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On the grounding of syntax and the role of phonology in human cognition

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ABSTRACT

Chomskyan generative grammar has long been committed to the ‘double-interface’ assumption that the faculty of language (FL) serves two interfaces, PF and LF, and correlatively that expressions have phonological and semantic properties. The paper argues this gives rise to (a) a grounding problem for syntax – i.e. for the interpretable content of syntax – and (b) a problem for the assumption that FL is a generative computation. It is argued these problems are resolved if we think of syntax as grounded exclusively in semantic/conceptual properties. Since this implies that FL is phonology-free, it is argued that FL should not be distinguished from a generative computation describable as ‘the language of thought’ (LOT). The paper explores to what extent this (FL=LOT) thesis is consistent with Chomsky’s thinking. Chomsky’s recent work can be seen as pointing in that direction but it is not consistent with the double-interface assumption, which he continues to regard as conceptually necessary. In the light of discussion of the issues, the paper concludes with a speculation on the role of phonology in human cognition and its evolution.

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1. Introduction

This paper addresses a foundational question about syntax posed by the assumption that language has a ‘double interface property’ (henceforth ‘the DIP assumption’). It is intrinsic to the DIP assumption that linguistic expressions have both phonological and semantic properties. DIP is assumed to be conceptually necessary if we seek to reconstruct the traditional idea of ‘language as sound with a meaning’ (Chomsky, 1995:2; Berwick and Chomsky, 2008:7). The attribution of phonological properties to expressions addresses the sound side of things. The attribution of semantic properties is intended to address the meaning side.

I argue, against these DIP assumptions, that there is a single species of human syntax, an innate syntax exclusively grounded in and defined over semantic, conceptual, properties and that the faculty of language (FL) should therefore be regarded as not distinct from the language of thought (LOT). The paper asks to what extent these contentions are consistent with Chomskyan generative grammar (CGG) and even implied by the Minimalist Program. There is a deep tension here. I address this tension first by asking: What is the nature/content of syntax in CGG? What is syntax grounded in? What is ‘syntax’ the syntax OF?

2. The syntax grounding problem

As noted, within CGG, the overwhelming majority of lexical items, and all expressions generatively projected from them, are thought of as double-interface objects. They have properties interpretable at two interfaces, PF and LF – phonological and

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semantic. I argue this presents a grounding problem for syntax, a problem concerning the interpretable content of syntax. To illustrate what I mean by a 'syntactic grounding problem', I first discuss such a problem for Jackendoff's Parallel Architecture proposal (Jackendoff, 1997, 2002, 2003).

Jackendoff denies there can be representations/expressions that 'mix' phonological and semantic properties. He describes such mixes as 'formally incoherent'. For Jackendoff, a phonological property is a property of and only of a purely phonological representation; a semantic/conceptual property is a property of and only of a purely semantic/conceptual representation. Crucially (as far as the present paper is concerned), Jackendoff extends this idea to syntactic properties. He holds that representations that 'mix' syntactic properties with either semantic or phonological properties are also 'formally incoherent'. A syntactic property, for Jackendoff, is a property of a purely syntactic representation. This yields the 'representational modularity' of his Parallel Architecture proposal. Phonology, semantics and syntax are three separate, informationally encapsulated modules – three mutually independent 'languages of the mind' (Jackendoff, 1997:41).

In respect of syntax, Jackendoff's proposal encounters a grounding problem. Arguably, no such problem arises with either phonology or semantics, both of which are by their natures grounded. Notwithstanding the contentions of e.g. Anderson (1981) and Hale and Reiss (2000) – for discussion see Burton-Roberts et al. (2000) and Burton-Roberts (2000) – the null hypothesis in phonology is that phonological features have 'intrinsic phonetic content'. The content/grounding of phonology is phonetic: it lies in (the mental representation of) articulatory and/or acoustic properties (e.g. Ohala, 1998 and, among other contributions to Burton-Roberts et al., 2000; Pierrehumbert et al., 2000). Semantics, too, is grounded in and of itself. The intrinsic content and thus the grounding of semantics is conceptual. It is the stuff of thought. This much seems uncontroversial whatever view one has of concepts and their content.

The question is: What is an informationally encapsulated (modular) SYNTACTIC property? What is the content, the grounding, of syntax on Jackendoff's modular terms? It might be thought this is answered by reference to category and structure. But that can be only the start of an answer. Category and structure don't just exist *in vacuo*. They must themselves be grounded. What I mean is that a given category must be the category of something; a given structure must be the structure of something. Jackendoff's modularity claim poses the question: category and structure of what?

Consider Fig. 1, for example, adapted from Jackendoff (2003:659).

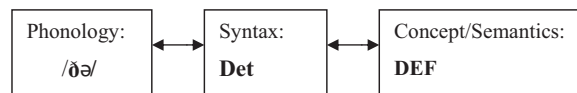


Fig. 1. Jackendoff's representational modularity, an example.

In Fig. 1, /ðə/ and DEF are each grounded, as just explained. But what of Det (for 'Determiner')? What is this purely syntactic object? Det is generally thought of as a syntactic category. But what is it the category of? Given Jackendoff's modularity (informational encapsulation), it can't be the syntactic category of /ðə/, for that would imply that the phonological expression /ðə/ actually belongs to, or has, a syntactic category. It implies that the content/grounding of Det is phonological. This would undermine Jackendoff's modularity claim, implying a syntactic-phonological 'mix'. In parallel, we can't assume that Det is the syntactic category of the concept DEF. That would imply that the content/grounding of Det is semantic. This 'mixes' syntax and semantics, again undermining modularity. Besides, even assuming DEF is an element of conceptual structure (which I'm inclined to doubt), it is hard to accept it has Det as its category – i.e. that Det is a category in conceptual structure. The more usual assumption concerning the relation between Det and DEF, I believe, goes the other way round: that DEF provides Det with its semantics. But this is ruled out by Jackendoff's modularity thesis for the same reason: it implies that the syntactic entity Det actually has a semantics and thus, again, that there are ('mixed') syntactic-semantic entities.

In short, if we take seriously Jackendoff's contention that syntax is an encapsulated module, syntax is *sui generis*. It has no content, is not grounded in anything. It can't involve category and structure as usually understood since, being informationally encapsulated, it does not attribute category or structure to anything. No answer is provided to the question: What is 'syntax' the syntax of?

Jackendoff's overarching contention, and his objection to CGG, is that the 'lexical items' assumed in CGG – putative objects with syntactic, phonological and semantic properties – don't exist. His contention is that, rather than dealing with OBJECTS ('items'), we are dealing simply with RELATIONS between separate objects. These relations (symmetric 'correspondences' for Jackendoff, captured by the arrows in Fig. 1) are entirely extrinsic to the objects related. On these terms, the only sense in which CGG's 'lexical items' can be said to exist is a purely abstract set-theoretical sense – they are mere sets, not real objects (not 'items').

I am fully in sympathy with Jackendoff's contention as far as phonology and semantics are concerned. For reasons explained below, I agree that phonological and semantic properties cannot 'mix' within a single object ('item'). What I have sought to show above, however, is that Jackendoff's modularity claim is problematic if we extend it to include syntax and assume, in consequence, that we are dealing with set-theoretical triples {PHON, SEM, SYN}. Syntax, if it is to have grounded content, cannot be regarded as just a further, independent, self-justifying, self-explanatory, member of a merely set-theoretical triple.

As noted, Jackendoff's proposal is a reaction against CGG in these matters. What then does CGG have to say about the grounding of syntax? First I suggest that CGG offers an apparently intuitive account of the grounding of syntax. Then I turn to problems posed by this account, particularly in Minimalism.

According to the double interface property (DIP) assumption of CGG, syntax is doubly grounded. It is grounded in both phonology and semantics, at least in the overwhelming majority of cases (for exceptions see below). Now, as argued, phonology and semantics are themselves each grounded, by their respective natures. Since grounding is a transitive relation, the grounding of syntax derives (is inherited) from that of phonology and that of semantics.

