reason why some of them have been preserved to this date. According to Darmesteter (1934: 443), the artistic beauty and richness of these compounds in French is inexhaustible. Mihajlović (1992), who collected over 500 Serbian place and people names in the form of VN compounds, reports that these condensed compositions pack in them not only sentences, but also frozen fairy tales, proverbs, and ancient wisdoms and metaphors (1992: 8–9). Like Darmesteter, Mihajlović also concludes that their wealth and depth are unfathomable. It is worth observing that even academics of the 20th century found beauty in these compounds, and reacted to them with admiration. Under the assumption that our hypothesis is correct, one can expect that at least as much admiration would have been engendered by comparable creations at a stage when language was just emerging.

4.4.   *Availability across (Unrelated) Languages*

Exocentric compounds are found across not only Indo-European languages, but

also non-Indo-European languages, with intriguing parallels in their morpho-logical and semantic make-up. In Tashelhit Berber, a language belonging to the Afro-Asiatic language family, which is spoken in Southern Morocco, *ssum-sitan* 'suck-cow' (insect) is closely parallel to Old English *burst-cow*, which also meant 'insect'. In addition, the drinking image for a miser *drynk-pany* is reminiscent of *ssum-izi* (suck-fly) in Berber (see Progovac 2006, 2007, for discussion and for additional examples and parallels).

It seems that this type of compounding appears in this VN order even in head-final languages, such as German (*Tauge-nichts*, *lit.* 'be.worth-nothing' = 'good-for-nothing', *Habe-nichts* 'have-nothing', comparable to English *dreadnought* and *know-nothing*).[11] It is not clear, however, if any correlation is expected bet-ween the ordering in exocentric compounds and the current word order in any particular language, for two reasons. First, according to Kayne's (1994) approach to cross-linguistic variation in word order, all languages are underlyingly verb initial, and any surface deviations from this ordering would be derived by various movement operations. If VN compounds involve no movement, as we assume (see Progovac 2007), then, at least for those that involve an internal argu-ment, it is to be expected that even head final languages would have VN ordering in these compounds.

Second, and regardless of whether or not one subscribes to Kayne's (not uncontroversial) approach, we argue that the VN compounds found in present-day languages are fossils of some ancient stage of language, whose word order is thus not expected to be identical to that of any present-day languages.[12] Needless to say, in-depth analyses of these exocentric compounds in additional languages, preferably by their native speakers (given that these compounds are hard or impossible to find in official reference books) would shed further important light on the ideas presented in this paper, and we hope that our paper will stimulate such research.

5.   Fitness Value

As we have seen, verbal dueling appears in a wide variety of places and cultures, begins early in development, and has occurred for the duration of social history, from flyting in the 8th century to 'sounding' (or 'the dozens') and other forms of verbal duels in modernity (Harris 1979, Parks 1990). Elsewhere we have argued that the strong male bias associated with verbal dueling, and attested increases in agonistic verbalization in juvenility and adolescence — taken with other facts — imply a causal role for testosterone (Locke & Bogin 2006). Since testosterone can get young men injured or killed, we suggest here that humorous appellations, in

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[11]   Thanks to Andreas Kyriacou (p.c.) for the German data.
[12]   In this respect, Miller **(**1975: 32) notes that in Proto-Indo-European the productive com-pound type was SV, OV, but that VS, VO was archaic and residual. To him, the residual com-pound type suggests that Proto-Indo-European was a VSO language that shifted to SOV and was in the process of shifting to SVO at the time of our earliest documentation (p. 33; see also Vennemann 1974). According to Miller, the oldest re-constructible stage of Proto-Indo-European may have been VSO (33). Proto-Indo-European also had a marked conjunct order, with the verb at the beginning (Watkins 1963), which is a residue of VS order.

the form of exocentric compounds, were an adaptive way to compete for status and sex. This would have enhanced relative status first by derogating existing rivals and placing prospective rivals on notice; and second by demonstrating verbal skills and quick wittedness, attributes that would have been valued both by men and by women. Darwin (1874) identified two distinct kinds of sexual selection: aggressive rivalry and mate choice (see also Miller 2000a, 2000b), both of which seem relevant for exocentric compounds. Since these compounds were used to (re-)name their victims — an act that in other contexts is preferentially performed by men (Hopper *et al*. 1981) — they would have carried more weight than temporary insults. The challenge therefore would have been to create names that captured the essence of the person in just two simple (concrete) words, a verb and a noun, a feat that clearly requires intelligence, creativity, and originality.

Historical records indicate that the playful and humorous use of vulgarity, in public contests, is a practice in which men clearly dominate (see Abrahams 1962, 1973, 1989, Apte 1985, Garrioch 1987, Pujolar i Cos 1997, and Gallant 2000). Supporting these general tendencies is a particular pattern in the use of insulting compounds — many are vulgar, and the vast majority target males, for example, *Poj-kurić* 'sing-dick' (womanizer; preserved as a name); *jebi-vetar* ('fuck-wind' charlatan, purposeless man). Even those that seem to refer to females, and could, in principle describe females (*Laj-kučka* 'bark-bitch', loud and obnoxious person; *plači-pička* 'cry-cunt', vulgar version of *crybaby*) are in fact typically used in reference to males, for a doubly insulting effect (Mihajlović 1992).[13]

The use of cursing and 'dirty words' is more common in males (Jay 1980, 1995, van Lancker & Cummings 1990). In a study by Code (1982), all the expletive lexical speech automatisms, whether negative (hatred, racism) or positive (humor, sex), were produced by men. Swear words frequently express emotions such as fear, pain, frustration, and may accompany sexual and violent activities. According to Darwin (1872), strong emotions expressed in animals are those of lust and hostility, and they may have been the first verbal threats and intimidations uttered by humans (Code 2005: 322). These considerations are all consistent with our proposal that VN compounds can be seen as 'living fossils' of ancient language forms, loaded with expressive and emotional power, which might have been used predominantly by males for display purposes.[14]

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[13] A reviewer wonders about the generality of the use of vulgarity in VN compounds, given that Serbian compounds seem much coarser than the English ones (see also the two appendices). First, as the reviewer himself points out, there are a few quite coarse compounds in use in English as well: *fuckwit, shithead, piss-poor, piss-artist*. Of note is also that the same basic verbs denoting bodily functions (*fuck, shit, piss*) are used in both English and Serbian (Appendix A). Second, sources dedicated to English and Romance VN compounds mention their "unquotable coarseness" (Weekley 1916), which led to their exclusion from dictionaries and grammar books, and to their virtual extinction (see also Lloyd 1968 and Darmesteter 1934). Coarse VN compounds are also routinely banned from Serbian reference books. Mihajlović (1992), from which most coarse compounds are taken, is a lonely exception. This reference is specifically devoted to VN compounds, and is a result of a thirty-year field effort which involved covering village by village, and consulting records of names in each. The pattern of vulgarity, thus, seems to be general, even if places where fossils of this pattern are preserved may be random.

[14] Our claim that sexual selection played a role in the emergence of exocentric compounds, and

If syntax evolved gradually, as has been proposed (e.g., Pinker & Bloom 1990, Jackendoff 1999, 2002, Progovac 2008, 2009), there may be some evidence of interspecific continuity. We note attested associations between innovation and intelligence throughout the primate world (Ramsey *et al*. 2007), and evidence of vocal innovation in chimpanzees (Hopkins *et al*. 2007).[15] There also are associations, in our own species, between various lexical measures (e.g., vocabulary size, metalinguistic skills) and general intelligence (Locke 2008). Taken together, these facts suggest that the ability to create and use insulting and humorous compounds in a competitive way may have improved status and mating opportunities in our evolutionary ancestors. If so, it is possible that creations comparable to exocentric compounds helped facilitate the transition from proto-language to syntax.

6. Conclusion

Not only do exocentric VN compounds suggest an ancient syntactic/combinatorial strategy, but their semantics and use also provide potential evidence of ritual insult and sexual selection at work, selecting for this basic/proto-syntax. The following special and unique properties of these compounds, difficult or impossible to explain otherwise, support the sexual selection proposal.

First, these VN compounds specialize for derogatory reference, often vulgar, providing evidence of aggression. Second, there is evidence in these compounds that males are targeted for insults, rather than females. Third, VN compounds are striking, expressive, novel metaphors, which use the most basic vocabulary (including body parts and functions) to express quite abstract human traits. Fourth, VN compounds provide evidence of imagination, quick-wittedness, and (crude) humor. Finally, the vast number of these compounds (reported to have been in the thousands!) clearly exceeds what is needed for survival or just communication; such excess is typically ascribed to sexual selection forces. As put in Miller (2000a: 369), "if language evolved in part through sexual choice as an ornament or indicator, it should be costly, excessive, luxuriant beyond the demands."

If the ability to merge two words to create a more stunning (ritual) insult was beneficial for sexual selection, then it is possible that the very foundation of syntax, the principle of (proto-)Merge, was reinforced by sexual selection.

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of syntax, is also consistent with findings that reveal sex differences in two cognitive systems that are recruited by language — declarative memory and procedural memory (Pinker & Ullman 2002, Ullman 2008). This issue will have to await further research, however.

[15] According to Miller (2000a: 411) neophilia, an attraction to novelty, runs deep in animal brains.

