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'Virtual conceptual necessity', feature-dissociation and the Saussurian legacy in generative grammar[1]

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This paper is a critique of two foundational assumptions of generative work culminating in the Minimalist Program: the assumption that, as a matter of conceptual necessity, language has a 'double-interface property' and the related assumption that phonology has a realizational function with respect to syntax-semantics. The issues are broached through a critique of Holmberg's (2000) analysis of Stylistic Fronting in Icelandic. We show that, although empirically motivated, and although based on the double-interface assumption, this analysis is incompatible with that assumption and with the notion of (phonological) realization. Independently of Stylistic Fronting, we argue that the double-interface assumption is a problematic legacy of Saussure's conception of the linguistic sign and that, conceptually, it is neither explanatory nor necessary. The Representational Hypothesis (e.g. Burton-Roberts 2000) develops a Peircian conception of the relation between sound and meaning that breaks with the Saussurian tradition, though in a way consistent with minimalist goals. Other superficially similar approaches (Lexeme–Morpheme Base Morphology, Distributed Morphology, Jackendoff's Parallel Architecture) are discussed; it is argued that they, too, perpetuate aspects of Saussurian thought.

This paper is about the conceptual underpinnings of generative grammar, particularly as embodied in work culminating in the Minimalist Program (MP). We approach the issues through a critique of Holmberg's (2000) analysis of Stylistic Fronting in Icelandic (SF). His analysis rests on the proposal that the syntactic computation can target just phonological features. Under this proposal, lexical feature bundles may become dissociated, with the phonological features of a lexical item in one syntactic position, and its semantic and formal features remaining in another.[2]

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[2] We use 'proposal' to refer to the general idea of feature-dissociation, and 'analysis' to implementations of it in treatments of specific phenomena. The proposal is adopted by

We believe that the consequences of feature-dissociation for the (minimalist) framework that Holmberg assumes, and for 'sound–meaning' relations more generally, are far-reaching. Feature-dissociation, we claim, entails the denial of foundational assumptions constituting the core of what Chomsky (1995a: 168f.) has referred to as 'virtual conceptual necessity'. These include the claim that the syntactic computation projects lexically paired sound–meaning representations onto an array of more complex expressions and the claim that the gross distribution of phonological features at PF (i.e. word order) is determined by the syntax. These interrelated claims have existed in one form or another since the earliest days of generative thought and can be traced back, via Saussure and others, to Aristotle – in the idea that language involves 'sound with a meaning' (Chomsky 1995a: 2). Chomsky (ibid.) refers to this as the 'double interface property' of language (DIP), a usage we will adopt. In Minimalism, this property is held to be the single, ineliminable, 'virtual conceptual necessity'.

The claim of this paper is that the 'double-interface property' is NOT conceptually necessary and is in fact profoundly problematic. Holmberg's feature-dissociation proposal and analysis of Stylistic Fronting in Icelandic provides us with a starting point because it so clearly highlights the issues, by undermining the very framework of assumptions on which it depends.

This is the concern of PART ONE (sections 1–5) of our article. Section 1 outlines how the DIP is reconstructed in the framework that Holmberg assumes. Section 2 presents Holmberg's arguments for feature-dissociation and the essentials of his analysis of SF. Section 3 presents our critique. It is here that we argue that feature-dissociation undermines the framework on which it depends and is problematic in other respects. Section 4 suggests, however, that Holmberg's analysis does, at a general level, accurately capture core empirical properties of SF. This empirical accuracy of Holmberg's analysis, we argue, directly exposes tensions within the Minimalist Program's DIP assumption and its conception of how phonology relates to syntax. Section 5 compares feature-dissociation à la Holmberg with the Split Sign Hypothesis (SSH) of Cormack & Smith (1997, 1999, 2000).

PART TWO is more general. We argue in section 6 that, although some kind of relation between sound and 'meaning' is a matter of necessity, the Saussurian manner in which this idea is generally implemented in generative grammar is problematic. Section 7 discusses an alternative possibility, the Representational Hypothesis (RH) of Burton-Roberts (see references in footnote 19 below). It follows from the RH's conception of the syntax–phonology relation that syntax is – necessarily – phonology-free. Section 8

Joitteau (to appear) in her analysis of Long Head Movement/Verb Second in Breton. Although we focus on Holmberg's analysis of SF, we suspect that our observations apply to 'PF-movement' analyses in general (particularly 'strong phonology approaches' in the sense of Bošković 2001).

compares the RH with three other approaches: Lexeme–Morpheme Base Morphology (Beard 1995), Distributed Morphology (Halle & Marantz 1993), and Jackendoff's (1997, 2002, 2003) Parallel Architecture. Insofar as these have been claimed to keep syntax phonology-free, they too might seem to break with the Saussurian (DIP) tradition. We hope these comparisons will serve the more general aim of the paper, to open up a debate about the Saussurian legacy in generative grammar.

PART ONE. THE DOUBLE-INTERFACE ASSUMPTION AND FEATURE-DISSOCIATION

1. DOUBLE-INTERFACE PROPERTY, MINIMALISM AND THE STATUS OF PHONOLOGICAL FEATURES

The central heuristic of the minimalist framework that Holmberg assumes is the oft-repeated phrase 'virtual conceptual necessity'. Pursuing the conjecture that language is 'perfect' (e.g. Chomsky 1995a: 9), Minimalism has sought to radically reduce the number of constraints, principles, and 'levels of representation'. But no matter how radically the minimalist idea has been pursued, there is one central idea which is thought to be ineliminable and which animates discussion within and beyond the Chomskian paradigm: the putative 'double-interface property' of language. Now it is undeniable that – in spoken communication – there is some relation or other between articulated sounds and concepts entertainable in thought. However, the DIP assumption is richer than that. It is a cluster of assumptions deeply embedded in the fabric of the Minimalist Program (and in work leading up to it).

The DIP assumption is implemented there in terms of a computational system which articulates the relation between two interfaces, the conceptual–intentional interface (Logical Form, LF) and the articulatory-perceptual (sensorimotor) interface (Phonetic Form, PF). Grammar – and, at its core, syntax – is taken to serve this double-interface property by operating on double-interface objects selected from the lexicon, i.e. objects which consist both of features interpretable at PF (phonological features, or p-features) and features interpretable at LF (semantic features).

The 'overt' part of the syntactic computation (up to Spell-Out) preserves these sound–meaning correspondences and projects them onto an array of larger expressions – composing arbitrarily complex expressions out of lexically encoded sound–meaning correspondences. Lexical items are assumed to be 'atomic' in the sense that, when the syntax selects an element, it inevitably selects all its features – its formal and semantic features and (if it has them) its phonological features (p-features). P-features are regarded as inalienable properties of linguistic expressions, inevitably entering the syntax because lexical items enter the syntax.

From one perspective, then, this manipulation of p-features is quite incidental to the syntax, the 'core' function of which is to serve the LF interface. The syntax actually TARGETS only formal/semantic features. P-features, being in part constitutive of these lexical atoms, are merely – but, given the DIP assumption, inevitably – pied-piped along with the formal and semantic features, as 'free riders'. From another perspective, however, pied-piping of p-features into and within the computation is far from incidental. It is in fact essential to the role of syntax in serving the double-interface property, the enterprise of projecting sound–meaning correspondences compositionally. This involves serving the PF interface as well. This tension (and the competing demands of the two interfaces) is a leitmotif of this paper.

At least within the framework assumed by Holmberg, phonological pied-piping is intrinsic to the view that phonology has a REALIZATIONAL role in relation to syntax. Since p-features are pied-piped into and within the syntax, the gross linear distribution of p-features at PF (word order) is determined by the syntactic computation. It is a reflection or expression of the hierarchical distribution of semantic and formal features. This is what it means to say that PF is the 'realization' of the syntax. It is on the basis of this realizational view of phonology that occurrence at PF is taken as evidence of syntactic occurrence, and linear order at PF as evidence of operations having applied in the syntax.[3]

In summary, several interrelated properties of the grammar arise out of this modelling of the relation between sound and meaning.

(I) Lexical double-interface objects – Saussurian conjunctions of PF-interpretable and LF-interpretable features – are assumed to be necessary.

(II-a) The computation itself is a double-interface system in as much as it compositionally projects the properties of the objects assumed in (I).

(II-b) Hence, p-features of necessity enter the syntax.

(III) The role of the phonology is to provide for the realization – the perceptual manifestation – of syntactico-semantic objects.

Underlying all this is a more fundamental assumption:

(IV) To articulate RELATIONS between PF and LF, it is necessary to postulate OBJECTS having features relating to both. More generally, a relation between $[\alpha]$ and $[\beta]$ is to be modelled by postulating a third object $[\gamma]$ constituted as $\{[\alpha], [\beta]\}$.

Assumption (IV) is the basis of the Saussurian sign. $[\alpha]$ and $[\beta]$ (as LF and PF) are PROPERTIES OF the sign $[\gamma]$; since relevant sounds are the sounds OF those objects, those sounds have meaning (viz., the meaning OF the objects

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[3] This view is most clearly (and extremely) illustrated by Kayne (1994) and analyses inspired by it.

they are the sounds OF); conversely, objects with syntactico-semantic properties can be heard/articulated.[4]

This is the framework that Holmberg assumes. In particular, his proposal crucially exploits the assumption that p-features enter the syntax (II-b). However, feature-dissociation exploits the presence of p-features in syntax in such a way as to entirely undermine both the compositional, double-interface property of grammar and the realizational view of phonology.

2. FEATURE-DISSOCIATION: ARGUMENTS AND MECHANICS

Here we present Holmberg's arguments for feature-dissociation, and examine the mechanics of the SF operation, reserving our critique for section 3. Holmberg's discussion is intricate and wide-ranging but we focus here just on relevant essentials.

Stylistic Fronting in Icelandic (SF) is illustrated by word order contrasts such as that in (1), where (1b) – synonymous with (1a) – is the order brought about by SF.

(1) (a) Hver heldur þú að hafi **stolið** hjólinu?
who think you that has stolen the-bike
'Who do you think has stolen the bike?'
(b) Hver heldur þú að **stolið** hafi hjólinu?
who think you that stolen has the-bike

In line with the framework outlined above, Holmberg assumes that word order is the realization of syntax. He claims, therefore, that the word order contrast visible at PF in (1) is brought about by (i.e. is the realization of) a syntactic operation. The problem is that the categories that undergo the relevant operation (SF) are syntactically a non-natural class. In addition to participles, as in (1) above, negation, as in (2) below, verbal particles, (3), and adjectives, (4), can all undergo SF.

(2) þetta er tilboð sem **ekki** er __ haegt að hafna.
this is an-offer that not is possible to reject
'This is an offer which it is not possible to reject.'
(3) þegar **fram** fara ___ kosningar eru blöðin full af áróðri.
when forth go elections are the-papers full of propaganda
'When elections are held, the papers are full of propaganda.'