In CGG, then, lexical items are *SYNTACTIC* objects. Syntactic categories and structures are the categories and structures of double-interface objects – expressions constituted by a phonology and a semantics. Phonological and semantic features are thought of as *PROPERTIES* of syntactic objects. Alternatively, in this context, we can speak of ‘parts’ or ‘aspects’ of syntactic expressions: ‘there are sensorimotor systems that access one aspect of an expression and there are conceptual-intentional systems that access another aspect of an expression, which means that an expression has to have two kinds of symbolic objects as its parts’ (Chomsky, 2000:9). See Fig. 2.

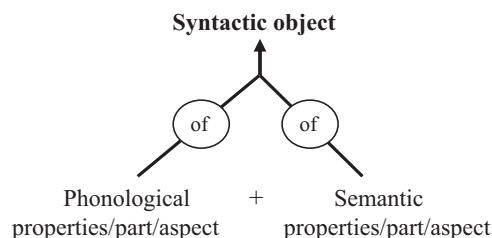


Fig. 2. The double grounding of syntax in CGG.

This amounts to an account of lexical items that is more than merely set-theoretical. Syntax, on these terms, is not just an additional, independent, self-explanatory member of a triple. What we have here is no mere set but a real *SYNTACTIC* object – doubly constituted by and grounded in phonological and semantic properties.¹

This picture of things reflects the centrality of syntax in CGG, the ‘syntactico-centrism’ of CGG that Jackendoff objects to. In CGG, the syntactic computation targets, and can only target, syntactic objects. Nevertheless, given the grounding of CGG’s syntactic objects, phonological and semantic features inevitably enter the syntactic computation because they are constitutive properties/parts/aspects of the objects that the computation manipulates. For the purposes of Merge, syntactic items are ‘atoms’. The syntactic computation projects the double-interface character of lexical items onto an array of more complex, syntactically structured expressions.²

Of course, not all syntactic expressions do have both phonological and semantic properties, i.e. not all are double-grounded. ‘Null’ (‘empty’) categories such as big *PRO* and little *pro* have semantic but not phonological properties. Conversely, complementiser *that*, and (on many accounts) expletive *it* and *there*, for example, have phonological but not semantic properties. It is significant, though, that no syntactic expression of CGG lacks both phonology and semantics. This is consistent with the above account of the grounding of syntax in CGG. The category and/or structure of an expression that lacked both phonology and semantics would not be the category or structure of anything. Such a ‘purely syntactic’ expression would be completely ungrounded, have no interpretable content.³

As compared with Jackendoff’s encapsulated syntax, the DIP assumption of CGG might seem to offer an intuitive conception of the content/grounding of syntax. But there is (or at least, I have) a problem here. On the one hand, I hold – effectively with CGG but, apparently, contra Jackendoff – that some account of the grounding/content of syntax is called for. On the other hand, I agree with Jackendoff in holding that it is, as he puts it, ‘formally incoherent’ to suppose that linguistic representations can ‘mix’ phonological and semantic properties.

My reason for agreeing with Jackendoff is this. Phonological properties and semantic properties are *SORTALLY* distinct. In illustration of what I mean by ‘sortally distinct’, take the properties prime and loud. Things of the sort that can be prime (i.e. numbers) are not the sort of thing that can be loud, and conversely. Sortally distinct properties are such that nothing can have both sorts of property.⁴ Nothing can be both loud and prime. It might be thought that phonological and semantic

¹ It is significant that Chomsky (e.g. 1995:219) refers to such objects as pairs, rather than triples, taking their syntactic nature for granted. It is also significant that he refrains from referring to them as ordered pairs. An ordered pair is licensed by the existence (or positing) of an antisymmetric relation, but notice that the relation between PHON and SEM in Fig. 2 is symmetric: they are co-properties, CO-parts or CO-aspects of the syntactic object.

Although Chomsky refers to pairs, I don’t think this can be taken to indicate he thinks of lexical items as merely set-theoretical. The important word here is ‘merely’. Pretty well anything can be thought of in set-theoretical terms if it suits our purpose, but that doesn’t mean that everything we think of in such terms is (nothing more than) a set.

² Despite claims made for them, ‘late (lexical) insertion’ models that go under the banner of Distributive Morphology don’t alter the general (DIP) assumption. See BR and Poole, 2006:617–8 for discussion.

³ Null expletives have been posited (see Sheehan, 2006: Chap. 5 for discussion) but they are radically inconsistent with minimalist principle. ‘The principles of UG involve only elements that can function at the interface levels’ (Chomsky, 1995:225).

⁴ In many cases, sortal incompatibilities are between the ‘abstract’ and the ‘physical’, but not always. Sneezes, epidemics, earthquakes, marriages, mortgages, years – none of these (‘physical’ or ‘abstract’) are of sorts that can be sky-blue, right-angled, taste of pineapple (all ‘physical’) or constitute a proof of Fermat’s last theorem (‘abstract’). I borrow ‘sortal’ from Thomason (1972). Incidentally, none of this is undermined by our preparedness to say that some book is green/heavy/on the top shelf and racist/heavy-going/illuminating, since that depends on the polysemy of *book* – intellectual content (x) vs. physical representation of such content (R(x)). $R(x) \neq x$. More on R – the relation of representation – below.

properties cannot be sortally incompatible on the grounds that they are both mentally constituted. However, given how phonology and semantics are grounded, their respective CONTENTS are sortally incompatible – articulatory/acoustic and conceptual-intentional respectively. It is this sortal incompatibility that underlies Saussurean arbitrariness. For this reason I hold, against CGG, that no real object can have both kinds of content/property, phonological and semantic. By ‘real object’ I mean anything more than a mere set. There is of course no objection to allowing that a relation may hold between (separate) objects that are sortally incompatible. Hence there is no objection to allowing that sets may have sortally incompatible members. What I am arguing is that there is at least one relation that CANNOT hold between sortally distinct objects, namely the co-property (co-argument, co-part) – i.e. mereological – relation countenanced in CGG, as in Fig. 2.

Whatever the grounding of syntactic objects, it cannot be BOTH phonological and semantic. There can be no ‘lexical items’ as conceived of in CGG. Jackendoff doesn’t put the matter in precisely these sortal terms (he in fact offers no gloss of ‘formally incoherent’), but I assume this is why he objects to representations that ‘mix’ those properties and that what he means by ‘formally incoherent’ is what I would mean by ‘sortally incoherent’.

Now, since CGG clearly does countenance mixed (PHON + SEM) syntactic entities, you might have supposed that this sortal kind of reasoning would/should be dismissed out of hand in CGG. Against this, it is an uncontroversial assumption of CGG that properties interpretable at the PF interface (phonological properties) are not interpretable at the LF interface and properties interpretable at the LF interface (semantic properties) are not interpretable at the PF interface. Arguably, CGG’s own insistence on the mutual un-interpretability of phonological and semantic properties amounts to an acknowledgement of the sortal incompatibility of phonological content and semantic content.

Consider CGG’s principle of Full Interpretation in the light of this. An LF representation is well-formed only if fully interpretable by the conceptual-intentional (CI) system, and hence does not include phonological information, which the CI system cannot use (such information is CI-incoherent). Correlatively, a PF representation is well-formed only if fully interpretable by the sensorimotor (SM) systems, and hence does not include semantic information, which the SM systems cannot use (such information is SM-incoherent). This poses a question. How is Full Interpretation at the interfaces to be reconciled with the DIP assumption that syntactic expressions are generally constituted as (PHON + SEM) ‘mixtures’? In what follows I explore CGG’s answer and its consequences.

It is lexical items that enter the syntactic computation in CGG. Constituted as atoms for the purposes of Merge, these syntactic objects necessarily pied-pipe their phonological and semantic properties into the computation. However, given the mutual un-interpretability of PF and LF properties, in order to satisfy Full Interpretation at the interfaces the computation must dismantle these objects into their component properties. This is effected at Spell Out (either once and for all, or ‘phase’ by ‘phase’). Spell Out splits apart the properties of the double-interface object, sending its phonological properties to be interpreted eventually at PF, and its semantic properties to be interpreted eventually at LF.⁵

But why should we assume that phonological and semantic properties could be constitutively conjoined as parts/aspects of lexical items in the first place, when it is acknowledged that such properties are mutually un-interpretability – in effect, sortally incompatible? The methodological correlate of that conceptual question is: why posit such double-interface objects in the first place, only to be faced with the necessity of an operation that splits them? Were phonology and semantics separate in the first place – as would seem necessary anyway given the acknowledged necessity of splitting them apart – there would be no need for such an operation. Its elimination should be indicated by minimalist principle.