Appendix A: Some (mostly coarse) Serbian VN compounds as people and place names (taken from Mihajlović 1992)

Čepi-guz	'cork-butt'
Češi-guz	'scratch-butt'
Ćuli-brk	'stick.out-moustache'
Deri-gaća	'rip/tear-underpants'
Deri-kučka	'rip-bitch'
Deri-muda	'rip-balls' (place name, a steep hill)
Draži-vaška	'tease-louse'
Gladi-kur	'smooth-V-dick' (womanizer)
Gori-guzica	'burn-butt' (a person in trouble; cf. English *Burn-breeches*)
Jebi-baba	'fuck-old woman' (unselective womanizer)
Jebi-sestra	'fuck-sister/cousin'
Jebi-vetar	'fuck-wind' (charlatan, good-for-nothing)
Kapi-kur	'drip-dick' (name of a slow spring)
Kosi-noga	'skew-leg' (lame person)
Kovrlji-guz	'drag-butt'
Kradi-gaća	'steal-underpants'
Krpi-tur	'patch-butt' (poor person)
Laj-kučka	'bark-bitch' (loud and obnoxious person)
Lezi-baba	'lie-old-woman' (loose woman or man)
Lezi-tetka	'lie-aunt' (loose woman or man)
Liz-guz	'lick-butt'
Muz-govno	'milk-shit'
Nabi-guz	'shove-butt'
Neper-gaća	'no-wash-underpants'
Peči-govno	'burn-shit'
Piš-kur	'piss-dick'
Plači-guz	'cry-butt' (cf. *crybaby*)
Plači-pička	'cry-cunt' (vulgar version of *crybaby*)
Plaši-vranac	'scare-crow'
Poj-kurić	'sing-dick' (womanizer)
Prdi-kučka	'fart-bitch'
Prdi-vuk	'fart-wolf'
Prdi-zec	'fart-rabbit'
Prti-mud	'carry-balls'
Puš-kur	'smoke-dick'
Razbi-dupe	'break-butt' (steep terrain)
Seri-sabljić	'shit-sword' (cf. English slang *shit-bullets*)
Seri-vuk	'shit-wolf'
Visi-guz	'hang-butt'
Vuci-guz	'drag-butt' (slow-moving person)
Vuci-klašnja	'drag-stockings' (carelessly dressed person)
Vuci-kuja	'drag-dog' (stray dog)

Appendix B: Some additional English VN compounds

As names in English (most are taken from Weekley 1916):

Bake-well ('well' as 'stream/pool'; a well-known advocate for cremation), Ben-bow (bend-bow), Bere-water (bear-water), Bran-foot (possibly from brand-foot, for animals/slaves), Break-speare, Burn-house, Catch-love (love = wolf), Cant-well, Crake-bone, Cut-bush, Cut-fox, Cut-love (love =wolf), Cut-right, Culle-hare (culle = kill), Culle-hog (culle = kill), Culle-bolloc (culle = kill), Do-best, Do-bet, Do-little, Do-well, Doubt-fire (from arch. 'dout' – in charge of furnace), Dread-nought, Drink-low, Drynk-pany (drink penny), Drink-water, Eat-well, Gather-all, Gather-cole (coal or cabbage), Gather-good (good = property, wealth), Go-lightly, Hab-good (from 'hap' = 'to snatch'), Hack-block, Hack-wood, Hate-crist (crist = Christ), Hop(e)-well (well = stream/pool), Hurl-bat, Kill-buck (Place name in the state of New York), Kis-sack, Lack-land, Lack-love, Love-gold, Love-good (probably good = God; contrast with Hate-crist), Love-well, Make-joy, Make-mead, Make-peace, Mar-brow, Mar-wood, Mean-well, Mend-market, Pass-field, Passe-low (cross-water), Perce-forest (perce = pierce), Perce-val (pierce-vale), Pers-house (pers = pierce), Pil-beam (pil = peel, barker of trees), Pinch-back, Porte-rose, Rack-straw (rack = rake), Rid-land (rid = clear), Rid-wood (rid = clear), Save-all, Scare-devil, Scatter-good (good = wealth/property), Shake-lady, Shake-lance, Shake-rose, Shak-shaft, Shake-speare, Shake-staff, Shear-gold (coin-clipper), Shear-lock, Shear-wood, Shave-tail (shave = shove), Spare-good (good = property, wealth), Spare-water, Spin-garn, Spyll-payn, Stab-back, Stand-even, Stand-fast, Strangle-man, Swep-stak, Thack-well (thatcher), Thumb-wood (cf. *mar-wood*; 'thumb' archaic for 'to handle clumsily'), Tickle-penny, Tire-buck (tire = tear), Tread-away, Tread-gold, Tread-well (well = stream), Trede-water, Trust-god, Tuck-well, Turn-bull, Turn-penny, Turn-pike, Wage-spere, Wag-horn, Wag-staff, Wag-tail, Wast-all, Win-bow, Win-penny, Win-rose, Wipe-tail, Wrynge-tail.

As common nouns, probably deriving from names/nicknames:
(based on references such as Weekley 1916, Jespersen 1954, Lees 1960, Marchand 1969)

bang-straw (thresher), break-back, break-covert, break-fast, break-neck, break-vow, break-water, burn-bag, burst-cow (insect), carry-all, carry-tale, catch-fly (plant), catch-penny, cease-fire, cover-shame (plant), cover-slut (apron), cure-all, cut-finger (plant), cut-throat, cut-purse, cut-water, do-nothing, do-nought, dread-nought (originally a person; later a battleship), end-all, fill-belly (glutton), fill-pot, find-fault, hang-dog (originally a person who hangs stray dogs), hang-man, heal-all (plant), hunch-back, kill-devil, kill-joy, kill-lamb (plant), kill-time, know-little, know-nothing, lack-brain, lack-bread, lack-grace, lack-land, lack-love, lack-luster, lack-mind, lack-sense, lack-wit, let-game, lick-box, lick-dish, lick-ladle, lick-platter, lick-pot, lick-spit, lick-spittle, lock-jaw, make-mirth, make-peace, make-rime, make-weight, pass-port, pas-time, pick-lock, pick-purse, pick-thank, pinch-back (miser), pinch-belly, pinch-gut, pinch-penny, prick-bill, rake-hell (scoundrel, ruffian), rake-shame, save-all, saw-bones, scare-crow, scatter-brain, scoff-law, scrape-gut (fiddler), shear-water (bird), shuffle-wing (bird), skin-flint, sling-shot, spend-thrift (miser), spill-bread, spill-time, spit-fire, spoil-sport, spurn-water, stay-ship (fish), stay-stomach 'snack', stop-gap, sweep-stake, swish-tail (bird), tangle-foot (whiskey), tear-thumb, tell-tale, tell-truth, toss-pot, tumble-dung (insect), turn-broach, turn-coat, turn-key, turn-penny, turn-skin, turn-spit, turn-stone (bird), turn-table, wag-tail (bird), want-wit.

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The Third Factor in Phonology

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This article attempts to investigate how much of phonology can be explained by properties of general cognition and the Sensorimotor system — in other words, third-factor principles, in support of the evolutionary scenario posed by Hauser *et al*. (2002a). It argues against Pinker & Jackendoff's (2005: 212) claim that "major characteristics of phonology are specific to language (or to language & music), [and] uniquely human," and their conclusion that "phonology represents a major counterexample to the recursion-only hypothesis." Contrary to the statements by Anderson (2004) and Yip (2006a, 2006b) to the effect that phonology has not been tested in animals, it is shown that virtually all the abilities that underlie phonological competence have been shown in other species.

Keywords: evolution of language; phonology; third factor

1.   Introduction

The present work is a preliminary attempt to determine how much of human phonological computation (i.e., representations and operations) can be attributed to mechanisms which are present in other cognitive areas and in other species. In other words, I explore the idea advanced in many recent Minimalist writings that phonology is an 'ancillary' module, and that phonological systems are "doing the best they can to satisfy the problem they face: To map to the [Sensorimotor system] interface syntactic objects generated by computations that are 'well-designed' to satisfy [Conceptual-Intentional system] conditions" but unsuited to communicative purposes (Chomsky 2008: 136). Phonology is on this view an afterthought, an externalization system applied to an already fully-functional internal language system. While some (e.g., Mobbs 2008) have taken this to suggest that phonology might be messy, and that we should not expect to find evidence of 'good design' in it, there is another perspective which suggests instead that the opposite conclusion is warranted: Even if the Conceptual-Intentional interface is more transparent than the Sensorimotor one, phonology

might nevertheless be much simpler (less domain-specific) than has previously been thought, making use of only abilities that already found applications in other cognitive domains at the time externalized language emerged.