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[4] The fact that, at Spell-Out, PF and LF properties get DISJOINED might seem to model the idea that what is heard is purely a function of PF and what is entertained in thought purely a function of LF. However, within the DIP framework, what is heard and what is interpreted stem from CONJUNCTIONS of PFs and LFs – in the lexicon and thence projected by the computation. Given compositional projection, the computation is as a whole (globally) conjunctive: PFs and LFs are the PFs and LFs OF lexical objects and of (DIP) objects projected from them. The system as a whole is, after all, intended as a model of 'language', a model in which 'the double interface property of language' is a basic assumption.

(4) þetta er eini maðurinn sem **hreykinn** er _____ af Kalla.
    this is only man-the that proud is   of Kalli
    'This is the only man that is proud of Kalli.'

It is this that drives Holmberg's analysis. He comments (p. 458):

> When confronted by [sentences like (1b), (2)–(4)], an obvious question to
> ask is, What do the categories fronted by SF have in common that singles
> them out for the operation? The answer seems to be, Nothing, except that
> they are visible (they have p-features).

The proposed solution is to allow the syntax to target and move just
the p-feature bundle of a lexical item. This would account for a central
property of these constructions: the operation seems to have no inter-
pretive (LF) consequences. It would also explain why the operation seems
to target a syntactically non-natural class of categories, including both X°s
and XPs.

Holmberg suggests that the trigger for the p-feature movement in (1)–(4) is
the Extended Projection Principle (EPP). He suggests that the EPP feature of
I should be split into two separate features: a 'D-feature' and a 'P-feature'.
The D-feature requires that the formal features of a nominal category enter
into a checking relation with I. The P-feature, present only in finite I, re-
quires Spec IP to be filled with phonological material.

If the nominal category that enters a checking relation with I has phono-
logical content, then both features (D, P) are checked off by the attraction of
the subject, as in the following Icelandic example:

(5) Jón las aldrei bókina.
    Jón read never the-book
    'Jón has never read the book.'

In raising from Spec VP to Spec IP, *Jón* checks off the D-feature of I. Since
Jón has phonological content, it also checks off the P-feature of I. By con-
trast, if (for example) the subject lacks phonological content, the two re-
quirements must be satisfied separately. Consider (6), which has an arbitrary
pro subject.

(6) Ef **gengið** er __ eftir Laugaveginum ...
    if walked is   along the-Laugavegur
    'If (one) walks along the Laugavegur ...'

Pro_{arb} can check off the D-feature of I but, because pro_{arb} lacks phono-
logical content, it fails to check the P-feature. This, for Holmberg, is the
trigger for SF. To check the P-feature of I, Holmberg claims, the
p-features – just the p-features – of the structurally closest element to I, in
this case the participle *gengið*, are attracted to Spec IP. All other features of
the lexical item remain in VP. Hence 'feature-dissociation'.

Another case, which will be important later, is (7):

(7)   Hann spurði hver **sagt** hefði __ fra   þessu.
      he   asked who **said** had   from this
      'He asked who had spoken about this.'

SF has applied here too. This must mean that, for some reason, the P-feature of I fails to be checked during the derivation. The need to eradicate this feature is what triggers the movement of (just) the p-features of the nearest element – here the participle *sagt* – to Spec IP. In the case of (7), Holmberg claims that the P-feature of I fails to be checked in (7) because – contrary to surface appearances – a phonologically 'empty' *wh*-operator *Op* has been selected from the lexicon, merged into Spec VP, and then moved to Spec IP and ultimately Spec CP. Being phonologically empty, *Op* can only check the D-feature, forcing the P-feature to be checked off by SF of /sagt/.[5]

How does this square with the appearance of /hver/ in (7), which looks as though it is a 'true' *wh*-word (i.e. *hver*)? Holmberg (note 28) briefly considers two possibilities, in terms of two different treatments of *Op*. For convenience, we'll call these 'Op_1' and 'Op_2'. Under the first approach – Op_1 – we encounter another aspect of feature-dissociation in Holmberg's analysis: with Op_1 having moved to Spec CP, the p-feature complex /hver/ is selected (independently of any semantic and formal features) and merged directly into Spec CP.[6] Thus, Spec CP plays host to the semantic and formal features of the null operator and the p-feature complex /hver/.

The second possibility suggested by Holmberg is that Op – Op_2 – is (again) a phonologically empty *wh*-operator, but, instead of moving from Spec IP to Spec CP, it moves to C. Then a 'true' *wh*-word *hver* is selected and merged into Spec CP.

3. IMPLICATIONS AND ISSUES

In this section we discuss the implications of Holmberg's proposal and analysis.

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[5] Here we introduce some notational distinctions which are important in what follows. We will continue to use italics when referring to lexical items taken as sound–meaning pairs (e.g. *sagt*). We will use slants (around ordinary orthography) to refer to p-feature bundles (e.g. /sagt/) and small caps to refer to bundles of semantic and formal features (e.g. SAGT); thus *sagt* = {/sagt/ + SAGT}.

[6] Holmberg, in his note 28, leaves open the formal implementation of the *Op* hypothesis. It is therefore not completely clear at what point the dissociation of *hver* (into /hver/ and HVER) takes place in (7). We return to this issue briefly in section 3.2 below.

3.1 *The abandonment of the 'double-interface property' and realizational phonology*

To see how feature-dissociation undermines the hypothesized (DIP) necessity discussed in section 1, consider again Holmberg's analysis of (6) (repeated here):

(6) Ef **gengið** er __ eftir   Laugaveginum …
     if walked is   along the-Laugavegur
     'If one walks along the Laugavegur …'

Here, pro_{arb} in Spec IP is unable to check the P-feature in I, so (just) the p-features of the participle are attracted to Spec IP. A major consequence of this is that Spec IP – a single syntactic position – is playing host to the semantic and formal features of one element (pro_{arb}) and the p-features of an entirely different element (*gengið*). In fact, pro_{arb} can be associated with ANY phonological material 'belonging to' an SF-able expression. But the double-interface assumption that p-features do inalienably 'belong to' expressions is abandoned under this analysis. Expressions can lose 'their' p-features and acquire p-features unrelated to their lexically-encoded ones.

   How sounds are able to change 'meanings' under feature-dissociation is seen even more clearly in Holmberg's analysis of (7), repeated here.

(7) Hann spurði hver **sagt** hefði __ fra   þessu.
     he   asked who **said** had   from this
     'He asked who had spoken about this.'

Recall that, since SF has applied in (7), the p-features of I must not have been checked during the course of the derivation. Holmberg explains this by claiming a phonologically null operator *Op* is what moves from Spec VP to Spec IP. Since this element has no p-features, SF must apply to check them (with a subsequent explanation of how and why /hver/ is seen at PF). But consider how this analysis impacts on the putative 'double-interface property'. Since *Op* (Op_1 or Op_2) has passed through Spec IP, the target site for SF, the SF operation is here deriving a sound–meaning association of /sagt/ and the trace of *Op*.

   More generally, in (6) and (7), the computation is both generating sound–meaning correspondences distinct from any encoded in the lexicon, and destroying correspondences that ARE lexically encoded, by dissociating the phonology of the participles from their formal/semantic features, which remain in VP.

   The fact that the computation is spontaneously generating and destroying lexically encoded sound/meaning correspondences creates a PF/LF mismatch that is inimical to the assumed 'double-interface property' of the system. This goes hand in hand with an abandonment of the realizational view of phonology. Were we to express the results of feature-dissociation in

realizational terms, we would be obliged to say that, in (6), the pro_{arb} subject is 'realized' at PF as /gengið/. Equally, a feature-dissociation analysis of (7) would embody the claim that the trace of Op is 'realized' as /sagt/. And the participle itself (in VP), or at least GENGIÐ, would not be 'realized' at all. But in fact it is not clear what sense can be attached to 'realization' in this context. Since the computation is targeting (rather than pied-piping) p-features, the distribution of p-features at PF is NOT a realization of syntax as usually understood. The computation is not –'at core' or at all – serving the LF interface, but only the PF interface.

3.2 *Implications for other syntactic operations*

Feature-dissociation effectively turns the participles in (6) and (7) into empty categories (semantic/formal features not associated with ANY p-features). But if that is a possibility, why assume that they are ever associated with p-features? Put another way, if 'their' p-features don't need to be pied-piped WITHIN the syntax, why should they be pied-piped INTO the syntax in the first place? The usual assumption – that relevant p-features enter the syntax because that's just the (double-interface) nature of the relevant expressions – simply doesn't apply in the context of this analysis. Those p-features are not serving any purpose connected with the participles themselves but only the independent purpose of satisfying the (modified) EPP. Given this, why shouldn't the syntax just select those p-features – or indeed any arbitrary bundle of p-features – and merge them directly into Spec IP?

That is not unlike something that Holmberg in fact proposes. His Op_1 treatment of (7) opens up the formal possibility that p-features can indeed enter the syntactic computation independently of ANY formal and semantic features. The p-features that are associated with a 'true' *wh*-word (*hver*) are, in (7), selected independently of any such expression and merged directly into Spec CP. This is another aspect of the feature-dissociation embodied in Holmberg's analysis.

It is true that, since Op_1 is already in Spec CP, this has the effect of associating the p-feature complex /hver/ with the formal/semantic features of a *wh*-operator, Op_1. Since we know that *hver* actually IS (though elsewhere) a *wh*-word, this derived isomorphism of Op_1 and /hver/ might seem satisfactory. But it raises the question what constraint or principle of syntax determines how, or allows that, this is possible. P-features generally land up in Spec CP only by being antecedently associated with a *wh*-expression and being pied-piped. But here the p-feature complex /hver/ only becomes associated with a *wh*-expression (Op_1) post hoc – in virtue of independently arriving in Spec CP. In default or in advance of any such association, those p-features simply count as 'rubbish' – i.e. not expressions – as far the syntax is concerned. There is no principled reason, then, to suppose either (a) that

Spec CP is the target site for the p-features or (b) that THOSE p-features, rather than any other 'rubbish', should be selected to fill Spec CP.

More generally, if the purely p-feature complex /hver/ can be merged into a position occupied by Op_1, this calls into question the rationale of Merge. The rationale of this SYNTACTIC operation is compositional and driven by semantic selection. Put simply, it is 'structure building' – but not if it can target just p-features, and not if it can add those features to already-instantiated structural nodes.

Holmberg briefly notes (note 28) that the Op_1 proposal calls for a 'modification' of assumptions about lexical insertion. (The same, arguably, goes for Op_2, see below.) But none of the ways in which this modification might be technically implemented seems attractive. Since Select and Merge must exhaust the numeration (Chomsky 1995a: 225), either (a) the lexicon contains 'free-floating' p-features or (b) the operation which creates the numeration is able to 'split the lexical atom' in the same way that the syntax does. In either case, the question arises as to why /hver/ is selected as opposed to any other p-feature bundle.