But perhaps this talk of a splitting operation is by the by. The fact is that all and only the phonological properties of generated objects are interpreted at PF and all and only the semantic properties are interpreted at LF. Possibly, a literal splitting operation is unnecessary since the effect is achieved as an epiphenomenon of the Principle of Full Interpretation itself. Let’s accept that. Nevertheless, the interpretative problem remains and it has, I argue, two serious ramifications.

The first concerns WORDS (or morphemes, if it is they rather than words that are merged). From CGG’s enterprise of modelling ‘language as sound with a meaning’ by means of the DIP assumption, it follows that those lexical items that do have both sound and meaning – i.e. all words (bar complementiser *that* and expletive *it* and *there*, etc.) – are not interpretable at either of the interfaces. Take the word *cat*. It is a ‘sound with a meaning’. On the DIP assumption it is constituted as [/kæt/ +CAT]_N. But the phonology of *cat* (/kæt/) is not interpretable at LF and its semantics (CAT) is not interpretable at PF. As a consequence, their combination [/kæt/ +CAT] – the word itself, *cat* – is not interpretable at either interface. For Full Interpretation, the constitutive properties of every such un-interpretability double object must be dis-aggregated into two separate objects. But neither of these is the putative double object, the word itself. Having been dismantled for (or by) Full Interpretation, the word *cat* exists neither at LF nor at PF. In short, neither of the interfaces that the computation serves is capable of interpreting the ‘atoms’ it is supposed to manipulate.

The problem is brought into sharper focus in Minimalism. The minimalist contention is that LF and PF are THE ONLY ADMISSIBLE levels of representation (they being the only conceptually necessary levels). I am not here questioning that

⁵ The fact that a splitting operation is thought to be required supports my contention that ‘lexical items’ are thought of as real objects in CGG, not just sets. A set {x,y} is licensed by a RELATION of some kind holding between x and y, and there’s no reason to suppose that x and y, as mere set members, are not separate anyway. Hence, were it the case that CGG’s lexical items are simply sets (and it has been put to me that this is how they are thought of), there would be no motive for splitting them, i.e. for separating their already separate members.

A reminder here of what I am arguing and what I’m not. I am not arguing against some kind of RELATION (between PHON and SEM) in this context (quite the contrary). Hence I’m not arguing against a merely set-theoretical account. What I (with Jackendoff) am arguing against is CGG’s positing of real objects – ‘lexical items’ – doubly constituted by the terms related. It is noticeable that the precise nature of the relation between phonology and semantics is never discussed in CGG. I suggest this is because CGG takes it for granted that it is the co-constitutive (co-part, co-aspect) relation.

contention. However, we have seen that words – conceived of as double-interface entities – are not interpretable (in fact don't exist) at either interface. As a consequence, if LF and PF are the only admissible levels of representation, we have a conception of the linguistic computation that cannot admit of the existence of words. And, if it cannot admit of words, it cannot admit of anything composed of words.

The second ramification is yet more general. It is this that bears on the grounding of syntax within a DIP framework. Take category first. If syntactic categories are the categories of double-interface objects, which are not as such interpretable at either interface, it would seem that syntactic category itself is not interpretable at the interfaces. Indeed syntactic categories – understood in DIP terms – can't actually exist at either interface because the doubly grounded objects they are categories of don't exist at either interface. The same goes for syntactic structure. Given its double grounding, syntactic structure itself can't be interpretable, and indeed can't exist, at either of the two admissible levels of representation since the objects that such structures are supposed to be the structures of don't exist at the interfaces. In short, if syntactic category and structure are thought of as the category/structure of double-interface expressions and if the two interfaces PF and LF are the only levels of representation, then syntactic category and structure themselves are not admissible concepts.

These ramifications suggest something is seriously wrong. **Either (a)** the double grounding of syntax is not coherent; **or (b)** my account (Fig. 2) of the grounding of minimalist syntax is simply mistaken. My argument has been that (a) is the case. That calls for an alternative account of syntax and its grounding. I turn to that below. Here I consider (b) – though in doing so I want to show there is a tension in Minimalism.

As regards (b), then: on what grounds might I be regarded as mistaken about the double grounding of syntax in Minimalism? Consider Fig. 3:

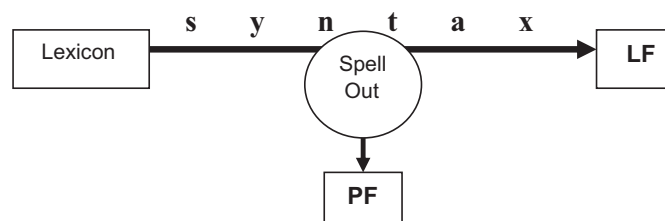


Fig. 3. The 'T' Model.

This 'T' model suggests, more explicitly than the 'Y' model did, that syntax (category and structure) persists in the computation to LF. It suggests more explicitly that there is one continuous syntactic computation that targets syntactic elements whether they have phonological properties or have lost them through Spell-Out. Syntactic properties must therefore be such as to survive the loss of phonology. This is contrary to the double grounding of syntactic properties I argued was assumed in CGG.

The problem with this is that, if syntactic category and structure are such as to survive Spell-Out (be it an operation or an epiphenomenon of Full Interpretation) – if syntactic properties persist after phonological features are stripped out – then phonological properties are *IRRELEVANT* to the nature, the content and grounding, of syntactic properties. What then is their nature? And why should – or could – phonological features enter the syntactic computation at all? Methodologically, why introduce phonological features into the syntactic computation only to be faced with the necessity of stripping them out?

The paradox is that these questions arise within the very framework that makes the inclusion of phonological features within the syntactic computation seem conceptually necessary. If those lexical items that are constituted in part by phonological features are merged by the syntactic computation, how could such features not enter the syntax? There is a tension here between the post-Spell-Out persistence of syntactic properties and (in consequence of the DIP assumption) the demand that phonological features enter the syntax.

3. The FL=LOT thesis

If syntactic properties coherently persist independently of phonology, phonology can have no business figuring in syntax. This contention, which I develop in what follows, chimes with Sigurðsson's (2004) arguments that 'language has innate structures that have meanings irrespective of whether or how they are expressed in Perceptible Form' and that '... we need to acknowledge that, in spite of being an extremely sophisticated motor system, the Phonological or Perceptible Form of oral languages is not part of Universal Grammar'.

If that is so, the computation must be just syntactico-semantic. Syntactic category and structure must be fully, exclusively, grounded in semantic properties. On this conception, syntax is_[df] the syntax-of-semantics.⁶ Syntactic structure

⁶ The more usual assumption has it the other way round: semantics is the semantics-of-syntax. (See above on DEF supplying Det with its semantics.) On this assumption, autonomous syntactic structure is 'first' generated – and the debt to PF discharged – and 'then' assigned a semantic interpretation or derivationally converted into a semantically interpretable object (notwithstanding the DIP assumption that items figuring in autonomous syntactic structures do have semantic properties in and of themselves).

just is the structure of conceptual entities and the categories over which such structure is defined (whatever those categories might be – on which see below). Syntax derives its content (grounding) from what it's the syntax of; the claim then is that syntax is only of-semantics; nothing else has syntactic 'form' as such and nothing else gives syntax 'content'.