This view accords with the evolutionary scenario developed by Hauser *et al.* (2002a) and Fitch *et al.* (2005), who suggest that language may have emerged suddenly as a result of minimal genetic changes with far-reaching consequences (cf. Pinker & Jackendoff 2005 and Jackendoff & Pinker 2005, who see language as manifesting complex design).[1] Particularly relevant is the distinction that Hauser *et al.* (2002a) make between the 'Faculty of Language – Broad Sense' (FLB), including all the systems that are recruited for language but need not be unique to language, or to humans, and the 'Faculty of Language – Narrow Sense' (FLN), which is the subset of FLB that is unique to our species and to language. At present, the leading hypothesis among proponents of this view is that FLN is very small, perhaps consisting only of some type of recursion (i.e., Merge) and/or lexicalization[2] plus the mappings from narrow syntax to the interfaces. Pinker & Jackendoff (2005: 212) claim that phonology constitutes a problematic counterexample to this hypothesis because "major characteristics of phonology are specific to language (or to language & music), [and] uniquely human." In this article, I investigate the extent to which Pinker & Jackendoff's criticism is viable, first by examining what abilities animals have which are relevant to phonology, and then by sketching out an account which I develop more fully elsewhere (Samuels 2009a), which I argue is consistent with the view that FLN is quite limited.

2.   What Does Phonology Require?

Few authors have discussed phonology as it pertains to the FLN/FLB distinction. For example, Hauser *et al.* (2002a: 1573) list a number of approaches to investigating the Sensorimotor system's properties (shown below in (1)), and these are all taken to fall outside FLN. However, none of these pertain directly to phonological computation.

(1)   a.   *vocal imitation and invention*

                Tutoring studies of songbirds, analyses of vocal dialects in whales, spontaneous imitation of artificially created sounds in dolphins

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[1]   The relation of Hauser *et al.*'s claims to the Minimalist Program is somewhat controversial, and the authors themselves claim that the two are independent. At least from my personal perspective, they are two sides of the same coin.

[2]   Hauser *et al.* focused on the idea that recursion might be the crucial component in FLN. However, it has proven difficult to pinpoint what is meant by recursion in the relevant sense, such that it may be unique to humans and to language. Another hypothesis to which I am sympathetic has been proposed by authors such as Spelke (2003) and Boeckx (in press). On their view, it is not recursion but rather lexicalization — the ability to embed any concept in a 'lexical envelope' which allows it to be recursively Merged — which arose uniquely in our species. For the purposes of the present inquiry, we may simply note that both of these hypotheses exclude phonology from FLN.

b. *neurophysiology of action-perception systems*

Studies assessing whether mirror neurons, which provide a core substrate for the action-perception system, may subserve gestural and (possibly) vocal imitation

c. *discriminating the sound patterns of language*

Operant conditioning studies of the prototype magnet effect in macaques and starlings

d. *constraints imposed by vocal tract anatomy*

Studies of vocal tract length and formant dispersion in birds and primates

e. *biomechanics of sound production*

Studies of primate vocal production, including the role of mandibular oscillations

f. *modalities of language production and perception*

Cross-modal perception and sign language in humans versus unimodal communication in animals

While these are all issues which undoubtedly deserve attention, they address two areas — how auditory categories are learned, and how speech is produced — which are peripheral to the core of phonological computation. Nevertheless, (1c) and (1f), which I discuss in Samuels (2009a: sect. 3.2.1), are particularly interesting. These are relevant to questions of phonological acquisition and the building of phonological categories, including the possibility that phonological features are emergent rather than innate (see Mielke 2008). And the instinct to imitate, addressed in (1a) and (1b), is clearly necessary to language acquisition. However, I leave these items out of the present discussion because neither these nor any of the other items in (1) have the potential to address how phonological objects are represented or manipulated, particularly in light of the substance-free approach to phonology I adopt (see Hale & Reiss 2000a, 2000b, 2008), which renders questions about the articulators (e.g., (1d–e)) moot since their properties are totally incidental and invisible to the phonological system.

Two papers by Yip (2006a, 2006b) outline a more directly relevant set of research aims. She suggests that, if we are to understand whether 'animal phonology' is possible, we should investigate whether other species are capable of the following:[3]

(2) a. Grouping by natural classes
     b. Grouping sounds into syllables, feet, words, phrases
     c. Calculating statistical distributions from transitional probabilities
     d. Learning arbitrary patterns of distribution
     e. Learning/producing rule-governed alternations
     f. Computing identity (total, partial, adjacent, non-adjacent)

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[3] Yip mentions two additional items which also appear on Hauser *et al.*'s list: Categorical perception/perceptual magnet effects and accurate production of sounds (mimicry).

This list can be divided roughly into three parts (with some overlap): (2a–b) are concerned with how representations are organized, (2c–d) are concerned with how we arrive at generalizations about the representations, and (2e–f) are concerned with the operations that are used to manipulate the representations. I would add three more areas to investigate in non-linguistic domains and in other species:

(2) g. Exhibiting preferences for contrast/rhythmicity
     h. Performing numerical calculations (parallel individuation and ratio comparison)
     i. Using computational operations: search, copy, concatenate, delete

In the sections to follow, I will present evidence that a wide range of animal species are capable of the tasks in (2a–i), though it may be the case that there is no single species (except ours) in which all these abilities cluster in exactly this configuration — in other words, it may be that what underlies human phonology is a unique *combination* of abilities, but the individual abilities themselves may be found in many other species. I show (*contra* Yip) that there is already a substantial amount of literature demonstrating this, and that it is reasonable to conclude on this basis that no part of phonology, as conceived in my ongoing work, is part of FLN. In section 3, I focus on the abilities which underlie (2a,b,h) — that is, how phonological material is grouped. Next, in section 4, I turn to (2c-g), or the ability to identify and produce patterns. Finally, in section 5, I discuss (2e,i), the abilities which have to do with symbolic computation.

Before turning to these tasks, though, I would like to address one major concern which might be expressed about the discussion to follow. This concern could be phrased as follows: how do we know that the animal abilities for which I provide evidence are truly comparable to the representations and operations found in human phonology, and what if these abilities are only analogous, not homologous? Admittedly, it is probably premature to answer these questions for most of the abilities we will be considering. But even if we discover that the traits under consideration are indeed analogous, all is not lost by any means. In connection with this, I would like to highlight the following statement from Hauser *et al*. (2002a: 1572):

> Despite the crucial role of homology in comparative biology, homologous traits are not the only relevant source of evolutionary data. The convergent evolution of similar characters in two independent clades, termed 'analogies' or 'homoplasies,' can be equally revealing [(Gould 1976)]. The remarkably similar (but non-homologous) structures of human and octopus eyes reveal the stringent constraints placed by the laws of optics and the contingencies of development on an organ capable of focusing a sharp image onto a sheet of receptors. […] Furthermore, the discovery that remarkably conservative genetic cascades underlie the development of such analogous structures provides important insights into the ways in which developmental mechanisms can channel evolution [(Gehring 1998)]. Thus, although potentially misleading for taxonomists, analogies provide critical data about adaptation under physical and developmental constraints. Casting the comparative net more broadly, therefore, will most likely reveal

larger regularities in evolution, helping to address the role of such constraints in the evolution of language.

In other words, analogs serve to highlight 'third-factor' principles — that is, general properties of biological/physical design (Chomsky 2005, 2007) — which might be at play, and help us to identify the set of constraints which are relevant to the evolutionary history of the processes under investigation. For example, both human infants and young songbirds undergo a babbling phase in the course of the development of their vocalizations. Even though we do not want to claim that the mechanisms responsible for babbling in the two clades are homologous, nevertheless:

> [T]heir core components share a deeply conserved neural and developmental foundation: Most aspects of neurophysiology and development — including regulatory and structural genes, as well as neuron types and neurotransmitters — are shared among vertebrates. That such close parallels have evolved suggests the existence of important constraints on how vertebrate brains can acquire large vocabularies of complex, learned sounds. Such constraints may essentially force natural selection to come up with the same solution repeatedly when confronted with similar problems.
>
> (Hauser *et al*. 2002a: 1572)

We may not know what those constraints are yet, but until we identify the homologies and analogies between the mechanisms which underlie human and animal cognition, we cannot even begin to tackle the interesting set of questions which arises regarding the constraints on cognitive evolution. The present study, then, provides a place for us to begin this investigation in the domain of human phonological computation. I also want to emphasize that the components of phonology in (1)–(2) are intended to be as theory-neutral as possible, though in section 6 I give a brief overview of Samuels (2009a), a theory which I argue is especially well-suited to Hauser *et al*.'s hypotheses regarding the evolution of language, and also congenial to the Minimalist conception of the architecture of grammar. Furthermore, the basic argument I present against Pinker & Jackendoff — namely, that phonology does not constitute a major problem for Hauser *et al*. or for the Minimalist Program — can certainly hold even if one does not adopt my particular view of phonology.

3.   Grouping

Since the hypothesis put forward by Hauser *et al*. (2002a) takes recursion to be the central property of FLN (along with the mappings from narrow syntax to the conceptual-intentional and Sensorimotor interfaces), much attention has been paid to groupings, particularly recursive ones, in language. While phonology is widely considered to be free of recursion,[4] nevertheless grouping (of features, of

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[4]   Some authors have argued for recursion in the higher levels of the prosodic hierarchy (e.g., at the Prosodic Word level or above). See Truckenbrodt (1995) for a representative proposal concerning recursion at the Phonological Phrase level. Even if this is correct (though see Samuels 2009a: chap. 5), the recursive groupings in question are mapped from syntactic

segments, and of larger strings) is an integral part of phonology, and there is evidence that infants perform grouping or 'chunking' in non-linguistic domains as well; see Feigenson & Halberda (2004). Additionally, segmenting the speech stream into words or morphemes (or syllables) also depends on what is essentially the converse of grouping, namely edge detection. We will discuss edge detection and the extraction of other patterns in section 4.