Consider now Holmberg's second Op possibility to explain (7). Recall that Op_2 moves (via Spec IP) to C, rather than Spec CP. Holmberg suggests (note 28) that a '*wh* phrase' (*hver* in our case) is selected and merged directly in Spec CP. But it can't be a '*wh* phrase' (or 'word') as usually understood. Since Op_2 in C checks the WH-feature there (note 28), there is no WH-feature (in CP) left to be checked. So, presumably, whatever it is that is merged does not carry any syntactic *wh*-feature. Also, since it has not moved there from any argument position (it is Op_2 that carries the features that check the D-feature of Spec IP) there is no reason to suppose that what is merged in Spec CP has any other formal or semantic features. So it can only be just the p-feature bundle /hver/ that is merged there. The problems posed by the merging of the p-feature bundle /hver/ in Spec CP on the Op_1 scenario thus carry over in full to Op_2.

But Op_2 involves a further departure from standard double-interface assumptions. At least under the proposal in which Op (as Op_1) moves to Spec CP, the relevant formal and semantic features and relevant p-features end up occupying the same position. This is not the case with Op_2. Op_2 is in C, but the p-feature complex /hver/ is in Spec CP. What we have here, then, is free-floating 'rubbish' in Spec CP. It is 'rubbish' at PF since, although /hver/ occurs at PF in (7), it is not realizing anything at all – and, by the same token, 'rubbish' for syntax since, as a purely p-featural complex, it serves neither any narrow syntactic function nor interpretation at LF.

3.3 *Feature-dissociation and 'Interpretation'*

Like Maling (1990), Holmberg assumes that SF is movement to Spec IP. A significant difficulty for Maling was that, while this implies that SF-ed

elements are subjects, she (quite rightly) did not wish to 'attribut[e] subject status' to them (1990: 85). Holmberg's analysis might seem to avoid this difficulty: the participle (negation, adjective, etc.) is not interpreted at LF as being in Spec IP (i.e. as subject) precisely because only its p-features are moved to Spec IP, and these are stripped away at Spell-Out. Its other features (e.g. SAGT, GENGIÐ) remain in the position in which they are interpreted at LF. Nevertheless, there remains a substantive problem: at PF, those p-features ARE in a linear position that, by Holmberg's analysis, 'realizes' Spec IP. If this has empirical content, it should predict that speaker-hearers interpret the participle (or whatever) as subject. The problem is not so much that this is empirically incorrect – though presumably it is – but that it is actually contradicted by the Interpretation assigned at LF on Holmberg's own analysis (in which the participle – or at least its LF-Interpretable part – remains within VP).

The resulting mismatch of PF and LF severs any link between the technical notion of 'Interpretation' at LF (upper-case 'I') and 'interpretation' by speaker-hearers (lower-case 'i'). But it is surely intrinsic to standard double-interface, realizational assumptions that speaker-hearer 'interpretation' and 'Interpretation' at LF ARE inherently linked – in the idea that sounds have meaning. On those assumptions, speaker-hearers perform interpretation on PF (PF is WHAT they interpret) and the LF Interpretation derived by the computation is the interpretation they assign (TO PF). It is precisely this link that gives empirical content to 'Interpretation at LF' – as an empirical claim about how speakers interpret the 'corresponding' PF. But, within Holmberg's proposal, the relevant PF does NOT 'correspond'. Thus, /gengið/ in (6), for example, is in a PF position that 'realizes' both a different element (pro_{arb}) and a different syntactic position (Spec IP) from the relevant element (GENGIÐ) and its position at LF. Abandoning the link between 'Interpretation' and 'interpretation' leaves a purely technical notion of 'Interpretation' whose empirical content and evidential basis are obscure.

3.4 *The Holmberg Effect, chains and 'displacement'*

It is important to distinguish the PF/LF mismatch just outlined from what Chomsky (1995a: 221f.) refers to as 'the fact that objects appear in the sensory output in positions "displaced" from those in which they are interpreted, under the most principled assumptions about interpretation'. This might seem to describe exactly the mismatch effected by feature-dissociation – call it 'the Holmberg Effect'. However, as we understand it, Chomsky is not here contemplating any such effect, but rather standard syntactic movement. Crucially, he is assuming phonological pied-piping. That is, he is assuming that (overt) movement maintains the

(double-interface) integrity of the relevant 'objects' (lexical items or projections of them) – in sharp contrast to Holmberg's feature-dissociation.

In fact, we would argue that movement does NOT entail 'displacement' as described. Take *wh*-movement to Spec CP. *Wh*-terms ARE Interpreted at LF as in Spec CP (for scope), albeit in addition to some other position (for theta role assignment). That is, a *wh*-term is fully interpretable AS A CHAIN: its members are each Interpreted at LF in their respective positions and, by definition (as chain members), they share all semantic and formal features.

The literal 'displacement' embodied in feature-dissociation – if it exists – is quite different. Holmberg's SF operation cannot, for example, be thought of as chain-forming. Members of a putative 'SF chain' share no features at all: the moved element consists just of p-features (/sagt/, /gengið/) and its 'trace' just of formal/semantic features (SAGT, GENGIÐ). Rather than forming a chain, SF creates two separate – and, once separated, independent – objects. It is even arguable that Holmberg's SF operation is not actually movement. On the copy theory of movement – described by Chomsky (2004: 111) as the null hypothesis for movement – it cannot be.

Introducing the above passage about 'displacement', Chomsky (1995a: 221) suggested that it 'might turn out to be one source of a striking departure from minimalist assumptions'. As indicated, it is far from clear that movement (chain formation, internal merge) does involve 'displacement'. Chinese, for example, is assumed to have *wh*-movement (*wh*-chains) without in any sense displaying *wh*-displacement. And it is significant that, in connection with Chinese, Chomsky (2004: 111) writes of 'the DISPLACED element spelled out *in situ*' (our emphasis). Clearly, Chomsky is here generalizing 'displacement' in such a way as to make it equivalent to the syntactic property of chain-formation, independent of any PF property. This makes it clear that, in referring to 'displacement', Chomsky was not contemplating the Holmberg Effect.

Once the chain property is distinguished from any 'displacement' property, it is far from clear that chains should be regarded as a departure from minimalist assumptions.[7] In sharp contrast, we suggest, the literal (uninterpretable, chainless) displacement embodied in feature-dissociation – if it exists – would indeed constitute a substantive threat to the last remaining 'conceptual necessity' of Minimalism: that the syntactic computation preserves and projects lexical sound/meaning correspondences.

In fact, as noted, not only does the movement of just the p-features of a lexical item (feature-dissociation) not constitute 'displacement' of that

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[7] Chomsky (2004: 111 and note 29) in fact retracts the quoted worry about 'displacement', suggesting that (as 'internal merge') it is a conceptual necessity. Again, we would argue, it is not the DISPLACEMENT property that is conceptually necessary but – if anything – the CHAIN property (i.e. 'internal merge').

lexical item, what is manipulated does not even constitute an 'object' in the sense intended by Chomsky in the above quotation. Feature-dissociation leaves that quotation literally uninterpretable. The 'objects' that Chomsky is referring to are lexical items – semantic/formal/ phonetic feature triples. It is only in this sense – if any – that some single thing could 'appear in the sensory output' in a different position from the one in which IT is interpreted. Under feature-dissociation, there simply are no such objects.

3.5 *P-features, linearity and 'imperfection'*

As discussed, given the Saussurian inheritance embodied in the double-interface conception, phonological features must be attributed to syntactic expressions if the syntax is to be thought of as compositionally projecting sound–meaning correspondences. Feature-dissociation crucially exploits the presence of p-features in syntax by allowing the syntax to target what were previously only 'free riders'. But in abandoning the double-interface conception of expressions in particular and language in general, feature-dissociation undermines the only principled reason for phonological features to be present in the syntax in the first place.

  As a result, the only role that p-features are left with, the only purpose served by their presence in syntactic representations, is to derive (however indirectly) the correct 'word order' at PF. But, as Chomsky (1995a: 334) notes, 'there is no clear evidence that order plays a role at LF or in the computation N to LF': phrase-structure has only hierarchical, not linear, properties. Linear order is a PF property not interpretable at LF. It is PF representations that have linear – that is, temporal – properties. Being uninterpretable (at LF), linearity is another respect in which phonology represents a departure from perfection.[8] However, since it is in the very nature of PFs to be linear, linearity can hardly be regarded as an imperfection WITHIN PHONOLOGY. It becomes an 'imperfection' only when imported – with p-features – INTO THE SYNTAX. This is arguably only tolerable (if it is) because necessitated by realizational pied-piping and the DIP assumption. But the feature-dissociation proposal abandons pied-piping within the syntax and hence the whole idea of realization.

3.6 *Interim summary*

This section has advanced the following arguments: (a) Allowing p-features to be targeted by the syntax (independently of formal/semantic features) has

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[8] It is clear from Chomsky (2002: chapter 4) that lack of Interpretation at LF is the ultimate source of 'imperfection'. As regards the mutual incompatibility of LF- and PF-interpretation, see section 6 below on the arbitrariness of the Saussurian sign.

the consequence that phonology can no longer be said to be 'realizational' of syntax in any meaningful sense. Thereby (b), the analysis abandons the assumption that p-features are constitutive of the relevant expressions. But if that is the case, then (c) there is no principled reason to assume that p-features enter the syntax in the first place. In fact, (d) far from projecting lexical sound–meaning correspondences, the computation destroys them. Where it appears to preserve them, it does so in an unprincipled manner (e). This (f) has problematic consequences for I/interpretation and (g) raises problems for the rationale of Merge as a syntactic operation. More generally, (h) the analysis abandons the idea that sound–meaning correspondences are projected compositionally, and thus the double-interface property of language.

4. Feature-dissociation: its theoretical significance

If Stylistic Fronting in Icelandic (SF) is empirically anything like the way it is modelled by feature-dissociation, it presents a challenge to the 'conceptual necessity' of double-interface assumptions. We have criticized the manner in which Holmberg's proposal achieves the SF effect. However, we don't deny that the Holmberg Effect exists or that it is instantiated in the phenomenon known as 'Stylistic Fronting' in Icelandic (and more generally). Crucially, although the analysis undermines the DIP framework, it is difficult to see how ELSE the phenomenon could be modelled within it. The problem, as we see it, is that it is an attempt to model SF within a conceptual framework ill-equipped to handle such effects. Feature-dissociation is significant in so clearly highlighting this.

As we see it, the key feature of the phenomenon that Holmberg addresses is that it is indeed stylistic. Stylistic effects, we suggest, are **[a]** DISTINCTIVE ORDERS AT PF which are **[b]** SEMANTICALLY INERT and **[c]** OPTIONAL. Together, [a] and [b] amount to the effect seen in feature-dissociation. And [c] is surely a function of [a]–[b]. It is our argument that [b] and [c] taken together imply that stylistic effects are **[d]** NOT SYNTACTIC. [a]–[b] would be consistent with [d] were it not for the assumption (undermined by Holmberg's proposal) that word order at PF is the realizational expression of the syntax.