All this suggests a view of the computation somewhat in the spirit of Fodor's 'language of thought' (LOT) – (e.g. 1975, 2008). I say 'somewhat' because Fodor thinks of the syntax of LOT as being quite different from syntax as conceived of in CCG, even assuming the latter were made phonology-free.⁷ Leaving that aside (i.e., however we conceive of LOT), Fodor's arguments for the existence of some system describable as 'LOT' appear to me compelling. And, notwithstanding disagreements on what 'semantics' precisely consists in, no-one who admits of LOT would want to deny that, if anything is, LOT is a locus of semantic properties. It is arguable more strongly, if only on grounds of parsimony, that LOT is THE (only) locus of semantic properties (Fodor, 1998:9, 2008:73; also Burton-Roberts, 2007 and the final two sections below). Furthermore, no-one who admits of LOT would want to deny that it has a generative syntax. A syntax-free (structure-less, recursion-free – and thus finite) semantics is inconceivable. Contrary to what Jackendoff appears to assume, it surely cannot be the case that syntactically structured semantic representations involve sortal 'mixing'; they cannot be 'formally incoherent'. Syntax derives its content from what it is the syntax of. Jackendoff himself admits of 'semantic/conceptual STRUCTURE'.⁸ His charge of 'formal incoherence' (interpreted in sortal terms) is certainly valid, I hold, but only in respect of the 'mixing' of phonological properties with syntactico-semantic properties.

LOT then is a generative computation. And, for those who accept such a thing (in whatever guise), LOT has a pre-eminent claim to be a generative computation that is natural, innate, and invariant across the species – and thereby a claim to be THE human generative computation. All of which is how the human faculty of language (FL) is conceived. FL, by hypothesis, is the invariant, uniform, natural, innate, generative, human computation. If only on (minimalist) grounds of parsimony, we must consider the possibility that CCG's 'faculty of language' is not distinct from the language of thought itself. Call it 'the FL=LOT thesis'.

In what follows I ask: How far off the mark is the FL=LOT thesis when it comes to Chomsky's own thinking? It could be argued, and there are indications enough, that in pursuing this thesis I'm pushing against an open door as far as recent minimalist thinking is concerned. But the DIP assumption is a fatal counter-indication. Here I explore some of the issues, in part by tracing (in section 4) certain themes in the history of the generative enterprise. The leitmotif here is what Chomsky refers to as 'the tension between descriptive and explanatory adequacy'. However, I will suggest that, as the generative enterprise has developed, the tension has evolved in a way that makes 'descriptive' vs. 'explanatory' an inappropriate way of characterising it.

Let me start by highlighting the significance of the earlier discussion of words for the FL=LOT thesis. A very salient putative difference between FL and LOT is that, whereas LOT clearly does not deal in words, FL is generally supposed to do exactly that. Against this, the earlier discussion gave reason, internal to FL-theory, to think that in fact FL itself cannot deal in words. And there is a further reason why FL can't deal in words – for the same reason that LOT can't. Words are, by definition, words of some – necessarily acquired – particular language.⁹ *Maradadi* is a Swahili, not a Polish, word. It is not plausible to suppose that any innate, universal system, whether thought of as FL or as LOT, could possibly deal in words. I develop this more generally below.

This brings us back to phonology (understood as including morphology). LOT does not deal in words because it has no phonology. It is the putative inclusion of phonology in FL that distinguishes FL from LOT. Continued adherence to the DIP assumption (the inclusion of phonology in the computation) thus argues clearly against the FL=LOT thesis.

Against this, as anyone familiar with his work knows, Chomsky has repeatedly deplored phonology as the fly in the ointment of perfection. For example:

(Q1) The whole phonological system looks like a huge imperfection, it has every bad property you can think of. (2002:118)¹⁰

⁷ Furthermore, I depart from Fodor's externalist/relational account of 'content', according to which 'content is constituted, exhaustively, by symbol-world relations' (1998:14). By 'conceptual content' I mean a (indeed the individuating) property of a concept. This is an internalist, constitutive notion of conceptual content – and nativist, implying that what you acquire is not a concept but a certain kind of ACCESS to a concept (from worldly and/or linguistic experience). More on access in the final section. Fodor himself allows that concept-world relations are determined by something 'mind-dependent'. This must have to do with the concept itself. 'Being a doorknob is just: striking our kinds of minds in the way that doorknobs do' (Fodor 1998:162) – which seems to me to allow, if not demand, that the concept DOORKNOB has some kind of internal constitutive property. This for me is its 'content', determining – non-arbitrarily – what external phenomena it 'locks onto' and thereby makes sense of. Concept-world relations (externalist, relational semantics) arise when a concept sufficiently complex to be entertained as a representation IS so entertained by an individual. I think this amounts to saying that the distinction-and-relation between internalist and externalist semantics is the distinction-and-relation between the language-of-thought and actual thoughts. See note 16 below for another respect in which I depart from Fodor.

⁸ But it depends on what Jackendoff means by 'syntax'. Possibly, Jackendoff contemplates two species of syntax, one of which 'mixes' happily with semantics (given 'semantic/conceptual structure' – in LOT) and the other not (given 'representational modularity' – in particular languages).

⁹ I'll use 'particular language' to refer indifferently to such as 'English', 'Swahili' etc. (E-languages) or I-languages (the languages of individuals), where that distinction is not germane to the discussion. It becomes relevant later.

¹⁰ This is an arresting statement. Phonology just is as it is and you might have thought that empirical language study should be such as to bite on that bullet. But it depends on what we mean by 'language (study)'. In section 4 I suggest that the problem lies, not in phonology, but in an instability in what exactly is meant by 'language' – the instability that has gone under the label of 'the tension between explanatory adequacy and descriptive adequacy'

Incidentally, Carr (e.g. 2006) offers a critique of attempts within phonological theory to make phonology appear less 'ugly' in the eyes of CCG.

Concomitantly, he has sought to downplay or sideline the role of phonology and the SM interface (PF) in the computation. Phonology is relegated to an ‘extraneous’ ‘periphery’ and thereby excluded from the ‘core systems of language’.

(Q2) ...language “imperfections” arise from the external requirement that the computational principles must adapt to the sensorimotor apparatus, which is in a certain sense “extraneous” to the core systems of language. ... (1995:265)

Equally, for Hauser et al. (2002), phonology belongs within a ‘broad’ rather than ‘narrow’ concept of language. But the theoretical substance of ‘broad’ vs. ‘narrow’ is far from clear. In a similar vein, Chomsky (2006) – see also Berwick and Chomsky (2008) – refers several times to a certain ‘primacy’ or ‘priority’ of the CI interface and to phonology and the SM interface as merely ‘ancillary’.

(Q3) We are assuming that FL provides at least instructions for the CI and SM interfaces, the former having priority (perhaps near-tautologically, insofar as the more radical thesis can be sustained). (2006:11)

The idea in Q2-3 is that there is an ‘asymmetry’ (see Q4) in the status/importance to be attached the CI and SM interfaces. But nothing in the conception of ‘language as sound with a meaning’ – or the DIP reconstruction of that conception that objects of the computation are PF-LF pairs – suggests any such asymmetry. Against the backcloth of those ideas, the asymmetry would seem to have the same status as the core-periphery distinction – in which connection Chomsky (1995:163 fn.3) has written: ‘The core-periphery distinction, in my view, should be regarded as an expository device, reflecting a level of understanding that should be superseded as clarification of the nature of linguistic inquiry advances’.

Insofar as an asymmetry can be contemplated here, it needs to be formulated in a more forthright manner, i.e. unqualified. Rather than refer a putative ‘primacy’ or ‘priority’ of the CI interface, we should say without qualification that the CI interface is THE (only) interface that the computation serves. It is the continued adherence to the DIP assumption, and hence the inclusion of phonology within the computation, that calls for qualifying reference to ‘primacy’ and Q3’s (logically questionable) ‘near tautologically’. In fact, were we to offer principled grounds for excluding phonology from the computation, thereby removing the stumbling block to the FL=LOT thesis, that would actually pre-empt all talk of an ‘INTERFACE’ between FL and LOT. It is the unqualified FL=LOT thesis, I urge, that would constitute the necessary ‘clarification of the nature of linguistic inquiry’.

‘The more radical thesis’ referred to in Q3 is Wolfram Hinzen’s (2006:179) methodological suggestion that ‘we should try deriving certain empirical properties of thought contents’ from the structures generated by FL (the structure of CP in particular) – see also Hinzen (2011). This is to the point – particularly if, as the discussion thus far has sought to establish, we should abandon the idea that FL needs to be distracted from its (‘core’) conceptual-intentional concerns by the need to serve the sensorimotor interface. Hinzen’s proposal, incidentally, provides the basis of a response to an apparently serious methodological objection to the FL=LOT thesis. The objection is that, while CGG has made substantial empirical discoveries about structure in FL, we know next to nothing about LOT structure as such and the prospects of remedying that look dim. I believe, looked at in the light of Hinzen’s proposal (and anyway), this objection seriously under-estimates the extent to which discoveries hitherto regarded as discoveries specifically about FL structure are in fact discoveries about LOT structure.