Human beings are masters at grouping, and at making inductive generalizations. Cheney & Seyfarth (2007: 118) write that "the tendency to chunk is so pervasive that human subjects will work to discover an underlying rule even when the experimenter has — perversely — made sure there is none." This holds true across the board, not just for linguistic patterns. With respect to other species, many studies beginning with Kuhl & Miller (1975) show that mammals (who largely share our auditory system) are sensitive to the many of the same acoustic parameters as define phonemic categories in human language (see further discussion in Samuels 2009a: sect. 3.2). Experiments of this type provide the most direct *comparanda* to the groupings found in phonology. Even from a substance-free perspective, such results are valuable because they shed light on the origins of biases in phonetic perception which give rise to phonological patterns (see Blevins 2004 and Samuels 2009a: chap. 2 for an explicit connection to the substance-free program).

 Also, relevantly to the processing of tone and prosody, we know that rhesus monkeys are sensitive to pitch classes — they, like us, treat a melody which is transposed by one or two octaves to be more similar to the original than one which is transposed by a different interval (Wright *et al*. 2000). They can also distinguish rising pitch contours from falling ones, which is an ability required to perceive pitch accent, lexical tone, and intonational patterns in human speech (Brosch *et al*. 2004). However, animals are generally more sensitive to absolute pitch than they are to relative pitch; the opposite is true for humans (see Patel 2008).

Another way of approaching the question of whether animals can group sensory stimuli in ways that are relevant to phonology is to see whether their own vocalizations contain internal structure. The organization of bird song is particularly clear, though it is not obvious exactly whether/how analogies to human language should be made. Yip (2006a) discusses how zebra finch songs are structured, building on work by Doupe & Kuhl (1999) and others. The songs of many passerine songbirds consist of a sequence of one to three notes (or 'songemes' as Coen (2006) calls them) arranged into a 'syllable'. The syllables, which can be up to one second in length, are organized into motifs which Yip considers to be equivalent to prosodic words but others equate with phrases, and there are multiple motifs within a single song. The structure can be represented graphically as follows, where M stands for motif, σ stands for syllable, and n stands for note (modified from Yip 2006a):

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structure, and are therefore not created by the phonological system alone. As an anonymous reviewer notes, this type of recursive structure is also quite different from the type found in syntax (for example, sentential embedding) which is limited in its depth only by performance factors.

(3)
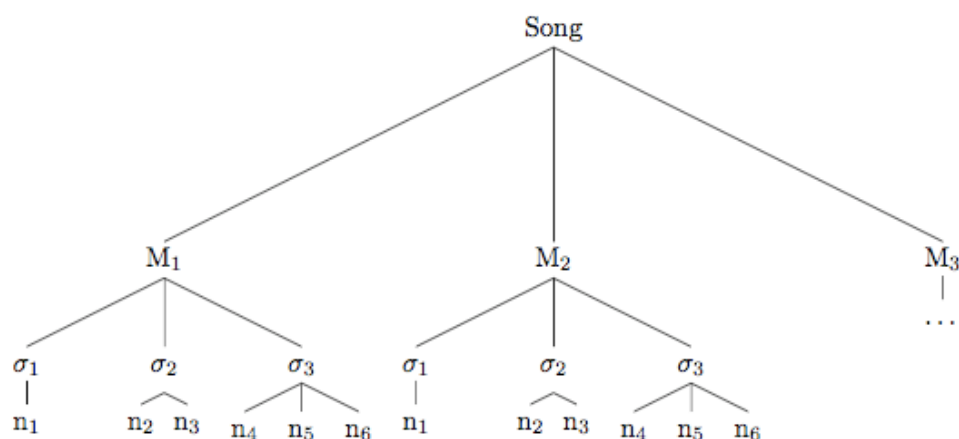
There are a few important differences between this birdsong structure and those found in human phonology, some of which are not apparent from the diagram. First, as Yip points out, there is no evidence for binary branching in this structure, which suggests that the combinatory mechanism used by birds cannot be equated with binary Merge, but it could be more along the lines of adjunction or concatenation, which creates a flat structure; see section 6 and Samuels & Boeckx (2009). Second, the definition of a 'syllable' in birdsong is a series of notes/songemes bordered by silence (Williams & Staples 1992, Coen 2006). This is very unlike syllables, or indeed any other phonological categories, in human language. Third, the examples from numerous species in Slater (2000) show that the motif is typically a domain of repetition (as I have represented it above); the shape of a song is $((a^x)(b^y)(c^z))^w$ with a string of syllables *a, b, c* repeated in order. This is quite reminiscent of reduplication. Payne (2000) shows that virtually the same can be said of humpback whale songs, which take the shape $(a \ldots n)^w$, where the number of repeated components, n, can be up to around ten.

Both birdsong and whalesong structures are 'flat' (in the sense of Neeleman & van de Koot 2006) or 'linearly hierarchical' (in the sense of Cheney & Seyfarth 2007) — they have a depth of embedding which is limited to a one-dimensional string which as been delimited intro groups, as in (4) — exactly what I argue in section 6 and in Samuels (2009a) for human phonology. It is interesting to note in conjunction with this observation that baboon social knowledge is of exactly this type, as Cheney & Seyfarth have described. Baboons within a single tribe (of up to about eighty individuals) obey a strict, transitive dominance hierarchy. But this hierarchy is divided by matrilines; individuals from a single matriline occupy adjacent spots in the hierarchy, with mothers, daughters, and sisters from the matriline next to one another. So an abstract representation of their linear dominance hierarchy would look something like this, with each *x* representing an individual and parentheses defining matrilines:

(4)   (xxx)(xx)(xxxx)(xxx)(xxxxxxx)(xxx)(x)(xxxx)

The difference between the baboon social hierarchy and birdsong, which I translate into this sort of notation below, is merely the repetition which creates a

motif (think of baboon individuals as corresponding to songemes and matrilinesas corresponding to syllables):

(5)

$$\underbrace{(n_1)(n_2 n_3)(n_4 n_5 n_6)}_{\text{motif}_1} \overbrace{(n_1)(n_2 n_3)(n_4 n_5 n_6)}^{\text{motif}_2}$$

There is evidence to suggest that, as in phonology (but strikingly unlike narrow syntax), the amount of hierarchy capable of being represented by animals is quite limited. In the wild, apes and monkeys very seldom spontaneously perform actions which are hierarchically structured with sub-goals and sub-routines, and this is true even when attempts are made to train them to do so. Byrne (2007) notes one notable exception, namely the food processing techniques of gorillas. Byrne provides a flow chart detailing a routine, complete with several decision points and optional steps, which mountain gorillas use to harvest and eat nettle leaves. This routine comprises a minimum of five steps, and Byrne reports that the routines used to process other foods are of similar complexity. Byrne further notes that "all genera of great apes acquire feeding skills that are flexible and have *syntax-like* organisation, with hierarchical structure. […] Perhaps, then, the precursors of linguistic syntax should be sought in primate *manual* abilities rather than in their vocal skills" (Byrne 2007: 12; emphasis his). I concur that manual routines provide an interesting source of *comparanda* for the syntax of human language, broadly construed (i.e., including the syntax of phonology). Fujita (2007) has suggested along these lines the possibility that Merge evolved from an 'action grammar' of the type which would underlie apes' foraging routines.

Other experiments suggest that non-human primates may be limited in the complexity of their routines in interesting ways. For example, Johnson–Pynn *et al.* (1999) used bonobos, capuchin monkeys, and chimpanzees in a study similar to one done on human children by Greenfield *et al.* (1972) (see also discussion of these two studies by Conway & Christiansen 2001). These experiments investigated how the subjects manipulated a set of three nesting cups (call them A, B, C in increasing order of size). The subjects' actions were categorized as belonging to the 'pairing,' 'pot,' or 'subassembly' strategies, which exhibit varying degrees of embedding:[5]

(6)   a.   *Pairing strategy*: place cup B into cup C. Ignore cup A.
       b.   *Pot strategy*: first, place cup B into cup C. Then place cup A into cup B.
       c.   *Subassembly strategy*: first, place cup A into cup B. Then place cup B into cup C.

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[5]   The situation is actually substantially more complicated than this, because the subjects need not put the cups in the nesting order. To give a couple examples, putting cup A into cup C counts as the pairing strategy; putting cup A into cup C and then placing cup B on top counts as the pot strategy. I refer the reader to the original studies for explanations of each possible scenario. The differences between the strategies as I have described them in the main text suffice for present purposes.