Holmberg's SF operation captures [a]–[b] by giving rise to an ordering contrast at PF not associated with any contrast (or chain) at LF. It is precisely the empirical accuracy of Holmberg's proposal with respect to [a]–[b] that is problematic for the realizational framework that he assumes. The problem is that, while the realizational framework assumes that it is expressions (double-interface objects such as words) that are manipulated by the computation, his analysis has it manipulating, not expressions, but merely phonetic material. As a result, the analysis is not in fact modelling an order of WORDS at PF, but merely an order of p-features (hence our

earlier quotes around 'word order').[9] Since this is not an order of syntactic expressions, we have no reason to think of it as realizational of the syntax.

It is arguable, then, that the analysis does capture [d], the non-syntactic nature of the SF effect. The paradox is that it is, effectively, an attempt to capture its NON-SYNTACTIC nature SYNTACTICALLY. But it doesn't operate on formal/semantic features; it forms no syntactic chain; on the copy theory of movement, it is not movement; it requires a questionable retooling of Merge. In short, SF can be construed as syntactic on this analysis only by considerably loosening what is meant by 'syntax'.

But if the SF effect is not syntactic, the only alternative is that it is phonological; i.e. operates in the phonological component. But, despite its phonological flavour, Holmberg's proposal is motivated by an explicit rejection of this move – rightly, we believe, given realizational assumptions. He argues (p. 469) that SF cannot be an operation of the phonology because it is sensitive to a syntactic distinction between two phonologically indistinguishable words, copula *verið* and auxiliary *verið*. The phonological component lacks the requisite information for the operation. In the framework that Holmberg assumes, this has to be taken seriously, we believe.

Equally important, in our view, is the fact that there is no strictly phonological explanation of the SF effect.[10] This is significant because the realizational conception of the role of phonology implies that properties of PF should EITHER (a) be explainable by reference to syntax OR, failing that, (b) be explainable in strictly phonological terms. This restricts the scope of what counts strictly AS phonology in realizational terms. (Otherwise, we risk turning phonology into a wastepaper basket for syntax; see section 7.3 below on 'PF movement'.)

As we see it, then, the dilemma is that the effect modelled by feature-dissociation can be neither syntactic nor – as usually understood – phonological. It is a PF effect that is neither realizational of syntax nor explainable in strictly phonological terms. The proposal that a syntactic operation can move just p-features can be seen as an attempt to resolve this dilemma. We suggest that the proposal reflects – rather than resolves – that dilemma,

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[9] The subtitle of Holmberg (2000) is 'How any CATEGORY can become an expletive' (our emphasis). This choice of phrase is revealing because it highlights Holmberg's own commitment to the double interface picture outlined in section 1. But little of that picture remains – and that subtitle is actually falsified or at least made difficult to interpret – under feature-dissociation.

[10] The only account of SF we know of that does attempt to motivate it in strictly phonological terms is Poole (1997). However, Burton-Roberts & Poole (2006) advances several reasons why the motivation offered cannot be maintained.

effectively modelling it as neither syntactic (as usually understood) nor phonological (as usually understood).[11]

5. THE SPLIT SIGN HYPOTHESIS

We conclude Part One by considering a proposal of Cormack & Smith (1997, 1999, 2000), which we will call 'the Split Sign Hypothesis' (SSH). The SSH is in many ways comparable to Holmberg's feature-dissociation. We believe that it calls into question the basic double-interface conception which Cormack & Smith – like Holmberg – assume.

5.1 *The SSH principle and its motivation*

Like Holmberg's approach, the SSH posits a lexicon consisting of double-interface objects – Saussurian conjunctions of LF and PF information. The 'normal, default' case (Cormack & Smith (C&S) 1999, 2000: 72) is for the integrity of the double-interface objects to be preserved and projected by the syntactic computation: the PF part of a sign n (PF_n) is merged conjointly with its LF part (LF_n). However, what motivates the SSH is the idea that 'a lexical item does not necessarily appear at Spell-Out as a Saussurian sign' (C&S 1997: 224), or 'not every sign can be interpreted at the LF interface in the position in which it is interpreted at the PF interface' (1999). As in Holmberg's proposal, signs can be 'split', and their PF and LF properties dissociated.

In contrast to Holmberg's proposal, splitting/dissociation in the SSH is achieved by Merge rather than Move.[12] The fundamental SSH principle (C&S 1999) is: 'Where you Merge is Where you Interpret'. Notwithstanding the Saussurian double-interface assumption, however, the principle has to be understood RESPECTIVELY with regard to PF and LF. The PF part of a sign is merged at the position (α) at which it is 'heard' and its LF part is merged at the position (β) at which it is understood (i.e. LF-Interpreted). $(\alpha) = (\beta)$ is the

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[11] Icelandic SF is just an example. Optional phenomena in general present the same problem – English heavy NP shift as in (i) and preposition stranding as in (ii), and *de* phrase fronting in French (Mathieu 2002), as in (iii), to name but a few cases.

(i) (a) I treated no less than four local hospital workers for fractures yesterday.
(b) I treated for fractures yesterday no less than four local hospital workers.

(ii) (a) Which shop was he caught stealing from?
(b) From which shop was he caught stealing?

(iii) (a) Combien de livres as-tu lus?
(b) Combien as-tu lus de livres?

More generally, any semantically empty 'movement' poses the same question. See section 5.

[12] The SSH is more general in its scope in that it is intended to obviate movement quite generally. (Incidentally, in presenting the SSH, we will retain the notation already introduced: *italics* for DIP (PF+LF) objects (Saussurian signs), /slants/ for PF objects, SMALL CAPS for LF objects.)

'normal, default' case. It is when $(\alpha) \neq (\beta)$ that splitting is called for. A sign gets split by its PF part being merged independently of its LF part. Given the overall DIP assumption, however, the PF part must be merged conjointly with – i.e. at the same point in the derivation, and thus at the same node, as – some other sign.

Consider (8), from C&S (1999, 2000), in which we find a disparity between the LF (scope) order and the PF order:

(8) You can't often bribe officials these days.
     LF: NOT [OFTEN [CAN ...
     PF: /can/ >/not/ >/often/

The SSH principle requires that the modal sign ([CAN +/can/]) be split. Its LF part must be merged below/before *often*, which is itself merged below/before negation. Its PF part is merged conjointly with negation and is thus hosted by the POL(arity) node (see C&S 2000 for detail).

5.2 *Comparing feature-dissociation à la Holmberg and the SSH*

Both the SSH and feature-dissociation à la Holmberg give rise to 'double-hosting'. This was among the more problematic aspects of the latter proposal, where a single node ends up hosting the formal/semantic features of one element and the p-features of an unrelated element. Now it is certainly true that – in the data discussed – the double-hosting countenanced by the SSH seems more natural. Intuitively, *can't* in (8) corresponds to at least two elements of Interpretation at LF. However, to properly compare Holmberg's feature-dissociation and the SSH, we need to ask whether the SSH rules out the kind of double-hosting countenanced by Holmberg.

As far as we can see, nothing in the SSH precludes its application to the analysis of SF. In fact, the SSH seems tailor-made for the analysis of phenomena like SF – especially since it is intended to obviate the need for movement that, like SF, has no semantic (LF-interpretable) effect (C&S 1999).[13] But, when applied to SF, the SSH seems to call for exactly the same double-hosting as Holmberg's analysis does.

In this connection, consider (6) yet again.

(6) Ef **gengið** er __ eftir Laugaveginum ...
     if walked is   along the-Laugavegur
     'If *pro$_{arb}$* (one) walks along the Laugavegur ...'

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[13] We subscribe to C&S's aim of eliminating semantically vacuous syntactic movements. However, C&S suggest that movement IN GENERAL has no interpretative effect and, as noted, present the SSH as eliminating movement altogether. This seems empirically too strong: not all movements (chains) ARE semantically vacuous.

By the SSH principle, the Saussurian sign *gengið* must be split: its LF part (GENGIÐ) must be merged where it is understood (or at least 'Interpreted'), namely at the head of VP (as in Holmberg's analysis). Its PF part (/gengið/) must be merged where it is heard. The question is: where is it heard? The straightforward answer might seem to be – and (since 'hearing' is at issue) arguably is – after /ef/ and before /er/. But for the SSH, as for Holmberg, this is not the (or even an) answer. 'Word order', in both frameworks, is a realizational epiphenomenon of the syntax. In both, /gengið/ must be hosted by a node provided by LF structure. It is 'at the host node' that it is 'heard'. Now hosting of /gengið/ by either *ef* or *er* is implausible: neither /ef/ + /gengið/ nor /gengið/ + /er/ are phonological words (unlike /can't/ above). We need a node hierarchically intermediate between the nodes projected by *ef* and *er*, respectively. The only candidate is the Spec IP node projected by pro_{arb}. Spec IP must therefore host /gengið/. This gives precisely the double-hosting – of /gengið/ and pro_{arb} – that Holmberg's analysis gives.

In respect of SF at least, we see little to choose between feature-dissociation à la Holmberg and the SSH – just different technologies (Move vs. Merge) for achieving within a DIP framework a 'displacement' effect. The problems of Holmbergian feature-dissociation are more salient only because the phenomenon (SF) it was developed to account for seems more obviously problematic.

As mentioned, *can't* (in contrast to *gengið*) is intuitively/interpretatively complex, so a double-hosting analysis – in terms of the SSH or à la Holmberg – might seem more natural. The problem here lies in how, technically, the SSH (and presumably feature-dissociation) reconstructs the (lower-case *i*) interpretative facts. Under the SSH, the only LF-interpretable property of *can't* is what, within *can't*, pertains to negation. What relates to the modal is merely PF material, not in itself LF-interpretable. It is thus unclear what the category of *can't* is. It can't be right to call it a 'negated modal verb' because the verb itself is elsewhere in the structure (compare section 7.5 below and especially footnote 37 there). As in SF, then, we have a conflict of I/interpretation: *can't* is not Interpreted as it is interpreted. C&S (1999) themselves acknowledge that, under their analysis, '*can't* has no meaning'. They suggest that this is unproblematic because *can't* is 'simply ASSOCIATED WITH the two LF meanings CAN and NOT' and the appropriateness of the association is guaranteed by (long-distance) checking. However, they concede that under their analysis split signs involve 'extra cost'. Given their basic Saussurian assumption, this cannot be lightly dismissed. If splitting is a possibility, non-splitting must count as mere co-incidence. But, given DIP, it is not a co-incidence – it is, as they note, 'the normal, default, situation'. But then splitting is abnormal, not to say anomalous.[14]

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[14] We elaborate further on the SSH analysis below in presenting 'the signing theorem', which, we argue, offers a simpler and more direct way of capturing what IS unproblematic here.

One might think that the Split Sign Hypothesis shows that phonological pied piping is not essential to the conceptual necessity (the basic DIP assumption) at the heart of Minimalism.[15] We are not persuaded by either of the presuppositions evident here: (a) that the SSH properly preserves the DIP assumption of Minimalism, (b) that the SSH dispenses with phonological pied-piping. In the default case, where a sign $n - \{PF_n, LF_n\} -$ is merged unsplit, we clearly do have phonological pied-piping, in its traditional and most constrained form. Elsewhere (with split signs) we still have it, but in two less constrained forms. First, as seen above with *can*, we have derivationally DELAYED, or DISJOINT, pied piping of PF_n by LF_n. Second, bearing in mind that PF_n, when split from LF_n, must be hosted by the node projected by some other sign m (with LF_m content), we have (conjoint but now) IMPROPER pied-piping. That is, LF_m conjointly pied-pipes its own – proper – PF (i.e. PF_m) but also a PF (PF_n) not proper to it. We suggest, in short, that the SSH does not dispense with phonological pied-piping, but rather abandons the natural constraints imposed upon it by the DIP conception of linguistic objects.