Assuming Hinzen’s proposal is consistent with the FL=LOT thesis, it implies that the computation must be purged of all CI un-interpretable properties and of any property that could be held responsible for the possibility of variation. (It is no accident that both classes of property include linear order, incidentally.) The purging will have to take the form of showing that all such properties are most accurately characterised in morpho/phonological terms. See Sigurðsson (2000) and Hinzen (2009).¹¹

Chomsky in recent work makes explicit reference to ‘a “language of thought”’:

(Q4) Generation of expressions to satisfy the semantic interface yields a ‘language of thought.’ If the assumption of asymmetry is correct, then the earliest stage of language would have been just that: a language of thought, used internally. It has been argued that an independent language of thought must be postulated. I think there are reasons for scepticism, but that would take us too far afield. (2006:9)

I interpret the last sentence of Q4 as expressing scepticism, not about the existence of LOT, but about the INDEPENDENT existence of LOT – in other words, whether LOT and FL are in fact independent. (Incidentally, a referee reports that Noam Chomsky (p.c.) confirms this interpretation.) This is the scepticism I am articulating in this paper.

¹¹ As a referee observes, morpho/phonology as traditionally conceived is simply not equipped to handle the range of phenomena that this proposal charges it with handling. So it calls for a reconceptualisation of morpho/phonology and its relation to syntax-semantics. But, as the referee also observes, so do recent proposals for off-loading from syntax to phonology, e.g., ‘PF movement’(Chomsky, 1995:368, 2001, Bošković, 2001); see BR and Poole (2006:602–3) for discussion. The head parameter is a case in point. Although traditionally regarded as syntactic, it seems clearly phonological (Berwick and Chomsky, 2008:8–9, 15). All that is syntactically relevant (and LF-interpretable) are the relations H~C and H~HP, definable irrespective of order. Order is thus arbitrary (and variable) with respect to the syntax. See section 7 for a sketch of my reconceptualisation of the role and scope of (morpho)phonology.

What I have sought to do thus far is point up the extent to which the FL=LOT thesis seems indicated but is in tension with, and undermined by, the DIP assumption that phonology necessarily figures in the generative computation. I now trace some history in an attempt to establish how this tension arose.

4. A brief history of 'language' in the generative enterprise

There was a time, in the early days of the generative enterprise (e.g. Chomsky, 1965), when the theory of 'language' was taken to be THE THEORY OF ALL PARTICULAR LANGUAGES. So conceived, it had a PLURALITY as its object of enquiry. 'Language' was to be understood unambiguously as a GENERIC term for that plurality. In 'language as sound with a meaning', 'language' can only be understood in that generic sense, for relevant sounds have meaning only within some particular (spoken) language. At that stage, particular languages were regarded as formal generative computations (as having or, more strictly, being grammars). This, I contend, is the origin of the idea that 'language' has double-interface generative architecture. If by 'language' we mean all particular languages ('language' in the generic sense) and if we assume that each of these is a formal generative system – then, since the phenomenon of 'sound with a meaning' (SWAM) has its locus in particular languages, it might well seem necessary to attribute a double-interface generative architecture to 'language'.

Consistent with this, UG was the theory of what all particular languages had in common. Each assumed object of enquiry – each particular language PL_i – was seen as posing two questions (a) 'What counts as PL_i ?', to be answered in terms of the goal of descriptive adequacy and (b) 'What makes PL_i count as a human language?', to be answered in terms of the goal of explanatory adequacy. Insofar as UG theory concerned a natural (innate) SINGULARITY – 'a real object of the natural world' as Chomsky later referred to it (1995:11), 'FL' as it is now known – it was regarded as concerned with a 'language acquisition device' (LAD). Again, the term 'language acquisition' has a generic orientation, pointing us in the direction of particular languages. Qua LAD, FL was a device for the acquisition of particular languages, regarded as generative computations. Thought of as a device for the acquisition of any particular generative computation/grammar (any 'language'), FL was NOT ITSELF a generative computation/grammar (or describable as a 'language' itself). Chomsky (1980:189) was explicit on this.

However, as is well known, the generative enterprise (and with it, I'm suggesting, the term 'language') has undergone a radical change over the years. Since at least Chomsky (1986), it has been questioned whether particular languages, understood as referring to socio-cultural constructs such as 'English' ('Swahili' etc.), could possibly be objects of naturalistic enquiry. Concomitantly, the enterprise of modelling them as formal generative computations was abandoned as impracticable and anyway conceptually misguided. Insofar as such things can even be said to exist in linguistically definable (i.e. formal, generative) terms, they are dismissed as 'E-language(s)', where 'E-' is to suggest 'external', both in the sense of existing as social constructs external to the individual mind and in the sense of existing as sets of utterances (even possible utterances). 'Some day ... the systems found in the world will not be regarded as languages in the strict sense, but as more complex systems, much less interesting for the study of human nature and human language. ... unhelpful for determining the real properties of the natural world' (Chomsky, 1991:51).

The focus shifted to 'I-language(s)', where 'I-' is to suggest 'internal' (independent of even potential external utterance phenomena) and 'individual' (independent of socio-cultural constructs and communities). But even in respect of I-languages, naturalistic scepticism is in order. An I-language is 'some accidental product of varied experience, of no particular interest in itself' (Chomsky, 1995:7); 'A deeper inquiry into I-languages will seek to account for the fact that Peter has the I-language L_p while Juan has the I-language L_j – these statements being high-level abstractions, because in reality what Peter and Juan have in their heads is about as interesting for naturalistic inquiry as the course of a feather on a windy day' (Chomsky, 2000:27).

In the light of this, naturalistic theory of language has shifted from being the theory of all particular languages and their acquisition – hitherto considered as formally definable generative computations – to being THE THEORY OF THE ONE HUMAN LANGUAGE considered ever more explicitly as the single, natural, formal generative computation itself. However, what has NOT shifted – and is in clear tension with this shift – is the characterisation 'language as sound with a meaning' (Chomsky, 1995:2; Berwick and Chomsky, 2008:7) and, with it, the DIP assumption.

This tension surfaces as a problem in Q3 above. Q3 assumes that the innate, natural faculty of language, FL, provides instructions for both the CI and SM interfaces. In other words, FL generates PF-LF pairs. This cannot be true. The idea of PF-LF pairs reconstructs 'sound with a meaning' (SWAM) and, as noted, SWAM is a phenomenon of and only of particular languages. 'SWAM' in this context alludes to speech – and speech is, by definition, speech-in-a-particular-language. Since pairings of sound and 'meaning' are non-natural (i.e. subject to Saussurean arbitrariness), they differ from one particular language to another. That is why particular languages are not given by the hand of nature but have to be acquired.

With the innate system regarded as a (the) generative computation itself, rather than as a device for the acquisition of a putative plurality of computations, it cannot be thought of as generating PF-LF pairs (for the same reason it can't be thought of as operating with words). Contrary to Q3, there is no SINGLE (invariant, universal) generative computation that provides paired instructions for the CI and SM interfaces. By the same token, no such computation could be NATURAL (innate). If a generative computation is one that generates objects of a certain kind then, as long as such objects are thought of as being PF-LF pairs, FL – which is by hypothesis invariant/universal and natural/innate – cannot be regarded as a generative computation.

By contrast, there is no conceptual objection (that I can see) to the proposition that FL is indeed a generative computation – and a natural, invariant one (free of Saussurean arbitrariness) – if what it generates are objects having just syntactico-semantic

properties (i.e. with syntax as the syntax-OF-semantics). If we seek to characterize FL as the single natural generative computation, then, we need principled grounds for excluding phonology from FL. This would obviate any distinction between FL and LOT. It is precisely LOT that generates objects having just syntactic-semantic properties.