The pairing strategy is the simplest, requiring only a single step. This was the predominant strategy for human children up to twelve months of age, and for all the other primates — but the capuchins required watching the human model play with the cups before they produced even this kind of combination. The pot strategy requires two steps, but it is simpler than the subassembly strategy in that the latter, but not the former, requires treating the combination of cups A + B as a unit in the second step. (We might consider the construction of the A + B unit as being parallel to how complex specifiers and adjuncts are composed 'in a separate derivational workspace' in the syntax; see Fujita 2007.) Human children use the pot strategy as early as eleven months (the youngest age tested) and begin to incorporate the subassembly strategy at about twenty months. In stark contrast, the non-human primates continued to prefer the pairing strategy, and when they stacked all three cups, they still relied on the pot strategy even though the experimenter demonstrated only the subassembly strategy for them. Though we should be careful not to discount the possibility that different experimental methodologies or the laboratory context is responsible for the non-humans' performance, rather than genuine cognitive limitations, the results are consistent with the hypothesis that humans have the ability to represent deeper hierarchies than other primates. This is, of course, what we predict if only humans are endowed with the recursive engine that allows for infinite syntactic embedding (Hauser *et al*. 2002a).

Many other types of experimental studies have also been used to investigate how animals group objects. It is well known that a wide variety of animals, including rhesus monkeys, have the ability to perform comparisons of analog magnitude with small numbers (<4). They can discriminate between, for instance, groups of two and three objects, and pick the group with more objects in it. As Hauser *et al*. (2000) note, such tasks require the animal to group the objects into distinct sets, then compare the cardinality of those sets. Further data comes from Schusterman & Kastak (1993), who taught a California sea lion named Rio to associate arbitrary visual stimuli (cards with silhouettes of various objects printed on them). On the basis of being taught to select card B when presented with card A, and also to select card C when presented with card B, Rio transitively learned the A-C association.[6] Rio also made symmetric associations: when presented with B, she would select A, and so forth. We might consider these groups Rio learned to be akin to learning arbitrary pairings such as which phonemes participate in a given alternation (A and C bear the same relation to B), or in which contexts a particular process occurs (choose A in the context of B; choose B in the context of C).

The concept of 'natural classes' has also been studied in animals to a certain degree, though not in those terms. We can think of natural classes as multiple ways of grouping the same objects into sets according to their different properties (i.e., features). Alex the parrot had this skill: He could sort objects by color, shape, or material (reported by his trainer in Smith 1999). As regards the ability to group

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[6]   See also Addessi *et al*. (2008) on transitive symbolic representation in capuchin monkeys, and Cheney & Seyfarth (2007) on transitive inference involving social hierarchy in baboons. Cheney & Seyfarth also discuss both transitive social dominance and learning of symbolic representations in pinyon jays.

objects, then, I conclude that animals — especially birds and primates — are capable of the basic grouping abilities which phonology requires. They perceive (some) sounds categorically like we do; their vocalizations show linearly hierarchical groupings like ours; they can assign objects arbitrarily to sets like we do; they can categorize objects into overlapping sets according to different attributes like we do. Their main limitations seem to be in the area of higher-degree embedding, but this is (i) at best, a property of phonology which arises because of recursion in syntax, not from a recursive engine within phonology (or, if Samuels 2009a is correct to eliminate the prosodic hierarchy, not a property of phonology at all) and (ii) an expected result if, as Hauser *et al*. (2002a) hypothesize, recursion is a part of FLN and therefore not shared with other species.[7]

4.   Patterns

The next set of abilities we will consider are those which deal with extracting patterns from a data stream and/or learning arbitrary associations. As I mentioned in the previous section, I view pattern-detection as the flipside of grouping: A pattern is essentially a relation between multiple groups, or different objects within the same group. Thus, the ability to assign objects to a set or an equivalence class is a prerequisite for finding any patterns in which those objects participate; the abilities discussed in the previous section are very much relevant to this one as well.

Several experimental studies on animal cognition more generally bear on the issue of abstract pattern learning. One such study, undertaken by Hauser *et al*. (2002b), tested whether tamarins could extract simple patterns ('algebraic rules') like same–different–different (ABB) or same–same–different (AAB) from a speech stream. They performed an experiment very similar to one run on infants by Marcus *et al*. (1999). The auditory stimuli in both of these studies were of the form $C_1V_1C_1V_1C_2V_2$ (the AAB condition) or $C_1V_1C_2V_2C_2V_2$ (the ABB condition), such as *li–li–wi* or *le–we–we*. After habituating the infants/tamarins to one of these conditions, they tested them on two novel test items: one from the same class to which they had been habituated, and a second from the other class. The item with a different pattern than the habituated class should provoke a dishabituation response if the subjects succeed in learning the appropriate generalization based on the pattern in the stimuli presented during the training phase. Both infants and tamarins evidenced learning of these simple patterns; they were more likely to dishabituate to the item with the new pattern.

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[7]   A reviewer asks whether this implies animals have 'a little' recursion, and what that would even mean. I view the situation as an exact parallel to the difference between humans and animals in the domain of numerical cognition; perhaps the two dichotomies are indeed manifestations of the same cognitive difference, namely that only humans have a recursive engine (Merge), as suggested by Hauser *et al*. (2002a). While many animals (and young human children) seem to be able to represent small numerals, only suitably mature (and, perhaps, suitably linguistic) humans go on to learn the inductive principle, which allows them to count infinitely high. See discussion later in this section and section 5 for more discussion and references on numeracy in animals.

This type of pattern-extraction ability could serve phonology in several ways, such as the learning of phonological rules or phonotactic generalizations. Heinz (2007) showed that phonotactics (restrictions on the co-occurrence of segments, such as at the beginnings or ends of words) can be captured without any exceptions if three segments at a time are taken into account, so it seems on the basis of tamarins' success in the Hauser *et al.* experiment that learning phonotactics would not be out of their range of capabilities (though as we will soon see, tamarins may have independent problems with consonantal sounds that would interfere with this potential). Furthermore, phonotactics (and all attested phonological rules) can be modeled with finite-state grammars, as has been known since Johnson (1970). Here the somewhat controversial findings of Fitch & Hauser (2004) may also be relevant. At least under one interpretation of the data obtained by Fitch & Hauser, tamarins succeed at learning finite-state grammars but fail to learn more complicated phrase-structure grammars. If we accept these conclusions, then in theory — problems with consonants notwithstanding — we would expect that tamarins could learn any attested phonotactic restriction or phonological rule.

One of the most important obstacles facing a language learner/user falls into the category of pattern-extraction. This difficult task is parsing the continuous speech stream into discrete units (be they phrases, words, syllables, or segments). This speaks directly to (2b–c). Obviously, segmenting speech requires some mechanism for detecting the edges of these units. Since the 1950s, it has been recognized that one way to detect the edges of words is to track transitional probabilities, usually between syllables. If Pr(AB) is the probability of syllable B following syllable A, and P(A) is the frequency of A, then the transitional probability between A and B can be represented as:

(7) $$TP(A \rightarrow B) = \frac{Pr(AB)}{Pr(A)}$$

The transitional probabilities within words are typically greater than those across word boundaries, so the task of finding word boundaries reduces to finding the local minima in the transitional probabilities. Numerous experimental studies suggest that infants do in fact utilize this strategy (among others) to help them parse the speech stream, and that statistical learning is not unique to the linguistic domain but is also utilized in other areas of cognition (see references in Gambell & Yang (2005)). With respect to the availability of this strategy in non-humans, Hauser *et al.* (2001) found that tamarins are able to segment a continuous stream of speech into three-syllable CVCVCV 'words' based solely on the transitional probabilities between the syllables. Rats are also sensitive to local minima in transitional probabilities (Toro *et al.* 2005).

While transitional probabilities between syllables are strictly local calculations (i.e., they involve adjacent units), some phonological (and syntactic) dependencies are non-adjacent. This is the case with vowel harmony, for instance, and is also relevant to languages with 'templatic' morphology, such as Arabic, in which a triconsonantal root is meshed with a different group of vowels depending on the part of speech which the root instantiates in a particular context. Comparing the results obtained by Newport & Aslin (2004) and

Newport *et al.* (2004) provides an extremely interesting contrast between human and tamarin learning of such patterns. Newport *et al.* tested adult humans and cotton-top tamarins on learning artificial languages, all with three-syllable CVCVCV words, involving the three different kinds of non-adjacent dependencies which I list below.

(8)   a.   *Non-adjacent syllables*: the third syllable of each word was predictable on the basis of the first, but the second syllable varied.
      b.   *Non-adjacent consonants*: The second and third consonants of each word were predictable on the basis of the first, but the vowels varied.
      c.   *Non-adjacent vowels*: The second and third vowels of each word were predictable on the basis of the first, but the consonants varied.

Both humans and tamarins succeeded at learning the languages tested in the non-adjacent vowel condition. Humans also succeeded at the non-adjacent consonant condition. These results are expected, at least for the humans, because both of these types of dependencies are attested in natural language (in the guises of vowel harmony and templatic morphology, as already noted). Tamarins failed in the non-adjacent consonant condition, though this does not cast aspersions on the fact that they were able to learn non-adjacent dependencies; rather, it suggests that they have the cognitive capability needed to create the appropriate representations, but they might have difficulty distinguishing consonant sounds. In other words, their failure may not be due to the pattern-detection mechanism, but rather due to the input which was available to that mechanism. This interpretation is supported by the fact that tamarins succeeded at establishing dependencies between non-adjacent syllables.