In the final analysis, what is at issue with any form of phonological pied-piping is whether PF information enters syntactic representations along with LF information – and more generally, whether there needs to be, or can be, any object or species of representation that includes both PF and LF information. We pursue this question in Part Two. Our argument in Part One has been that, if the presence of PF information in the syntax is required by the DIP conception of language (and it is), then that conception is properly maintained only by phonological pied piping in its most constrained – conjoint, proper – form. If this presents a problem, the solution cannot lie in abandoning those natural constraints on pied-piping. This merely acknowledges the problem with the DIP assumption. Rather, we should review pied-piping itself and the DIP conception that makes it necessary.

PART TWO. QUESTIONING THE DOUBLE-INTERFACE ASSUMPTION

In Part One we considered empirically motivated proposals and analyses which, although developed within the double-interface conception of language, arguably undermine it. Part Two is more general. Section 6 presents more general reasons for questioning the DIP conception. Section 7 presents an alternative possibility, the Representational Hypothesis (RH). Section 8 considers three other frameworks, in the light of both the RH and the DIP framework discussed in Part One.

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[15] As suggested by a referee.

6. CONCEPTUAL NECESSITY REVISITED

The existence of some kind of relation between sounds and structures interpretable by the conceptual-intentional system is undeniable. This much, we believe, genuinely is conceptually necessary. We want to ask: what KIND of relation is it?

It might be argued that the question is just empty, on the grounds that the relation between PF_n and LF_n is simply DEFINED BY their conjunction in the lexical item n (and thence projected by the computation). This bare MEREOLOGICAL – i.e. part ∼ whole and part ∼ part – relation has the status of an axiom (the DIP axiom).

But this response cuts the double-interface assumption of generative grammar from its Saussurian roots. The point is that Saussure did offer a richer account of the relation embodied in the 'sign'. For him, it was the relation of *signifiant* (signifier) to *signifié* (signified). While generative grammar helps itself to Saussurian mereological pairing, it is noticeably silent on what, for Saussure, actually motivated this pairing.[16]

There are, in fact, fundamental problems with Saussure's own conception of and motivation for the sign and hence with its adoption in generative grammar. Recall assumption IV from section I above:

(IV)   To articulate RELATIONS between PF and LF, it is necessary to postulate OBJECTS having features relating to both. More generally, a relation between [α] and [β] is to be modelled by postulating a third object [γ] constituted as {[α], [β]}.

Assumption IV is in itself quite dubious, on several counts. On the one hand there are OBJECTS and on the other – ontologically quite distinct – there are RELATIONS. It is, at the very least, not conceptually necessary, in acknowledging a relation between [α] and [β], to reify (objectivize) that relation as a third object [γ], with properties {[α], [β]}. Besides, not all relations are (or can be reduced to) mereological relations. Mereological pairing (within an object [γ]) is only necessary if it is the only conceivable or most explanatory account of the relation. But this is surely not the case here: mereological pairing is not the only conceivable relation between PF and LF and is hardly explanatory. Moreover, as soon as we give an explanatory rationale for that pairing – articulating a richer conception of the LF ∼ PF relation (as Saussure did by appeal to *signifiant* ∼ *signifié*, for example) – mereological pairing is rendered not only unnecessary but arguably incorrect. The *signifiant* ∼ *signifié* relation and the mereological relation are simply different and, we will argue, incompatible.

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[16] Cormack & Smith (e.g. 1999), with Beard (1995), are unusual in even alluding to the '(Saussurian) sign'; see Seuren (1993) on the near-disappearance of 'sign' in modern linguistics.

We might question whether Saussure believed in the literal existence of the sign, as an object. The most charitable interpretation is that his 'sign' is a *façon de parler* for talking about a cognized RELATION (*signifiant* ∼ *signifié*). By contrast, the literal existence of double-interface objects is built into the fabric of Minimalism (at least), and (as we have seen) substantive issues within the framework are based on it.

A foundational reason for questioning the conceptual viability of the Saussurian sign and the DIP objects of standard generative grammar concerns arbitrariness. The arbitrariness of the Saussurian sign is universally acknowledged.[17] What is seldom discussed is the metaphysical basis of this arbitrariness (WHY it is arbitrary) or its consequences for the viability of the Saussurian sign. It is arbitrary because it is a relation between two things that, in the final analysis, are SORTALLY distinct (Thomason 1972), things whose respective properties are incommensurable: articulated acoustic phenomena vs. conceptual structures. Notice that this sortal incommensurability is acknowledged in Minimalism's own terms – in its assumption that what is PF-interpretable is not LF-interpretable and conversely (hence the necessity of Spell-Out, the operation that strips p-features from the computation before it reaches LF.)

Given the sortal basis of the arbitrariness, no single entity could possibly be constituted by both sets of properties. But the Saussurian sign – and, pre-Spell-Out, lexical items – consists precisely in the conjunction, within a single object, of both sets of properties. We conclude that there can be no such thing as the Saussurian sign – or any such double-interface object. We do not see that it is possible both to postulate the double-interface object that the Saussurian sign is and to acknowledge its arbitrariness.

Although he doesn't discuss the matter in terms of the arbitrariness of the Saussurian sign, Jackendoff comes to the same conclusion. He insists, on the basis of the distinct natures of the respective primitives of phonology and syntax, that 'there can be no "mixed" representations that are partly phonological and partly syntactic' (1997: 83); such representations are 'formally incoherent' (2003:660). This is one of the motives for his Parallel Architecture, which we consider in section 8.3.

We fully endorse the rationale of Jackendoff's rejection of 'mixed' (= DIP) representations. But it is surely undermined by his assumption that syntactic and phonological representations share the property of being linearly ordered (see Wiese's 2003 response to Jackendoff 2003). As we see it, linearity is a feature of PF that distinguishes it, sortally, from syntax and the conceptual-intentional system which syntax serves. In other words, linear

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[17] Chomsky (1995a) acknowledges Saussurian arbitrariness, but only to 'put [it] aside' (p. 8) and 'henceforth ignore' it (pp. 69f.). It is noticeable that Minimalism, while intended to model an 'object of the natural world' (Chomsky 1995a: 11), seeks to reduce it to the one property (the DIP) that is arbitrary (or at least conventional) and thus NOT natural.

order is a prime source of the necessary arbitrariness of the relation between the two. We know what linear order is about in respect of PF: TIME, the temporal succession of articulatory/perceptual events. Given the sortal considerations that militate against 'mixed' representations, we cannot assign linearity a temporal interpretation in syntax – that's the prerogative of PF – but it is inconceivable what ELSE linearity might be about in the syntactic domain. See below (especially section 8.3).

Noticeable in this connection is the pervasive ambiguity of 'position'.[18] As generally used in this context, it is applied in two incommensurable domains: recursive hierarchy and temporal sequence. To assume that the two sorts of representation have 'position' in a single undifferentiated sense is like saying that a bank is something you can both launch a boat from and have a current account with. It might be legitimate, even necessary, within a double-interface conception of the computation and the ('mixed') representations it generates. But this, we suggest, is part of the general conceptual problem posed by that conception.

7. THE REPRESENTATIONAL HYPOTHESIS

The discussion thus far, we hope, has demonstrated the need for a rethinking of the relation between sound and 'meaning'. Here we discuss a conceptual possibility, as a counterpoint to the assumed necessity of the double-interface assumption: the 'Representational Hypothesis' (RH).[19] As regards SF itself, we present only as much detail on the RH's own account as will allow us to contrast it with DIP accounts of SF. For such an analysis, see Burton-Roberts & Poole (2006).

7.1 *Representation as a relation*

The central idea of the RH can be stated very simply: speakers produce phonetic (or other perceptual) phenomena in aid of REPRESENTING the objects manipulated/generated by the computation. Ultimately, as will become apparent, this boils down to the entirely intuitive and unoriginal idea that speakers utter sounds as a way of perceptually representing – and thus communicating – their thoughts. However, from the RH's reconstruction of

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[18] For example, in the above 'displacement' quote (section 3.4) from Chomsky (1995a: 221f.). The same ambiguity is evident in 'where' in Cormack & Smith's principle 'Where you Merge is Where you Interpret'.

[19] See Burton-Roberts (1998, 2000, 2001, to appear, in preparation), Burton-Roberts & Carr (1999), Chng (1999), Carr (2000), Poole & Burton-Roberts (2004), Burton-Roberts & Poole (2006). 'Hypothesis' is probably too strong, 'conjecture' hardly less strong (see Popper 1963). We'll sometimes refer to the RH simply as an 'idea'. It is a conceptual project, an invitation to rethink (notwithstanding the achievements of DIP thought, which we fully acknowledge).

this idea, a picture emerges that contrasts sharply with standard generative assumptions.

It is important to understand how 'representation' is intended in the RH. It is not intended in the sense usual in current linguistics (and used earlier here). Chomsky remarks that '"representation" is not to be understood relationally, as "representation of"' (2000: 159f.). In this 'non-relational' sense, what-is-represented is not distinct from, but rather is constituted by, the representation. A 'syntactic representation', for example, is not a representation OF anything; it simply IS (constitutes) the syntax. Burton-Roberts (e.g. 2000) adopts 'C-REPRESENTATION' for this sense; 'c' for 'constitutive'. In the RH, by contrast, 'representation' IS intended 'relationally': a representation emphatically is a representation-OF something – something else. To emphasize this, Burton-Roberts (e.g. 2000) uses 'M-REPRESENTATION'; 'm' is for Magritte, a reminder of his painting *La Trahison des Images*, in which the image of a smoker's pipe is accompanied by the warning 'Ceci n'est pas une pipe'. A representation-of-*x* (e.g. a pipe) is not (and doesn't include) an *x*. Expressed in terms of 'signs' – as pointing up the distinction between THE SIGN ITSELF and WHAT-IT-IS-A-SIGN-OF – Magritte's painting is a corrective to the Saussurian tradition in which a sign is constituted as a merological pairing and therefore IS constituted in part by what it is a sign of (viz. *signifié*). Peirce's (1933: 136) counter-Saussurian insistence that a sign must be OTHER than its object is crucial here.

One way of expressing a central idea of the RH is that the Saussurian sign ('S-SIGN') must indeed be 'split' – not derivationally, by Move or Merge or Spell-Out – but by entirely rejecting it (and therefore rejecting derivational operations that split it) in favour of a Magrittian/Peircian concept of sign ('M-SIGN') that acknowledges *signifiant* ∼ *signifié* as a relation between distinct and independent objects.