As I have presented things, the conception of FL has shifted, on motivated naturalistic grounds, from FL conceived as a device for the acquisition of the plurality of all particular generative computations (FL-qua-LAD) to FL conceived of as the single, natural generative computation itself (FL-qua-LOT). Given its 'generic' orientation, the earlier conception (FL-qua-LAD) inevitably gave rise to the methodological tension (conflict) between descriptive and explanatory adequacy, a tension that intensified as the naturalistic character of the enterprise came ever more explicitly into focus. For, on that conception, enquiry was obliged to focus BOTH on the plurality of acquired particular languages in all their richness and diversity AND on the natural innate singularity that made possible their acquisition. In principle, seeing the shift as one from FL-qua-LAD to FL-qua-LOT obviates that tension. Given that perspective, there is no reason why these contrasting conceptions of 'FL' should be in contemporary tension. The one has superseded the other. But it does – explicitly and urgently – pose the problem of where particular languages (and SWAM) fit into the picture. If there is one human language (one generative computation), what ARE 'particular languages' and what is the nature of their RELATION to the one language? I turn again to these questions in the final two sections. It is perhaps because no new answer to these questions has been developed that the double-interface assumption has been retained despite its inconsistency with the later (FL-qua-LOT) conception. As a result, FL-qua-LAD vs. FL-qua-LOT is now itself the tension (yielding a pervasive ambiguity in the use of the term 'language').

5. FL-qua-LAD and FL-qua-LOT

Here I present an evolutionary consideration that counts against FL-qua-LAD and – if only by default – in favour of FL-qua-LOT.

If the cognitive rationale of FL is exhausted by the language acquisition function – i.e. if FL and LAD are one and the same and that's all there is to be said about FL – it seems to me we face an evolutionary circularity, both conceptually and empirically. Conceptually: if acquisition of a particular language is impossible without X, the evolution of X must have predated any particular language. But, in advance of there being any such a thing as a particular language, X – whatever it was – cannot have been a 'LAD' specifically. X must have had some rationale independent of particular languages and their acquisition. Empirically: while LAD, by definition, is necessary for language acquisition, it is not sufficient. Exposure to 'primary linguistic data' is necessary. But no such data would have been available at the point of the (necessarily prior) evolution of X.

This is an argument against FL-qua-LAD; it is not an argument against attributing to FL a necessary role in language acquisition. We could still attribute to FL a role in acquisition provided that role is no more than an incidental, epiphenomenal by-product of FL's primary rationale. The question then is: What was – and is – the primary cognitive rationale of FL? The thesis that FL=LOT provides a plausible answer. In fact, I can think of no other (more on this below). This indeed is what Q4 above seems to envision, taking an evolutionary perspective.

However – turning from the evolutionary to the developmental – I believe the FL=LOT thesis renders much less plausible the idea that FL is an 'initial state' that (ontogenetically) develops into, or is instantiated as, an individual's particular language ('final state'). For the combination of (a) the FL=LOT thesis and (b) the 'initial state' idea implies that it is LOT that develops into (is replaced by) an individual's particular language.¹² This amounts to saying that an individual's particular language IS her LOT. Quite apart from 'generic-ising' the very notion of LOT (implying lots of I-LOTs), this would seem to commit us to thought being impossible without a particular language – an extreme form of the 'Sapir-Whorf hypothesis'.

I don't believe this idea – at least in that form – can be sustained or that Chomsky would seek to defend it. He presents (Q5 below) anecdotal, but surely universally familiar, introspective evidence that we 'can and do think without language' – i.e. think independently of the particular language we speak – suggesting that we have some kind of access to LOT ('conceptual structure') independent of, not mediated by, our acquired particular languages.

(Q5) 'Now what seems to me obvious by introspection is that I can think without language. In fact, very often, I seem to be thinking and finding it hard to articulate what I am thinking. It is a very common experience at least for me and I suppose for everybody to try to express something, to say it and to realize that is not what I meant... it is pretty hard to make sense of that experience without assuming that you think without language. You think and then you try to find a way to articulate what you think and sometimes you can't do it at all;... Experiences like that seem to indicate that we can and do think without language and, if you are thinking, then presumably there's some kind of conceptual structure there.' (Chomsky, 2000:76).

¹² This scenario is contemplated in Smith (1983). Hinzen might also be interpreted as contemplating it when he writes 'language formats thought as opposed to expressing it' (2011:17). However, as I understand it (but not with any great certainty), Hinzen is using 'language' to refer to FL, independently of languages (i.e. not in the generic sense) and with the means of 'externalisation' excluded.

Incidentally, although the canonical CGG account of the FL-PLs relation is instantiatonal (particular languages are instantiating states of FL), Chomsky did once suggest that there is a 'crucial inadequacy' in this idea (1995:6–7). Elucidating, he commented 'it is hard to imagine that the properties of the language faculty – a real object of the natural world – are instantiated in any observed system' (1995:11 fn. 6).

In the light of this, it seems to me that FL-qua-LOT has a necessary role in language acquisition in the following crucial respect: IT GIVES YOU SOMETHING TO TALK ABOUT IN YOUR ACQUIRED LANGUAGE – NAMELY, YOUR THOUGHTS. This is crucial if, as I would argue, that's all there ever is to talk about. (If you don't like 'talk about', you could perhaps replace it with 'express' or 'articulate'.) Of course, it's not guaranteed that your thoughts – or what you think of as being your thoughts – won't change in the light of how you try to talk about them. Hence, trying to talk about (articulate) your thoughts is itself a kind of thought. I wouldn't be the first to suggest there is more than one kind of thought (e.g. Carruthers (1996), who is nevertheless almost wholly concerned with language-mediated conscious thought). Q5 is thus consistent with a more nuanced Whorfanism (Burton-Roberts, 2005). More on this in the final section.

6. An interpretation of 'perfection'

Chomsky suggests that 'perfection' is to be assessed in terms of 'interface conditions': language is perfect to the extent that the thesis/heuristic that 'language is a perfect solution to interface conditions' (2006:3) can be sustained. Consistent with the DIP assumption, he assumes there are two sets of interface conditions, those imposed by SM systems and those imposed by the CI system. Against this, Q1 and Q2 suggest that the conditions imposed by the SM interface – since they are the acknowledged cause/locus of imperfection – must, somehow, 'in a certain sense' and/or to some degree, be discounted in contemplating perfection (see Q3). This is also indicated if imperfection is held to be lack of interpretation at LF, i.e. in CI terms. See Chomsky (2002:112): 'the imperfection is uninterpretable features' – and the context makes clear that this means uninterpretable in CI terms. PF properties are not interpretable in CI terms.

Here I speculate what 'perfection' might actually amount to in the final analysis. What standard or yardstick of perfection can we appeal to? I suggest it is LOT that provides the yardstick. I'm not of course suggesting that LOT is perfect (the suggestion is barely meaningful). What I am suggesting is that it seems to be LOT that is setting the (implicit) standard against which the degree of perfection of FL itself is to be judged. This is indicated if, as in Q3, 'language "imperfections" arise from the external requirement that the computational principles must adapt to the sensorimotor apparatus'. On these terms, FL ('language') is perfect precisely to the extent it approximates to LOT and would be wholly perfect if FL=LOT.

In the light of this, consider Jackendoff's scathing summary of perfection: 'In other words, language could be perfect if only we didn't have to talk' (1997:19). 'Language' for Jackendoff seems to be a generic term for all speakable languages. Given THIS understanding of 'language', we could only sympathize with his reaction. Perfection is indeed an absurd concept in respect of 'language' so understood and, as we have seen, there is plenty in Chomsky's own writings to indicate he himself thinks so. But is this in fact how Chomsky now intends 'language'? Difficult question. On the one hand, his continued commitment to the DIP assumption (language as SWAM) justifies interpreting Minimalism as being about 'language' in that generic sense – i.e. as being about (all) speakable languages. On the other hand, I believe Jackendoff's comment – and the reaction of Jackendoff and Pinker (2005) to Hauser et al. (2002) – misses the target that, notwithstanding the DIP assumption, Chomsky is in fact aiming at. In the sense of 'language' that interests Chomsky, I suggest, the theory of language is the theory of the one human language – a natural singularity. Now, if that is what it concerns, it clearly does not concern any speakable language – and couldn't, since there is, inevitably, a plurality of speakable languages. The single, invariant, natural unspeakable language cannot be other than LOT itself.