From a phonological perspective, perhaps the most intriguing result is that humans failed at this non-adjacent syllable condition. Newport *et al.* (2004: 111) ask:

> Why should non-adjacency — particularly syllable non-adjacency — be difficult for human listeners and relatively easy for tamarin monkeys? […T]his is not likely to be because tamarins are in general more cognitively capable than adult humans. It must therefore be because human speech is processed in a different way by humans than by tamarins, and particularly in such a way that the computation of non-adjacent syllable regularities becomes more complex for human adults.

They go on to suggest that perhaps the syllable level is only indirectly accessible to humans because we primarily process speech in terms of segments (whereas tamarins process it in more holistic, longer chunks).[8] This is a possible contributor to the observed effect, but other explanations are available. I will propose one here.

What I would like to suggest is that, in effect, tamarins fail to exhibit a

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[8]   Alternatively, Newport *et al.* suggest, it could be that tamarins' shorter attention span reduces the amount of speech that they process at a given time; this would restrict their hypothesis space, making the detection of the syllable pattern easier. It is not obvious to me how this explains the tamarins' pattern of performance across tasks, however.

minimality effect.[9] Let us interpret the tamarins' performance in the non-adjacent consonant condition as suggesting, as I did above, that they either (for whatever reason) ignore or simply do not perceive consonants. Then for them, the non-adjacent syllable task differs minimally from the non-adjacent vowel task in that the former involves learning a pattern which skips the middle vowel. So rather than paying attention to co-occurrences between adjacent vowels, they have to look at co-occurrences between vowels which are one away from each other. It seems likely, as Newport *et al*. also suggest, that the adjacent vs. one-away difference represents only a small increase in cognitive demand. But for us, the non-adjacent syllable condition is crucially different — and this is true no matter whether we are actually paying attention to syllables, consonants, or vowels. These categories have no import for tamarins, but for humans, they are special. The dependency we seek in this condition is between two non-adjacent elements of the same category, which are separated by another instance of the same category. This is a classical minimality effect: if α, β, γ are of the same category and $\alpha \succ \beta \succ \gamma$ ($\succ$ should be read for phonology as 'precedes' and for syntax, 'c-commands'), then no relationship between α and γ may be established. This restriction is captured straightforwardly if the way linguistic dependencies are established (be that dependency an instance of Agree, harmony, or whatever else) is established by means of a search procedure which scans from α segment by segment until it finds another instance of the same type (i.e., β), then stops and proceeds no further. If I am on the right track, then perhaps tamarins succeed where humans fail because their search mechanism does not work this way — which would be odd if minimality/locality restrictions arise from third-factor principles such as efficiency of computation — or more likely, that they do not represent the portions of the stimuli which they track as all belonging to the same abstract category of 'vowel' which is sufficient to trigger minimality effects for us.

A variety of other studies on primate cognition focus on the ability to learn sequences. Given that sequencing or precedence relationships are extremely important to language, particularly given the Minimalist emphasis on Merge in syntax and my parallel emphasis on concatenate in phonology, these studies are quite intriguing from a linguist's perspective. One apparent cognitive limitation of non-human primates relative to our species in the domain of pattern-learning is that they have extreme difficulty with non-monotonic sequences. Conway & Christiansen (2001) report on a number of studies which compare primates' performances on this kind of task. When presented with an 'artificial fruit' requiring four arbitrary actions to open it and thereby reveal a treat, chimpanzees and human preschoolers perform similarly; both succeed at learning the sequence.

However, another study highlights what seems to be a difference in the way humans and other primates plan and perform sequential actions. One

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9   Such effects have been discussed in terms of Relativized Minimality (Rizzi 1990) or the Minimal Link Condition (Chomsky 2000, 2004) in syntax and the No Line-Crossing Constraint (Goldsmith 1976) in auto-segmental phonology. I argue minimality in phonology and syntax emerges from the same underlying cause: A directional search mechanism which traverses strings of segments (see Mailhot & Reiss 2007, Samuels 2009a).

experiment undertaken by Ohshiba (1997) tested human adults, Japanese monkeys, and a chimpanzee on the ability to learn an arbitrary pattern: They were presented with a touch screen with four different-sized colored circles on it and had to touch each one in sequence to receive a reward; the circles disappeared when touched. All the species succeeded in learning a monotonic pattern: touch the circles in order from smallest to largest or largest to smallest. They also all succeeded, but were slower, at learning non-monotonic patterns.[10] But as we will discuss in section 5, measurements of reaction times suggest the humans and monkeys used different strategies in planning which circles to touch.

Rhythm, too, is a type of pattern. Rhythmicity, cyclicity, and contrast are pervasive properties of language, particularly in phonology. Everything that has been attributed to the Obligatory Contour Principle (Leben 1973) fits into this category. Walter (2007) argues that these effects should be described not with a constraint against repetition (see also Reiss 2008), but as emerging from two major physical limitations: the difficulty of repeating a particular gesture in rapid succession, and the difficulty of perceiving similar sounds (or other sensory stimuli) distinctly in rapid succession. These are both extremely general properties of articulatory and perceptual systems which we have no reason to expect would be unique to language or to humans.

To date, perhaps the most direct cross-species test of the perception of human speech rhythm (prosody) comes from Ramus *et al.* (2000). In Ramus *et al.*'s experiment, human infants and cotton-top tamarins were tested on their ability to discriminate between Dutch and Japanese sentences under a number of conditions: one in which the sentences were played forward, one in which the sentences were played backward, and one in which the sentences were synthesized such that the phonemic inventory in each language was reduced to /s a l t n j/. The results of these experiments showed that both tamarins and human newborns were able to discriminate between these two unfamiliar and prosodically different languages in the forward-speech condition, but not in the backward-speech condition. A generous interpretation of these results would suggest "at least some aspects of human speech perception may have built upon preexisting sensitivities of the primate auditory system" (Ramus *et al.* 2000: 351). However, Werker & Voloumanos (2000) caution that we cannot conclude much about the processing mechanisms which serve these discrimination abilities; this is of particular concern given that the tamarins' ability to tell Dutch and Japanese apart was reduced in the reduced phonemic inventory condition. This may indicate that tamarins rely more strongly on phonetic cues rather than prosodic ones. Given the apparent importance of prosody for syntactic acquisition in human children — specifically, babies seem to use prosodic information to help them set the head parameter — Kitahara (2003: 38) puts forth the idea that

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[10]   In some situations, non-human primates fail entirely at learning non-monotonic patterns. For example, Brannon & Terrace (1998, 2000) found that while rhesus macaques taught the first four steps in a monotonic pattern could spontaneously generalize to later steps, they failed to learn a four-member non-monotonic pattern even with extensive training. It is not clear what to attribute the worse performance in the Brannon & Terrace studies to; there are too many differences between the paradigm they used and the one reported in the main text, including the species tested.

"cotton-top tamarins fail to discriminate languages on the basis of their prosody alone, because syntactic resources that require such prosodic-sensitive system [sic] might not have evolved for them." Though it is unclear how one might either support or disprove such a hypothesis, it is at the very least interesting to consider what prosody might mean for an animal which does not have the syntactic representations from which prosodic representations are built.

Another example of rhythmicity in speech is the wavelike sonority profile of our utterances, which is typically discussed in terms of syllable organization. Syllables range widely in shape across languages. In (9)–(10) I give examples from opposite ends of the spectrum: a series of three CV syllables in (9), and a syllable in (10) that has a branching onset as well as a coda, and additionally appendices on both ends. The relative heights of the segments in (9)–(10) represent an abstract scale of sonority (making no claim about the units of this scale).[11]

(9)

(10)
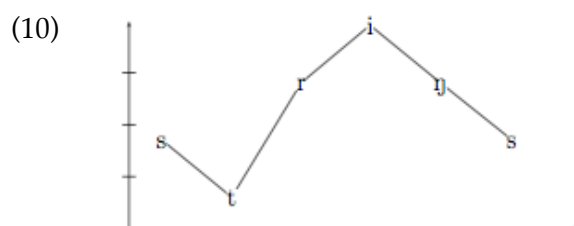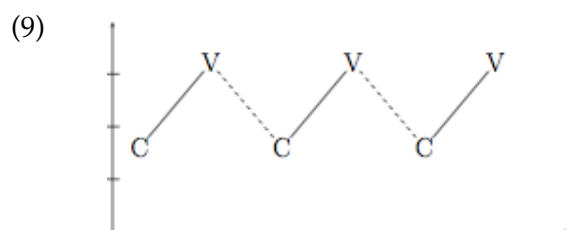
All syllables, from CV (9) to CCCVCC (10), combine to yield a sonority profile roughly as in (11):

(11)

The peaks and troughs may not be so evenly dispersed, and they may not all be of the same amplitudes, but the general shape is the same no matter whether the sonority values being plotted come from syllables that are CV, CVC, sCRV:CRs, and so forth, or any combination of these. This is hardly a new observation; it is over a century old (e.g., Lepsius & Whitney 1865, de Saussure

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[11]   I remain agnostic about the exact nature of sonority. However, see (among others) Ohala (1992) and Ohala & Kawasaki–Fukumori (1997) for arguments that it is a derived notion rather than a primitive one.

1916). Ohala& Kawasaki–Fukumori (1997: 356) point out that it is inevitable:

> Just by virtue of seeking detectable changes in the acoustic signal one would create as an epiphenomenon, i.e., automatically, a sequence showing local maxima and minima in vocal tract opening or loudness. In a similar way one could find 'peaks' (local maxima) in a string of random numbers as long as each succeeding number in the sequence was different from the preceding one.