The reader may have noticed our scare quotes around 'meaning' (at the opening of section 7 and earlier). As it appears in 'sound ∼ meaning', the relational term 'meaning' suggests that whatever-it-is-that-sounds-relate-to is to be thought of as a PROPERTY OF the sound (cf. 'sound with a meaning', quoted earlier). This is incompatible with the RH and Peirce's stricture that a sign must be other than its object. The RH seeks to emphasize that what the sounds relate to is an entirely independent object, with an independent rationale. It is in fact a CONCEPTUAL-INTENTIONAL (C-I) STRUCTURE, an object not defined by (and innocent of) the fact that certain sounds relate to it – and emphatically not a property of sound. It is the C-I structure that HAS semantics – at least in the thoughts of an individual – but it is not the case that the C-I structure IS the semantics-OF anything.[20]

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[20] See Burton-Roberts (to appear) for discussion. We are denying that sounds have SEMANTICS. We don't deny that, when recognized as produced with m-representational intention (i.e. as m-signs), sounds have SIGNIFICANCE. But all sorts of things have significance

7.2 *Realization vs. representation*

The relation of (m-)representation contrasts with the relation of realization, both in itself and in its implications. 'Realization' implies that phonetically interpretable properties are an OUTPUT OF or EMANATE FROM the generative computation, which must thus be seen as serving the articulatory-perceptual (A-P) system as well as the conceptual-intentional (C-I) system. This reconstructs an intuition that what speaker-hearers produce and hear (i.e. speech) is the external facet, or a manifestation or tokening, of the internal computation and its generative capacity.

'Representation', by contrast, implies that phonetically interpretable properties are (m-representationally) TARGETED AT the computation. The idea here is that speaker-hearers don't produce or hear the (wholly mind-internal) objects manipulated/generated by the computation. What they produce and hear are – quite distinctly – perceptual M-REPRESENTATIONS OF such objects. Speech, then, is not the external facet, nor a manifestation or tokening, of the generative computation.[21] Speech stands in a relation of m-representation TO it. This both distinguishes and relates production-by-speakers (a behavourial notion) and generation-by-the-computation (non-behaviourial). The two are distinguished not only sortally but also because of how they are related: m-representation of the latter by the former.

In Magrittian/Peircean terms, what concerns the-(m-)-REPRESENTATION-OF-*x* cannot be PART-OF-*x*. The central idea of the RH is that P-features don't figure in the computation (are not interpretable in it) precisely because they concern only the perceptual (typically phonetic) m-representation of what does figure (and is interpretable) in the computation. Viewed from this perspective, inclusion of PF information within the generative system conflates *representans* and *representatum*, collapsing facts about WHAT-is-represented with logically and empirically independent facts about HOW-it-is-represented in the acoustic (or other perceptual) medium.

A central implication of the representational idea, then, is that expressions of the computation cannot be possessed of properties interpretable in articulatory-perceptual terms. As a matter of conceptual necessity, p-features are not present in the syntax. Nor can they ('later') be 'inserted' into syntactic (c-)representations. It follows in turn that there is no operation of Spell-Out, no distinction between 'overt' and 'covert' syntax – syntax is

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(for someone) without having semantics (black clouds, red litmus paper, green lights, ringing bells). 'Meaning' is a notoriously vague term, covering both 'significance' and 'semantics'.

[21] The tokens involved in speech are tokens of phonetic types, not of conceptual-intentional types. C-I types are tokened in and only in conceptual-intentional structures. See Burton-Roberts (2000: 44f.).
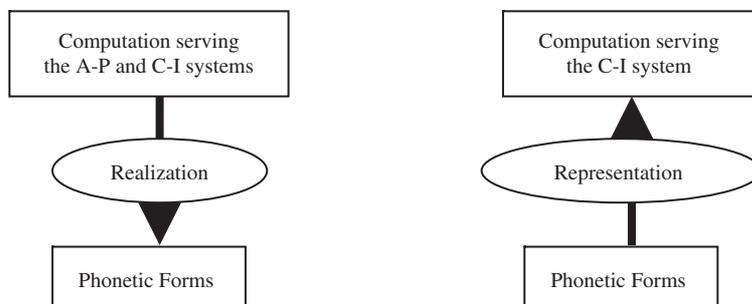
Figure 1
Realization vs. representation

uniformly 'covert', mind-internal – and that, being 'phonology-free', the computation serves just the C-I system. See figure 1.[22]

The idea that syntax should be '(morpho-)phonology-free' is not new. However, to our knowledge, no other proposal offers this general m-representational rationale for it (over and above the sortal rationale). Furthermore, as Jackendoff notes (1997: 86f.), existing proposals (e.g. Fiengo 1980, Di Sciullo & Williams 1987) don't rule out the (eventual) derivation of DIP (c-)representations, i.e. representations that include both syntactic and (morpho-)phonological information. (See Jackendoff (ibid.) for further references, and section 8 below.) Only Jackendoff's Parallel Architecture (PA) – which rejects any form of lexical insertion – claims, with the RH, to reject DIP representations entirely (but see section 8.3).

A radical speculation suggested by the RH concerns the relation between the Human Faculty of Language (HFL) and the conceptual-intentional (C-I) system (or 'Language of Thought', Fodor 1975). It is arguable that the very concept of 'syntax' in HFL – as distinct from the syntax of the C-I system – is wholly motivated by the need (given DIP assumptions) to serve the interface with articulatory-perceptual (A-P) systems. This is manifestly the case with the 'overt' (pre-Spell-Out) syntax. But it is arguably also the case with the 'covert' (post-Spell-Out) syntax. The need to serve the A-P systems (in the overt syntax) entails elements and operations that militate against interpretation by the C-I system. It is a (if not the) function of the covert syntax to 'mend' this – UNDOING (by deletion and reconstruction) what militates against C-I interpretability and DOING what the needs of PF did not allow or demand. When does the (covert) derivation stop? It stops when full C-I interpretability is achieved. This is the point known as 'LF', the interface

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[22] The direction of the arrows in figure 1 reflects the differing logic of each relation. It has nothing to do with the distinction between production and perception.

with the C-I system. In other words, we know we have reached LF – and indeed what LF is – when all traces (positive or negative) of the need to serve PF have been expunged. It is in this respect that even the covert syntax is motivated by the assumed need to serve PF. And it is only in this respect, we suggest, that 'LF' is a well-defined 'level of representation'.

With (morpho-)phonology excluded FROM HFL – on grounds of its m-representational relation TO HFL – the RH suggests that there should be no such 'interface' between HFL and the C-I system.[23] That they are one and the same system is at least the most parsimonious assumption. And it is indicated in any case if, as Chomsky suggests, HFL is a recursive system, invariant across the species, innate, natural, and wholly mind-internal: these are surely the very properties of the conceptual-intentional system itself. These claims about HFL seem most clearly sustainable if HFL and the C-I system are indeed one and the same.[24] For these reasons, we will adopt this here as a working assumption, sometimes referring to the syntactico-semantic C-I system as simply 'L'.

7.3 *Conventions, particular languages, representational ($\Re$-) phonology*

Magritte's m-representation of a pipe depends on RESEMBLANCE between *re-presentans* and *representatum*. In Peirce's terms it is 'iconic'. The m-representations we have to deal with in the linguistic context are not iconic. The phonetic m-representations are devoid of the syntactico-semantic properties of their *representata*, which are themselves devoid of the articulatory-perceptual properties of the representations: the m-representation of (non-linear) purely conceptual-intentional recursive hierarchy by purely articulatory-perceptual temporal linearities. Since the respective properties are non-resembling – and since no sound is of itself m-representational – m-representation in this context is of necessity effected by conventional rule. For Peirce, it is 'symbolic'.

Here we consider the general nature and the locus of representational conventions, developing further the contrast between a realizational and a representational conception of (morpho-)phonology, and explaining the

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[23] Jackendoff (2002): '[W]e must consider the domain of linguistic semantics to be continuous with human conceptualization as a whole' (p. 282); 'A priori, it would make more sense for there not to be any special level of linguistic semantics' (p. 283). In practice he does – and (given his conception of 'syntax') must – distinguish between the syntax of HFL and that of the C-I system. In section 8.3 below we discuss problems with (Jackendoff's conception of) 'syntax' once the DIP assumption is relinquished.

[24] This speculation is consistent with but goes further than Dominey (2003: 674), who in turn goes further (in the direction of 'conceptuocentrism') than Jackendoff (2002, 2003) in suggesting that 'the origin of the universal combinatorial capacity lies more in the independent combinatorial capability of the conceptual system than in [Jackendoff's] syntax'.

very different conception, in the RH, of particular languages and their re-lation to the language faculty, L.

In realizational phonology, PF strings are determined as much by syntax as by phonology. Since this requires that syntax be such as to anticipate the linear order of PF strings, it is conceptually impossible in m-representational terms. Given the asymmetry of the relation of m-representation, properties of WHAT is m-represented simply cannot depend on or anticipate HOW it is (variously) m-represented. In the RH, PF is determined independently and exclusively by phonology. Since the relevant PF strings are m-representa-tional, this means that a phonology is a system that determines what counts as a well-formed phonetic m-representation (of L).

What counts as a well-formed PF string differs from language to language. It depends on the particular language, i.e. on its particular m-represen-tational conventions. Now, arguably, a particular language just IS (nothing other than) the set of its particular conventions. On this basis, the RH sug-gests that a particular (spoken) language is a phonologically-constituted Convention System for the Phonetic m-Representation of L – a CSPR$_{(L)}$. Certain implications of this idea deserve spelling out, since they diverge radically from traditional generative thought.

The first concerns the nature of the relation between particular languages and the universal, innate, natural system (which we are calling 'L'). In the Chomskian tradition, a particular language is an INSTANTIATION of L (and L is 'the initial state'). However, in a little-noted passage, Chomsky (1995a: 6) acknowledges a 'crucial inadequacy' in that idea, explaining in a note (1995a: 11) that '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'. As we see it, the problem with the instantiational idea is that it requires that something natural gets instantiated as something conventional (and thus not natural). It follows from the RH, by contrast, that a particular language stands in a relation of – is a (conventional) system for – M-REPRESENTATION of the (natural) language faculty, L. Since what concerns the-m-representation-of-x is manifestly NOT an instantiation-of-x, the RH offers a distinct conception of the relation, one that obviates the problem Chomsky alludes to, by giving an m-representational account of the relation between nature and convention in this context.[25]

A further radical implication is that each particular (spoken) language doesn't just INCLUDE a phonological system: it IS a phonological system.

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[25] The RH offers a perspective on a range of distinctions: language vs. languages, invariance vs. diversity, nature vs. convention, innate vs. acquired, internal vs. external, generate vs. produce, hierarchy vs. linearity and, finally, semantico-syntax vs. phonology. In each pair, the latter item is motivated by the relation of m-representation to the former. On these terms, L is 'universal' only in the sense of being instantiated in all members of the species, not in all particular languages.