Chomsky (2002:109) includes the following arresting statement:

(Q6) Even the fact that there is more than one language is a kind of imperfection.

This is consistent with my speculation regarding 'perfect' and that full perfection in FL ('language') would be achieved by its identification with a natural singularity, a generative computation describable as 'the language of thought'. But, again, Q6 reflects the general tension. What kind of imperfection is referred to in Q6? Is it ontological ('substantive') or methodological? Surely not ontological: if the world is such that it includes a plurality of languages (and it is), that's the way the world is. Chomsky's continued adherence to the double-interface assumption ('language as SWAM') suggests the theory should indeed be construed as a theory of (all) particular languages (with their phonologies). And, as noted, if that's the object of enquiry, its plurality is, if not conceptually necessary, then at least inevitable, for it is precisely the non-natural conventionality – the Saussurean arbitrariness – of 'sound with a meaning' that makes for the plurality/diversity of particular languages.¹³ Could it be, then, that the imperfection is methodological, in fact lying IN A THEORETICAL PROGRAM for 'language' that's unstable as to what it is a theory of – a natural singularity or a *de facto* plurality?

That's one construal of the 'imperfection' constituted by the plurality of particular languages. An alternative construal, suggested by the closing questions of section 4, is this. If by 'language' ('FL') we in fact intend the one human language describable as 'the language of thought', the imperfection lies in a theory of language that is left undecided what to do with – what to make of – particular languages. How do they fit into the picture, on that interpretation of 'language'? More specifically, what is the relation between that natural singularity and the plurality of particular languages? What we require is a new account of the RELATION between FL/LOT and the plurality of particular languages, one that would allow us to make a more radical DISTINCTION between them than is generally made. I turn to this now.

¹³ This is my answer to Berwick and Chomsky's (2008:1 et passim) 'Why are there so many languages?' and Hinzen's (2009) 'Why do languages vary?'

7. Symbolic representation, phonology and particular languages

As mentioned, the proposal that syntax is grounded exclusively in semantics – is the generative engine of conceptual structure – and thus that FL and LOT should be identified, demands a story on phonology. What principled grounds – independent of any desire to equate FL and LOT – can be offered for excluding phonology from the generative computation? We need a story on phonology and how exactly it RELATES TO the one human generative computation (FL/LOT). Equally, we need a story – consistent with (ideally, identical to) the story on phonology – about particular languages and how they relate to that single computation.

I have elsewhere made a proposal in this regard, the Representational Hypothesis (RH) – see references. Although it informs the thoughts outlined above, the RH is not the primary focus of this paper. The primary focus is the interpretation of CGG and Minimalism in particular. But I am bound to offer answers to those questions and an alternative to the double-interface assumption. Insofar as the RH does this with any plausibility, it shows the DIP assumption is not conceptually necessary.

What motivates the DIP assumption is the notion of ‘externalization’. Chomsky (e.g., 2006, see also Berwick and Chomsky, 2008) repeatedly adverts to this. The evolutionary picture that emerges from Q2 plus Q4, for example, is that the internal system (‘a “language of thought”’ – Q4) underwent an internal modification – ‘adapt[ing] to the sensorimotor apparatus’ and thereby developing into a double-interface system by the incorporation of a phonological component – specifically to allow for ‘externalization’ of the objects it generates.¹⁴ ‘Externalization’ here seems to mean: manifestation in – by some kind of conversion into – some mind-external form produced by human motor systems and perceived by human sensory systems. (See Berwick and Chomsky (2008:15) for the most recent appeal to conversion.) This reflects the widely held assumption that objects generated by the mind-internal computation can actually be pronounced (Chomsky, e.g. 2006:8, 9, 14; Berwick and Chomsky, 2008:10).

The RH, by contrast, holds that FL, and certainly anything describable as a language of thought – and thus anything generated by it – is of necessity RADICALLY INTERNAL. By ‘radically internal’ I mean, not just not-internalized (it’s innate, not acquired/learned), but also not externalizable (not pronounceable).¹⁵ Relevant mind-external phenomena, on this view, are not ‘externalizations’ of the radically mind-internal system, nor its ‘sensory output’ (Chomsky, 1995:221). For the RH, the relevance of such external phenomena to the mind-internal computation consists in their standing – by convention and intention – in an asymmetric relation of symbolic REPRESENTATION TO the radically mind-internal objects that the computation generates (‘symbolic’ in C. S. Peirce’s sense). This representational view calls for no CONVERSION of anything into anything else. If *y* stands in a relation of representation to *x*, *y* is not converted into *x* nor *x* into *y*. In particular, it calls for no metaphysically questionable ‘conversion’ of the mind-internal into the mind-external or vice versa.

In Burton-Roberts (2011), I argue that the RH provides an alternative account of ‘sound with a meaning’ to the DIP assumption that expressions of particular languages have phonological and semantic properties. Very briefly, the argument is that ‘meaning’ resides in, indeed is constituted by, that representational relation. The crucial idea here is that ‘meaning’ is a RELATION, not a PROPERTY – and hence not a semantic property. More specifically, what generally passes for ‘the meaning of *p* (for someone *s*)’ actually resides in the relation of *p* to the constitutive semantic (CI) properties of an object generated by LOT, a thought entertainable by *s*. Conceptual/semantic content resides in and only in LOT. Semantics, in short, is one of the terms (one of the *relata*) of any meaning relation. So, while meaning crucially involves semantics (it being a relation TO semantics), ‘meaning’ and ‘semantics’ are distinct. If *p* is an uttered sound, this reconstructs ‘sound with a meaning’ without appeal to anything having both phonological and semantic properties, i.e. without appeal to double-interface objects.

In the light of this, consider this from Fodor (1998:6): ‘... English has no semantics. Learning English isn’t learning a theory about what its sentences mean, it’s learning how to associate its sentences with the corresponding thoughts’ (italics in original). The RH is fully consistent with Fodor’s first sentence. However, the passage appears to suggest that if sentences of English have no semantics they can’t be said to ‘have meaning’. The relational account of meaning (its distinction between meaning and semantics) avoids that unwelcome conclusion – and pre-empts the usual motive for attributing semantic properties to English (‘what has meaning must have semantics’). On the contrary, ‘learning how to associate [X]s with... thoughts’ PRECISELY AMOUNTS TO ‘learning... what [X]s mean’.

¹⁴ Berwick and Chomsky (2008:12) suggest the architecture needed for this is analogous to a computer that incorporates a printer. The analogy is vivid with respect to ‘externalisation’ and the DIP assumption. Furthermore, it is useful in this context because it serves to contrast clearly the DIP assumption with the representational idea of the RH (see below). I don’t think a print-out can be regarded as standing in the relation of REPRESENTATION TO anything in ‘the computer’s central processor’ or responsible for the print-out.

However, it seems to me the analogy falls short in CGG’s own terms in capturing only ‘externalisation’ (output from the computer to the print-out), not the converse i.e. input from the ‘print-out’ to the computer. We need to accommodate hearers’ understanding as well as speakers’ production. I assume that in understanding speech, individuals deploy the same system they deploy in their own speech.

Incidentally, it cannot be accurate to say that the system ‘develop[ed] into a double-interface system by the incorporation of a phonological component’ for, as argued, there is NO SINGLE (universal, invariant) double-interface system. ‘Phonology’ is an inalienably GENERIC term for the plurality of phonological systems, standing in a many-to-one relation to (non-generic) syntax-semantics. The RH is a proposal about what species of relation this is.