I have suggested in previous work that the ability to break this wave up into periods (based partially on universal and partially on language-specific criteria) aids with the identification of word boundaries: they tend to fall at the local minima or maxima in the wave (Samuels 2009a: sect. 3.3). And as we saw earlier in this section, we already know that both human infants and tamarins are sensitive to local minima (of transitional probabilities) in speech, which I believe suggests that this is a legitimate possibility.[12]

Animals from a wide variety of clades show preferences for rhythmicity in their vocalizations and other behaviors as well, though it is important to note that our own (non-musical) speech has no regular beat; while language does have a rhythm, it is not a primitive (see discussion in Patel 2008). Yip (2006b) mentions that female crickets exhibit a preference for males who produce rhythmic calls, and Taylor *et al*. (2008) discovered that female frogs prefer rhythmic vocalizations as well. Rhythmic behaviors, or the ability to keep rhythm, appear to be widespread in the animal kingdom. Gibbons produce very rhythmic 'great calls,' and while Yip (2006b: 443) dismisses this, saying that "the illusion of rhythm is probably more related to breathing patterns than cognitive organization," this should hardly disqualify the data. For example, the periodic modulation of sonority in our speech is closely connected to opening and closing cycle of the jaw (Redford 1999, Redford *et al*. 2001), and it is widely accepted that the gradual downtrend in pitch which human utterances exhibit has to do with our breathing patterns. So for humans, too, there is at least some purely physiological component; however, the fact that females of various species prefer rhythmic calls shows that at the very least, there is also a cognitive component to animals' perception of rhythmicity.

There are also some animals which synchronize the rhythms produced by multiple individuals. For example, frogs, insects, and bonobos all synchronize their calls; some fireflies synchronize their flashing, and crabs synchronize their claw-waving (see Merker 2000 and references therein). However, while elephants can be taught to drum with better rhythmic regularity than human adults, they do not synchronize their drumming in an ensemble (Patel & Iversen 2006).

Finally, we should note that it is extremely common for animals to exhibit 'rule-governed' behavior in the wild, and in their communicative behavior in particular. Cheney & Seyfarth (2007) make the case that baboon vocalizations are

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[12]   In all of the studies on tamarins (and human infants) of which I am aware, the shape of syllables tested does not extend beyond CV. As a reviewer suggests, it would be most informative to see studies which test a variety of syllable shapes — but note that tamarins' difficulties with perceiving consonant sounds, as discussed earlier with regards to the Newport *et al*. (2004) experiments, would likely confound such investigations.

rule-governed in that they are directional and dependent on social standing. That is, a baboon will make different vocalizations to a higher-ranked member of the group than she will to a lower-ranked member. By this same rubric, vervet monkey grunts and chimpanzee calls should also be considered rule-governed; a number of articles on species ranging from treefrogs to dolphins to chickadees in a recent special issue of the *Journal of Comparative Psychology* (August 2008, vol. 122.3) devoted to animal vocalizations further cement this point. And as we saw in the previous section, both bird and whale songs obey certain combinatorial rules — in other words, they have some kind of syntax (in the broad sense of the term). Here the distinction made by Anderson (2004) and suggested in earlier work by Peter Marler is useful: Plenty of animals have a 'phonological' syntax to their vocalizations, but only humans have a 'semantic' or 'lexical' syntax which is compositional and recursive in terms of its meaning. Again, this reiterates Hauser *et al.*'s view that what is special about human language is the mapping from syntax to the interfaces (and particularly the LF interface, as Chomsky emphasizes in recent writings; see, e.g., Chomsky 2004), not the externalization system.

5.   Operations

The final set of abilities which we will discuss are those which pertain to the phonological operations for which I argue in Samuels (2009a): SEARCH and COPY.[13] While these operations enjoy an elevated status in my work, as we will see in the next section, it is important to note that *any* theory of phonology, or of language in general, will have to make use of these operations. For example, Hornstein (2001) argues that insertion of an element into a linguistic derivation is copying from the lexicon, and I would add that it is very difficult to see how this copying might be done without a prior search into the lexicon. So, in short, one may contest my view that search, copy, and delete are the *only* operations in phonology, but it should not be seen as controversial that they play *some* role within the module. I also discuss here a fourth operation, concatenation. By this I mean the ability to connect morphemes — a root and an affix, for example — in a manner that creates a linear structure, not the nested hierarchical structure of Merge.[14] This concatenation mechanism properly belongs to the syntax–phonology interface, but since it operates at a stage at which phonological material has already been added (see Idsardi & Raimy, in press), as we know since some affixes are sensitive to phonological properties such as the stress pattern of the stem, it is relevant to the present work.

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13   I have little to say about the third operation which I posit, DELETE, but nothing suggests to me that this should be considered a domain-specific or species-specific ability.

14   Whereas iterative applications of concatenate yield a flat structure, iterative applications of Merge yield a nested hierarchical structure: syntactic structures must be flattened, whereas linear order is a primitive in phonology (Raimy 2000). Also, since phonology lacks Merge, it also follows that it lacks movement, since movement is a sub-species of Merge (Internal Merge or Re-Merge, Chomsky 2004). Without the possibility of re-merging the same element, the notion of identity is extrinsic in phonology, unlike in syntax (see Raimy 2003). Samuels & Boeckx (2009) discuss this issue in greater detail.

Searching is ubiquitous in animal and human cognition. It is an integral part of foraging and hunting for food, to take but one example. The Ohshiba (1997) study of sequence-learning by monkeys, humans, and a chimpanzee is an excellent probe of searching abilities in primates because it shows that, while various species can perform the multiple sequential searches required to perform the experimental task (touching four symbols in an arbitrary order), they plan out the task in different ways. The humans were slow to touch the first circle but then touched the other three in rapid succession, as if they had planned the whole sequence before beginning their actions (the 'collective search' strategy). The monkeys, meanwhile, exhibited a gradual decrease in their reaction times. It was as if they planned only one step before executing it, then planned the next, and so forth (the 'serial search' strategy).

Perhaps most interestingly of all, the chimpanzee appeared to use the collective search strategy on monotonic patterns but the serial search strategy when the sequence was not monotonic. That chimpanzees employ collective searches is corroborated by the results of a similar experiment by Biro & Matsuzawa (1999). The chimp in this study, Ai, had extensive experience with numerals, and she was required to touch three numerals on a touch-screen in monotonic order. Again, her reaction times were consistently fast after the initial step. But when the locations of the two remaining numerals were changed after she touched the first one, her reactions slowed, as if she had initially planned all three steps but her preparation was foiled by the switch. It is not clear to me exactly what should be concluded from the disparity between humans, chimps, and monkeys, but notice that the SEARCH mechanism proposed by Mailhot & Reiss (2007) and extended by Samuels (2009a, 2009b) operates in a manner consistent with the collective search strategy: scan the search space to find all targets of the operation to be performed, and then perform the operation to all targets in one fell swoop.

A close parallel to the COPY operation in phonology, particularly the copying of a string of segments as in reduplication, would be the patterns found in bird and whale songs. As we saw in section 3, Slater (2000) shows that for many bird species, songs take the shape $((a^x)(b^y)(c^z))^w$: That is, a string of syllables *a*, *b*, *c*, each of them repeated, and then the whole string repeated. We also saw that whale songs are similarly structured (Payne 2000). With respect to the copying of a feature from one segment to another (as in assimilatory processes), the relevant ability might be transferring a representation from long-term memory to short-term memory: extracting a feature from a lexical representation and bringing it into the active phonological workspace. This seems like a pre-requisite for any task which involves the recall/use of memorized information, and perhaps can be seen as a virtual conceptual necessity arising from computational efficiency (a prime source of third-factor explanation; see Chomsky 2005, 2007).[15]

As I mentioned in the previous two sections, concatenation serves both the

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[15]   If we think of copying as including imitative or mimicking behaviors, then this, too, is a very common ability. However, as Hauser (1996) stresses, monkeys and apes are not very strong vocal learners, as opposed to songbirds and cetaceans, which are quite skilled in this area. Nevertheless, monkeys' learning is facilitated by watching a demonstration (Cheney & Seyfarth 2007), and Arbib (2005) argues that chimpanzees have the capacity for simple imitation that monkeys lack; humans have the capacity for complex imitation chimps lack.

ability to group and the ability to perform sequential actions. Without the ability to assign objects to sets or combine multiple steps into a larger routine, neither of these are possible. We have already seen that bird and whale songs have the kind of sequential organization which is indicative of concatenated chunks, and primates can perform multi-step actions with sub-goals.

I would like to suggest that concatenation may underlie the 'number sense' common to humans and many other species as well (for an overview, see Dehaene 1997, Lakoff & Nuñez 2001, Devlin 2005). This is perhaps clearest in the case of parallel individuation/tracking, or the ability to represent in memory a small number of discrete objects (< 4; see Hauser *et al.* 2000 and references therein). Shettleworth (1998) provides an overview of animal abilities in this domain, which have been shown for species as diverse as parrots and rats.