Although implausible (even incoherent) in realizational DIP terms,[26] we believe that this idea is defensible (coherent and even necessary) in representational terms. Particular languages – as $CSPR_{(L)}s$ – don't themselves have syntax or semantics precisely because they are for the phonetic m-representation of what does have syntactico-semantics. Phonetic m-representations require 'parsing' (and parsing is fallible) precisely because they don't possess syntactico-semantic properties. Parsing is a matter of putting what LACKS syntactico-semantic structure (e.g. acoustic phenomena) into correspondence with what HAS such structure, on the assumption that the former was produced with the intention of m-representing the latter. The intuition that a language's phonetically constituted vocabulary items and its 'sentences' (i.e. the conventionally sanctioned strings of its vocabulary items) have meaning and/or structure is real enough, but it is explained in terms of their function and thus significance AS m-representations. We 'read into' the phonetic m-representations what we know of their *representata.*

Clearly, all this entails a concept of 'phonology' that is different from and richer than the traditional realizational concept. We shall refer to this richer concept as 'representational phonology' ($\Re$-phonology). $\Re$-phonology must be richer if only because, in contrast to realizational phonology, it has sole responsibility for determining PF order (insofar as order is constrained by the conventions of the given language, see below). But it must be richer also because, on these terms, $\Re$-phonology is the sole locus of cross-linguistic diversity. This follows from our earlier reasoning. We are dealing with a many-to-one relation: a diverse (because conventional) set of representational systems – $CSPR_{(L)}s$ – with a single, natural, universal *representatum,* L. If a given language IS the set of its conventions and if those conventions are ($\Re$-)phonologically constituted (determining what constitutes a well-formed PF representational string in the language), then $\Re$-phonology must be the sole locus of cross-linguistic diversity.[27]

The RH by no means seeks to underplay such variation. Within the RH, it is not a matter of 'REDUCING' cross-linguistic variation to phonology. It is a matter of assigning variation its proper place in the context of the conceptual relation/distinction between *representans* and *representatum.* The claim that it is PHONOLOGY that determines PF seems entirely reasonable in itself (if we posit a phonology, what else is it for and why should anything

[26] Though it is entirely consistent, notice, with Epstein et al.'s (1996: 7) characterization of (un)grammaticality in terms of what is (or is not) 'a licit sound'.

[27] Sigurðsson (2000: 105) suggests that 'all linguistic variation, including parameter settings, is on the PF side (which means that PF must have access to "syntactic" information)'. See also Roberts & Roussou (e.g. 2003: chapter 5). These suggestions are not, of course, motivated by the representational idea. Nevertheless, if cross-linguistic diversity is PF-diversity, they imply, with the RH, that particular languages are phonological systems – with a consequent need to reconceptualize what phonology is.

else determine PF?) – as does the claim that the evidence for cross-linguistic variation is phonetic (on what OTHER basis could knowledge of the variety of language-particular phenomena be acquired? (Chomsky 1995a: 169, 2001: 2)). But this clearly calls for (and the RH clearly motivates) a richer concept of phonology than is traditional. The suggestion is that, when the tradition purports to be addressing SYNTACTIC diversity, what it is in fact addressing is the cross-linguistic variety of PFs and how they are m-representationally deployed in particular languages – including word order, morphological case and agreement, m-representation (or not) of (especially external) arguments, etc.

Noticeable in this connection are recent proposals for recasting what were previously thought of as syntactic movements in 'phonological' terms – as 'PF movement' (Chomsky 1995a: 368, 2001; Bošković 2001). This, too, calls for a different conception of phonology, beyond the expressive power of realizational phonology. 'PF movement' requires that phonology have access to syntactic properties. And, with PF-movement, the gross distribution of p-features is neither determined by syntax nor motivated by phonological considerations as traditionally understood. But we are not aware of any attempt to articulate any new conception that would legitimize this greater expressive power. (In fact – on DIP assumptions – it simply re-distributes complexity around and within the double-interface computation.) The RH, by contrast, explicitly is a conceptual framework that motivates a richer conception of phonology and its relation to the syntax. Crucially, it embodies a clear rationale for insisting that phonology (as ℜ-phonology) necessarily does have access to syntactico-semantic properties. Since an ℜ-phonological system is FOR the phonetic m-representation of syntactico-semantic properties of L, it necessarily makes reference both to phonetic properties and to syntactico-semantic properties.

It is tempting to say that, as representational, ℜ-phonology is the 'interface' between phonetic and syntactic-semantic properties. But it is an 'interface' only in the sense that it makes REFERENCE TO both sets of properties. It is not a LOCUS OF any of those properties. And, if an 'interface' is thought of as the point at which one kind or level of representation ('c-representation' here) is 'transduced' or 'converted' into another, it is quite out of place in the RH. It is intrinsic to the RH that nothing is 'converted' into anything else in this context (any more than we have conversion to/from Magrittian paint and a pipe). Equally, nothing non-linear is 'linearized'. The relation of m-representation is not a derivational notion. Metaphorically, there is no transportation of properties across the (sortal) gulf that separates the two domains. A representational convention should rather be seen as affording a view (cognitive access) across that gulf.

In the RH, it is not even the case that 'ℜ-phonological representations' are somehow converted into 'phonetic representations' (c- or m-). In fact, if ℜ-phonology consists entirely of conventions/rules – behavioural

instructions – to be implemented by speaker-hearers in the production/re-ception of phonetic phenomena, there ARE no '(ℜ-) phonological rep-resentations' – there are only phonetic m-representations. The conventions/rules which constitute a phonology are expressed in terms of phonetic types and their tokens. For example, English has a rule/convention that allows for the m-representation of just the C-I property of definiteness (DEF) – by means of the phonetically constituted, free (though clitic) m-sign known as the 'definite article'. The convention – i.e. the relevant ℜ-phonology – goes: to m-represent exactly this C-I property, produce a token of the phonetic type /ð/ liaised with a following token of the phonetic type /ə/ (or /i/ if preceding a vowel).[28]

Like any phonology, an ℜ-phonology must specify at least which phonetic features and combinations of them (segments) are exploited for represen-tational purposes in the given language and what ordered combinations of those segments constitute allowable syllables. Features, segments and syl-lables are not as such m-representational; they have m-representational import only insofar as they contribute to the phonetic composition of elements which ARE m-representational. It is for an ℜ-phonological system to specify which syllables or ordered combinations of them are m-representational – i.e. count as m-signs, or 'morphemes'.[29] And, in specifying which of these are bound and which free, it specifies what the words of the language are. Furthermore, in admitting of an m-sign, it must specify what it is a sign OF. So, in admitting a syllable or combination of syllables as an m-sign/morpheme, it specifies which C-I property/ies it is m-representational of. All this is part and parcel of specifying how (in some language) the properties of the conceptual system are phonetically m-represented.

Different languages have different vocabularies. For example, the ℜ-phonology of English, but not French, provides for the m-representation of certain concepts by the single words /shallow/ and /breakfast/. Speakers of French clearly have those concepts. Simply, m-representations of them have to be constructed from other words the language does provide (/peu profond/, /petit déjeuner/). This difference, we argue, is a matter of their phonologically constituted representational conventions.[30]

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[28] We choose this example for purposes of comparison with Jackendoff below. Incidentally, notice how the term 'definite article' attributes to the phonetic m-sign itself a property ('definite') that actually pertains only to what it is a sign of. There's nothing 'definite' about the phonetic form /ðə/.

[29] Since we are dealing with m-signs and not s-signs, we use 'morpheme' (and 'word' below) without any double-interface implication. Our use corresponds to Beard's (1995), see sec-tion 8.1 below.

[30] Svenonius & Ramchand (2005: 13) speculate that 'variation in lexical inventories is in fact the limit of variation across languages' (though word order variation surely needs to be taken into account).

We assume that ALL m-representations radically 'underdetermine' C-I structure(s) entertained in thought (Sperber & Wilson 1995, Carston 2002). Svenonius & Ramchand (2005: 13) plausibly suggest (though not in these terms) that a central dimension of cross-linguistic variation consists in how perspicuous/explicit (with respect to C-I structure) particular languages (as CSPR$_{(L)}$s) are; i.e. how much the m-representational strategy constituted by a particular language leaves to pragmatic inference within the C-I system itself. As they put it, 'where one language more fully specifies certain discourse-functional information … another leaves this information up to the context' (ibid.).

7.4 Linear 'position', structural 'position' and the signing theorem

In addition to necessarily specifying all of the above (and more), a particular language MAY specify the linear (temporal) order of words – its free (phonetically constituted) m-representational elements. Insofar as it is conventionally constrained in a language, word order is intrinsic to what constitutes a well-formed PF string in that language, so it is for the given $\Re$-phonological system to specify it.

Languages that have conventions constraining word order do so as a way of harnessing linearity to the m-representation of structural relations in the C-I system. (We present some representational conventions below.) However, as conventions, they are in no sense necessary even in the languages that have them. In contrast to realizational theories, in which order is a necessary function of syntactic structure, it follows from the RH that there ARE languages with free word order – free in the sense that it is not ordered by convention.[31] And if word order in any language CAN be free, then – even in languages in which it isn't free – there can be no necessary or natural connection between structural relations and linear order.

It might be thought – and conceivably it is an intuition behind the realizational view – that, if there is no necessary connection between linear order and syntactic structure, then a totally unexplanatory picture of interpretative chaos must result, particularly in the case of 'free word order' languages. But this is not the case. The RH allows us to acknowledge a simple and obvious truth, which we'll call 'the signing theorem'. In this connection, the distinction between a (m-)sign and what-it-is-a-sign-of is crucial, as is the RH's separation of linear 'position' (in phonetic temporal strings) and structural/hierarchical 'position' (in C-I structure).

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[31] If not determined by convention, order can be exploited for other (non-representational, e.g. information-packaging) purposes, in which case it won't, in a looser sense than we intend, be 'free'.

The signing theorem is simply this: Let some phonetically constituted m-sign /A/ be a sign of some property of the C-I system [Z]; then /A/ is a sign of [Z] regardless both of the linear position of /A/ and of the structural position of [Z]. More significantly: if /A/ is a sign of [Z], and [Z] is in some structural position S, then REGARDLESS OF ITS LINEAR POSITION, /A/ just is – and will be interpreted as – the sign of a C-I property located at structural position S.

It is this that makes it unnecessary, interpretatively, that the order of words be constrained. It explains the existence of free word order languages and why – even in languages that have conventions that constrain word or-der for the purpose of m-representing structure – those conventions can (as we shall see) be overridden.

The interpretative logic of the signing theorem can be illustrated by Cormack & Smith's discussion of (8) above (section 5.1), adapted here as (8′):

(8′) (a) /You/ > /can't/ > /often/ > /bribe/ > /officials/ > /these/ > /days/
        (b) C-I structure: … [PRES [NEG [OFTEN [POSS …[32]

Their discussion demonstrates that – regardless of the linear order of the PF string – they (and the rest of us) just KNOW that, within the C-I m-*representatum* of (8′a), modality is structurally 'below' (in the scope of) the adjunct (here OFTEN), which in turn is structurally 'below' and in the scope of negation. This knowledge is given to us by the C-I system itself, directly, independently of the linear order of the m-signs in the string (though not of course independently of what signs make up the string).