¹⁵ A referee suggests this implies that FL/LOT is ‘a private language’ in Wittgenstein’s sense. There are two respects in which the private language argument has been thought relevant in this context, (i) individualism and (ii) rule-following, neither of which seem relevant to FL/LOT. As regards (i), while ‘I’ certainly applies to FL/LOT if ‘I’ = ‘Internal’, it definitely does not apply if ‘I’ = ‘Individual’ (as against universal). As for (ii), rule-following is a behavioural, more or less person-level notion inapplicable in the context of LOT, which I take to be a sub-personal system. It may be relevant to individuals’ linguistic behaviour but, as discussed in Burton-Roberts (2011), this does have an inter-subjective dimension in as much as individuals speak and understand in the way they, rightly or wrongly, believe others speak and understand. This is a major determinant of the ‘course of the feather’ of an individual’s language.

An individual's acquired particular language is now thought of as a system that provides for such representation – i.e. a system for the representation, in some mind-external medium (typically phonetic), of radically mind-internal conceptual structures generated by LOT. Phonology (including morphology) is central here. Each acquired (spoken) language, L_i , has a phonological system that plays an essential role in establishing L_i 's representational system. It determines what counts in L_i as a well-formed representational phonetic (perceptual) form. Given this representational rationale, phonology is located in and only in the representational systems we call 'languages', not in the radically internal system (LOT) that is the unique *representatum* of those representational systems.

Elsewhere I have argued that particular languages just ARE (morpho-) phonological systems (e.g. Burton-Roberts, 2000). This entails an explicit reconceptualization of phonology as being much richer than traditionally thought. As a phonological system, a particular language L_i determines what phonetic phenomena count as well-formed representational phonetic phenomena producible by its speaker(s). To do this, it must specify (i) the phonetic features of L_i ; (ii) the combinations of features that constitute the segments of L_i ; (iii) the ordered combinations of segments that constitute its basic signs (morphemes); (iv) which of those are free-standing and which bound, thereby specifying the words of L_i (its lexicon). Finally, it must specify (v) which sequences of words constitute well-formed (representational) phonetic forms in L_i . In short, as Epstein et al. (1996:7) put it, L_i specifies what counts as a 'licit sound' in L_i . (i)–(iv) fall within (morpho-)phonology as traditionally conceived and (v) is implied by any framework that assumes that linear order is not syntactic but phonological. However, what counts as a well-formed ('licit') REPRESENTATIONAL sound in L_i depends on what conceptual LOT expression the sound is supposed to be representational of. (For example, /flying+planes+are+dangerous/ will not be well-formed if the sub-sequence /flying+/planes/ is supposed to be representational of an activity concept.) On a representational conception, then, an individual's phonology must make reference to conceptual structure as well as to (types of) phonetic phenomena – i.e. to what is represented as well as to how it is represented. Given a relational account of meaning, then, it is the (morpho) phonology that constitutes L_i which effects meaning in L_i . The representational conception also departs from CGG assumptions in not positing generated 'phonological representations'. The sole locus of generativity is LOT. A representational (morpho-)phonology consists entirely of rules ('instructions?') behaviourally implemented by a speaker producing (and understanding) representational phonetic phenomena. See Burton-Roberts and Poole (2006).¹⁶

8. Phonology and human cognition – a speculation

As regards evolution, the picture presented by the RH is that, once in place, LOT underwent no phylogenetic modification. It remained radically internal, in the sense that it did not – and, given the representational idea, can't be thought of as needing to – 'adapt to the sensorimotor apparatus' (Q2). If anything needed to adapt, it was the sensorimotor apparatus itself – adapting to the representational function. However, assuming the sensorimotor apparatus was already in place in a form sufficient for that function (Tattersall, 1998:15, 24), it did not need to adapt. It was merely harnessed to the new (radically new) function.

The relevant modification in human cognition, then, was external to LOT. It consisted in the emergence of a capacity for the conventional (i.e. symbolic) representation, in mind-external media, of radically internal conceptual structures generated by LOT. And I suggest that, in this respect, ontogeny recapitulates phylogeny. What develops in the human individual is a particular (I-) language, thought of as a complex (not necessarily fully systematic or even self-consistent) set of conventions for the externally physical (typically phonetic) representation of conceptual structures. On this account, the relation between the non-natural (i.e. symbolic) acquired plurality of I-languages and the natural, innate singularity – correlatively, between generic phonology (the plurality of phonological systems) and syntax-semantics, between what is generated and what is produced – is representational. Furthermore, since the relation of representation depends on there being something to represent independently of the fact of representation (i.e. $R(x)$ presupposes x), this provides a conceptually PRINCIPLED basis for describing x (the generative capacity of LOT) as 'primary' and anything pertaining to $R(x)$ as 'secondary'.

Chomsky (2006:10) alludes to a 'great leap forward' in human evolution. As I understand it, this refers to the genetic emergence of LOT (Merge), with 'modes of externalization' being merely 'contrived'. However, it is not implausible to suppose that LOT emerged more generally. Chomsky (2006) suggests crucial features 'might well have other primate origins'. While not seeking to underplay the dramatic character of the emergence of LOT itself, I speculate that it considerably predated a more specifically human, and unprecedented, 'great leap forward', one that consisted in the emergence of a specifically human capacity for REPRESENTATION – and more specifically symbolic (non-iconic) representation – a capacity arguably found nowhere else in nature. What this capacity – and your particular language as the prime example of this capacity – endows you with is a further, more conscious, kind of ACCESS to your thoughts and thus a certain more conscious form of thinking (and communication) denied to species lacking that capacity. This is the crucial *cogito* phenomenon, the

¹⁶ A referee rightly asks about lexicalisation. It is beyond the scope of this paper to address this properly but, for what it is worth, my own view is that most words give you SHORT-CUT (stored-in-memory, computation-saving) ACCESS to a node n indefinitely high up in compositional conceptual structure (compare e.g. English *shallow* with French *peu profond* and Italian *dopodomani* with English *the day after tomorrow*). This departs from Fodor's word~concept isomorphism and conceptual atomism (e.g., 1998, 2008; also, more surprisingly, Berwick and Chomsky, 2008:16). I think it is a correlative of this kind of short-cut (lexical) access that one may have difficulty in more consciously accessing what n dominates, where 'what n dominates' = the (internalist!) structured content of the concept.

capacity to consciously represent to yourself that (and what) you are, non- or less consciously, thinking. It is this conscious access to thought, and the species of conscious thinking it affords, that's evidenced in Q5 above. While this offers an alternative to Smith's (1983) idea that LOT is an initial state that's instantiated in its final state as your language, it captures the intuition that there is indeed a species of thought necessarily conducted in your language.

This capacity for symbolic representation of thought allows for conscious computation by representational PROXY. By this I mean: computation that uses the representations themselves as proxies for what they are representations of. A classic case is the use of pen and paper in arithmetical calculation. Even in mental arithmetic, it's barely conceivable that computation operates directly with numerical concepts as such rather than with their I-linguistic representations. What's evidenced in Q5 is a non-arithmetical example of just this, i.e. computing what the thought *T* precisely is – gaining more or less conscious access to *T* – by judging whether some expression in *L_i* satisfactorily represents *T* and, to the extent that it does, how *T* stands up under such conscious representational scrutiny.

In assigning a representational role to phonology with respect to the single innate syntactic-semantic computation, the RH suggests simultaneously (a) that phonology is NOT a component of the generative computation itself and thus NOT an imperfection in it and (b) that, being essentially involved in the most striking (the prime) example of the symbolic representational capacity (i.e. particular languages), phonology is absolutely central to that species of access to thought that is defining of specifically human cognition. Wherever phonology is, it surely must be central there. This, to me, is a more palatable stance than one that situates phonology within a computational system that would, by hypothesis, be 'perfect' were it not for the putative inclusion of phonology.¹⁷

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¹⁷ A representational approach to phonology offers a plausible criterion of perfection in phonology (though it is, appropriately, a criterion of functional perfection rather than perfection in the technical sense): a phonological system is perfect to the extent that it makes available fully PERSPICUOUS phonetic representations of conceptual structures entertained in thought. On those terms, phonological systems are far from perfect – inevitably so, since it is difficult to see how phonetic phenomena could constitute fully perspicuous representations of thoughts, as a matter of principle. They radically underdetermine represented thought and they allow for ambiguity, for example. Burton-Roberts and Poole (2006) can be read as a consideration of how certain phonetic representations in Icelandic and English, for example, are less than fully perspicuous – and of why, despite that, they are good enough.

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