The idea that there is a connection between parallel individuation and concatenation is suggested by the fact that the speed of recognizing the number of objects in a scene decreases with each additional object that is presented within the range of capability (Saltzman & Garner 1948). This leads me to suspect, along with Gelman & Gallistel (1978) (but *contra* Dehaene) that such tasks require paying attention to each object in the array separately, albeit briefly. Lakoff & Nuñez (2001) also discuss a number of studies showing that chimpanzees (most notably Ai, whom we met previously as the subject of Biro & Matsuzawa's 1999 study), when given rigorous training over a long period of time, can engage in basic counting, addition, and subtraction of natural numbers up to about ten. These tasks clearly involve the assignment of (sometimes abstract symbolic) objects to sets, which is the fundamental basis of concatenation. Conversely, subtraction or removal of objects from a set could be seen as akin to the delete operation; the ability to subtract has also been shown in pigeons. This and a number of other studies showing that primates, rats, and birds can both count and add with a fair degree of precision are summarized in Gallistel & Gelman (2005).

6.   Approaching Phonology from Below

Now that we have seen an overview of animal abilities which seem to be relevant to phonological computation, I would like to take the next step and briefly describe how we might pursue a theory of phonology which employs virtually nothing besides these abilities plus the input given to phonology by (morpho-) syntax; the theory is laid out in detail in Samuels (2009a). This work is consistent with the 'bottom-up' approach to linguistic theory which is being pursued in syntactic circles. While more and more structure has been attributed to UG over the years, with the goal of reducing language acquisition to a manageable parameter-setting task for a child learner (i.e., taming Plato's Problem), this perspective has shifted with the advent of the Minimalist Program (Chomsky 1995; MP), and particularly in the recent Minimalist works, (e.g., Chomsky 2004, 2005, 2007, Boeckx 2006, *inter alia*). Rather than asking how much UG must include, Minimalists argue, we must now turn this question on its head:[16]

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[16]   In advocating for a slimmer UG, it may seem that Minimalists find their aims more aligned

> Throughout the modern history of generative grammar, the problem of determining the character of [the faculty of language] has been approached 'from top down': How much must be attributed to UG to account for language acquisition? The MP seeks to approach the problem 'from bottom up': How little can be attributed to UG while still accounting for the variety of I-languages attained [...]? (Chomsky 2007: 3)

Such a bottom-up approach to phonology is made possible by treating the phonological module as a system of abstract symbolic computation, divorced from phonetic content, pursuing the research agenda laid out by Hale & Reiss (2000a, 2000b). Along with Hale & Reiss and other 'substance-free' phonologists, I seek to investigate the universal core of formal properties that underlie all human phonological systems, regardless of the phonetic substance or indeed of the modality by which they are expressed. A major theme which I explore in recent work (Samuels 2009a, Samuels & Boeckx 2009) is that, while phonology and syntax may look similar on the surface — and this is not likely to be a coincidence — upon digging deeper, crucial differences between the two modules begin to emerge. One area where surface similarities hide striking differences is in the comparison between phonological syllables and syntactic phrases. Syllables and phrases have been equated by Levin (1985) and many others, with some going so far as to claim that phrase structure was exapted from syllable structure (Carstairs–McCarthy 1999). I argue these analogies are false, and that many of the properties commonly attributed to syllabic structure can be explained as well or better without positing innate structure supporting discrete syllables in the grammar. In Samuels (2009a: chap. 5) I move to eliminate the prosodic hierarchy as well, instead arguing that phonological phrasing is directly mapped from the phase structure of syntax (see also Kahnemuyipour 2004, Ishihara 2007). This means phonological representations are free to contain much less structure than has traditionally been assumed, and in fact that they are fundamentally 'flat' or 'linearly hierarchical.' Thus, the theory of phonology for which I argue has fewer groupings, and fewer chances for those groupings to exhibit recursion or hierarchy, than most other approaches. This is true at virtually every level, from the sub-segmental to the utterance: I posit no feature geometry; no sub-syllabic constituency; no bracketing of morphemes; no prosodic hierarchy. The illusion of hierarchy is created by the pervasive processes of chunking (recall section 3) and repeated concatenation (recall section 5):

(12) Concatenation   Chunking

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with those of neo-behaviorists/empiricists than was the case during earlier investigations in Principles–and–Parameters, as one reviewer points out. However, it is important to keep in mind that the driving force behind Minimalism (and the present work specifically) is not to deny that there is innate language faculty, but rather to search for the deep organizing principles of language whether they be specific to that faculty or not, and to present a theory which is consistent with the best current understanding of human evolution.

Still, nobody can deny the role of grouping/chunking in phonology: features group into segments, segments belong to natural classes on the basis of their featural composition, and segments group into longer strings such as syllables, morphemes, and phonological phrases. Of these last three types of groups, only the first is a truly phonological concept, since on my view phonology is a passive recipient of morphemes (strictly speaking, morpheme-level Spell-Out domains, which often but not always correspond to a single morpheme) and the chunks which correspond to phonological phrases (determined by the Spell-Out of phases common to narrow syntax, LF, and PF).[17]

I posit only three basic computational operations for phonology, as mentioned in the previous section:

(A)   SEARCH provides a means by which two elements in a phonological string may establish a probe-goal relation. The search algorithm, adapted from Mailhot & Reiss (2007), formalizes the system of simultaneous rule application proposed in Chomsky & Halle (1968: 344): "[T]o apply a rule, the entire string is first scanned for segments that satisfy the environmental constraints of the rule. After all such segments have been identified in the string, the changes required by the rule are applied simultaneously."

(B)   COPY takes a single feature value or bundle of feature values from the goal of a search application and copies these feature values (onto the probe of the search).

(C)   DELETE removes an element from the derivation.

If I am correct in positing such a spare set of phonological representations and operations, then the research presented in the previous sections of the present work strongly suggests that at least the rudiments of all of the abilities which underlie this minimalist theory of phonology are present in other animal species, and in domains outside of language: That is, phonology may belong entirely to FLB.

7.   Conclusions

I argue that the studies of animal cognition and behavior which I have presented here provide evidence that Pinker & Jackendoff's (2005) criticism of Hauser *et al.* (2002a) concerning phonology is unfounded, particularly if the theory of phonological representations and operations proposed in Samuels (2009a) is on

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[17]   Note that the model I assume is recursive in the sense that there are two types of Spell-Out domain, the morpheme-level and the clause-level, with the potential for several morpheme-level domains within a single clause-level one. However, these domains come directly from the narrow syntax, which is totally compatible with Hauser *et al.*'s hypothesis that syntax is the source — but crucially not the exclusive domain — of all recursive structures, and that once syntax is available, the modules with which it interfaces may be subject to modification.

the right track. Most conservatively, we can say that — *contra* Anderson (2004) and Yip (2006a, 2006b), we *have* tested for the building blocks of phonology in a wide range of species and found that they can group objects, extract patterns from sensory input, perform sequential objects, perform searches, engage in copying behaviors, and manipulate sets through concatenation. And more speculatively, we might tentatively conclude that, looking at the data we currently have, phonology provides little challenge to the idea that FLN is very small, perhaps consisting of just recursion or lexicalization and the mappings from syntax to the Conceptual-Intentional and Sensorimotor interfaces. This is most plausible if phonology is as conceived of in Samuels (2009a). The human phonological system would be, on this view, a domain-general solution to a domain-specific problem, namely the externalization of language. However, much research remains to be done in each and every one of the domains which I have discussed here, and I hope that the present work will be taken as an invitation to delve deeper and ask the more sophisticated questions which arise once we identify the basic points of potential consonance and divergence between human and animal cognition as far as phonology is concerned.

Another one of Pinker & Jackendoff's (2005) qualms with Hauser *et al.* — that the latter implicitly reject the popular hypothesis that 'speech is special' — should also be viewed skeptically. I do not deny the wide range of studies showing that speech and non-speech doubly dissociate in a number of ways which should be familiar to all linguists, as evidenced by aphasias, amusias, Specific Language Impairment, Williams Syndrome, autism, studies of speech and non-speech perception, and so on. Pinker & Jackendoff (2005) provide numerous references pointing to this conclusion, as does Patel (2008) with regards to language and music specifically (in this area the state of the art is changing rapidly, and the presence of a language/music dissociation is still an open and interesting question). But on the other hand, there is also a great deal of literature which shows that many species' vocalizations are processed in a different way from non-conspecific calls, or from sounds which were not produced by animals. This is true of rhesus macaques, who exhibit different neural activity — in areas including the analogs of human speech centers — and lateralization in response to conspecific calls (Gil da Costa *et al.* 2004). Perhaps we should amend the 'speech is special' hypothesis: speech is special (to us), in just the same way that conspecific properties throughout the animal kingdom often are; but there is nothing special about the way human speech is externalized or perceived in and of itself.

As a final note, consider the following set of characteristics which Seyfarth *et al.* (2005) ascribe to baboon social knowledge: it is representational, discretely-valued, linear-ordered, rule-governed, open-ended, modality-independent, combinatoric or concatenative, propositional, and linearly hierarchical. With the arguable exception of propositionality (though cf. Bromberger& Halle 2000 on phonemes as predicates), this describes phonology perfectly. How can we maintain in light of this that the core properties of phonological computation are unique to language or to us?

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