Given the signing theorem, there can be no interpretative issue raised by the fact that English /can't/ incorporates the m-representation of three distinct C-I properties (PRES, NEG, POSS) each in its proper, distinct position in C-I structure. Furthermore – given the signing theorem but also the RH as a whole – there is no need to assume that any phonetically con-stituted m-sign needs to be moved to the linear position at which it is heard from some other position dictated by what it is a sign of (i.e. by C-I structure). As a result, insofar as a language has conventions that determine word order, it can and must ordain the linear position of each m-sign directly (declaratively).[33] It MAY – or, as in the case of (8′a), may not – do this wholly in aid of m-representing structural C-I position (see footnote 40 below).

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[32] Of course, in this attempt to physically (m-)represent the non-linear C-I structure, we cannot but m-represent it linearly. The order opted for here is governed by convention (10) below. ([POSS] stands for [POSSIBLE].)

[33] Note that, as a set of behavioural instructions, the phonology itself doesn't order the m-signs, it merely ordains how they are to be ordered – by speakers, who have just one bite at the cherry.

Conversely, there is no need to assume, in order to accommodate the single linear position but threefold significance of /can't/, that elements of the C-I system need somehow to agglomerate at a single structural position. In the RH, linear position is NOT an epiphenomenon of structural position. The two are simply distinct, related (if at all) by convention. Given the signing theorem, there is no place here for a concept of derivational 'displacement'. In fact, the RH and the signing theorem allow us to insist on the following fundamental principle (Burton-Roberts & Poole 2006: 581f.): there is no criterion for structural C-I position other than Interpretation in the C-I system and no criterion for linear position other than the temporal succession of hearable events.

As we understand it, this is the idea behind Cormack & Smith's 'Where you Merge is Where you Interpret'. But it is problematic in the context of their (DIP) assumption that the linear position of a significant PF element is a (realizational) function of structural position. This, we have seen, makes the (single) linear position (and very existence) of the form /can't/ anomalous, given its threefold significance. Given the DIP assumption, there seems no alternative to their claim that a structural position (the node projected by NEG) hosts a purely phonetic form (that relating to POSS). But it is difficult to know what sense to attach to the idea that a structural position that is ONLY C-I interpretable can host something that – as a pure PF – is NOT C-I interpretable. In the RH, /can't/ is no more heard at the node projected by NEG than /gengið/ is heard at SPEC IP. They are heard in and only in the sort of 'position' that is proper to what is hearable: BEFORE and/or AFTER other hearable phenomena.

Nevertheless: if we relinquish the DIP assumption, abandon the s-sign for the m-sign, unambiguously distinguish linear and structural 'position' and thus the two senses of each of 'where', 'merge' and 'interpret', then 'Where you Merge is Where you Interpret' can be acknowledged as the conceptually necessary truth it surely is.

7.5 *Some precedence conventions and the SF effect in Icelandic*

We assume that the Head-Complement (H-C) relation and the structural relation of H and of C to HP (the phrase they constitute) are matters of the C-I system (the *representatum*). What is not a matter of that system is how, or whether, particular languages m-represent these relations. Linearity is not part of the syntactic definition of the H-C relation. So it cannot be H and C themselves that are linearly ordered. For the RH, the ordering is, quite distinctly, an ordering of phonetic m-representations of H and C – symbolically $\Re(H)$ and $\Re(C)$.

English and Icelandic, in common with other ('head-initial') languages, include the following convention for the linear m-representation of the H-C

relation and, thereby, for the linear m-representation of the hierarchical relation of H and C to HP.

(9)   *Default precedence convention I*
      Symmetric c-command between a Head (H) and its Complement (C) is m-represented by the m-representation-of-H ($\Re[H]$) being adjacent to and preceding the m-representation-of-C $\Re[C]$.[34]

This is an entirely arbitrary matter. There is nothing in the H-C relation itself on which to base any expectation about linear order or adjacency. Japanese, for example, has a distinct convention, determining that $\Re[C]$ precedes $\Re[H]$ – and not requiring that they be adjacent. Free word-order languages have neither convention.

Not all ordering conventions are entirely arbitrary, however. There is one respect in which linear and structural position ARE comparable: they both involve transitive relations – linear precedence with the former, c-command with the latter.[35] We surmise that every language that has conventions constraining word order exploits this comparability – m-representing structure by making the transitivity of linear precedence match that of c-command. English and Icelandic are among languages that exploit it, by means of the following very general convention:

(10) *Default precedence convention II*
      Asymmetric c-command between elements X and Y – such that X c-commands Y but not vice versa – is to be m-represented by $\Re[X]$ preceding $\Re[Y]$.

Although not wholly arbitrary – i.e. although motivated – (10) nevertheless is only a CONVENTION. It is not necessary. Japanese achieves the same effect (matching the transitivity of linear precedence to that of c-command) by a comparable convention ordaining that $\Re[X]$ follows $\Re[Y]$. And, again, other languages don't have either convention. Furthermore, given the signing theorem, (10) can (as we shall see) be overridden even in languages that include it (hence 'default').

This brings us to 'Stylistic Fronting' (SF) in Icelandic. Here we summarize (and partly develop) the RH analysis presented in Burton-Roberts & Poole

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[34] (9) is underdetermining in the sense that, from the fact that $\Re[X]$ immediately precedes $\Re[Y]$, it does not follow that X and Y necessarily are in the H-C relation.

[35] Precedence and c-command are distinguished in their other formal properties. Precedence is antisymmetric (if A precedes B, B can't precede A) and anti-reflexive (A can't precede itself). C-command is non- (rather than anti-)symmetric – X and Y may or may not c-command each other – and it is reflexive. The difference between c-command and precedence, for the RH, is that, while the former is a function of constituency, the latter emphatically is not. It will be clear that the RH's take on the distinction-and-relation between linearity and structure is far removed from Kayne's 'antisymmetry of syntax', which 'attribut[es] certain properties of linear order to hierarchical structure' (1994: xv).

2006 (BR&P). Although we refer to it as 'SF' for convenience, it involves no 'fronting' in the RH. Linear precedence generally is not achieved derivationally; it is determined directly, declaratively, by m-representational convention. The order seen in (6), for example – adapted here as (6′) – simply is the order ordained by Icelandic m-representational conventions.

(6′) /Ef/ > /**gengið**/ > /er/ > /eftir/ > /Laugaveginum/ …
    if   walked   is   along   the-Laugavegur
    'If pro_{arb} (one) walks along the Laugavegur …'

As mentioned, (9)–(10) above are conventions of Icelandic. However, the position of /gengið/ in (6′) is consistent with neither. So what convention might be responsible for the position of /gengið/ in (6′)? Following an intuition of previous researchers on SF, particularly Maling (1990), BR&P suggested that the crucial feature of modern Icelandic is that is it is a 'verb-second' (V2) language. We suggested that there must be a declarative convention that determines this, having the form (11a).

(11) (a) *Icelandic verb-second convention* – $\Re[V_{fin}]2$
    The m-representation of a finite verb ($\Re[V_{fin}]$) must appear as the second element in the m-representation of the IP ($\Re[IP]$) that contains it.[36]

Before elaborating on this approach to verb-second and SF, we need to discuss the precise form of the relevant convention. As it stands, (11a) needs to be revised. At issue here is the status of '(finite) verb'. BR&P assumed that 'verb' named an element of the syntactic *representatum*. However, we have now made more explicit a speculative implication of the RH: that the *representatum* is in fact the syntax of the C-I system itself (7.2 above). In the light of this, it is not obvious that (finite) verbs as such should be considered elements of the *representatum*. An alternative possibility (the only alternative, given the RH) is that 'finite verb' is in fact a representational term, a label for the phonetic m-representation of a predicate combined with the m-representation of time reference and/or mood. In fact, what makes the m-representation of a predicate a 'verb' specifically is the capacity to combine in this way, i.e. be 'tensed'. (See footnote 40 on /can't/, below.) On these terms, '(finite) verb' is a classificatory label for phonetic material having regard to its m-representational function (as an m-sign).[37] With 'finite

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[36] Like any m-representational convention, (11a) (and its reformulation (11b) below) makes reference to syntactic objects but it does not manipulate (or even apply to) them. And, while it applies to phonetic m-representations, it does not in itself position any m-representation (much less move it). The convention merely ordains where (Icelandic) speakers should position it in the temporal string.

[37] We effectively adopted this approach with the 'definite article'. Furthermore, on these terms (and given the signing theorem) there's no objection to calling /can't/ 'a negated modal verb' (compare section 5.2 above). Notice that, on these terms, 'noun', 'verb', etc.

verb' so interpreted, (11a) should be reformulated in terms of the finite verb itself:

(11) (b) *Icelandic verb-second convention – /V$_{fin}$/2*
        If the m-representation of a clause ($\Re$[IP]) includes a finite verb, it must appear as the second element in $\Re$[IP].[38]

We will adopt (11b) but refer to just '(11)' where the distinction between (11a) and (11b) is irrelevant.

In BR&P we proposed that the SF effect arises from the interaction of (11) with other conventions. We assume that the m-*representatum* of (6′) is the structure partially represented in (12):

(12)   [$_{CP}$ EF [$_{IP}$ PRO$_{ARB}$ ER GENGIÐ EFTIR LAUGAVEGINUM]][39]

A convention of Icelandic particularly relevant in this context is (13).

(13)   PRO$_{ARB}$ is not m-represented.

Given (13), blind adherence to conventions (9)–(10) would dictate that the m-representations of the elements of the IP be ordered as in (14):

(14)   *…/er/ >/gengið/ >/eftir/ >/Laugaveginum/

But this doesn't conform to convention (11) and is an ill-formed PF in Icelandic.

By contrast, (6′) (/gengið/ > /er/ > /eftir/ …) does conform to (11). Notice that adherence to convention (11) here results in (9)–(10) being overridden (which is why they are only 'default' conventions). We reiterate here that the order in (6′) involves no operation affecting any of GENGIÐ, /gengið/, ER or

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are a legacy of the structuralist view whereby they serve a classificatory function over pieces of phonetic material in aid of (linguists') distributional statements. In the RH, such 'syntactic categories' are epiphenomenal. See the next footnote, and section 8.3 on 'syntax' in the context of Jackendoff's Parallel Architecture proposal.

[38] It might be thought that this attributes (and that the RH needs to attribute) syntactic structure to the phonetic m-representation itself. Not so (the point is crucial though subtle). Representationally, we have a mere (i.e. syntactically unstructured) sequence of m-signs. The intuition that phonetic sequences have structure is what drives the DIP assumption, but that intuition derives from projecting onto linear representations what we know of their structured *representata*. In any m-representation of a structure, there will be sub-sequences of m-signs that m-represent constituents of that structure (but notice that /yes/ and /no/ are counterexamples). It doesn't follow from this that the representation itself has that constituency. (If it did, 'structural ambiguity' would be impossible; see above on parsing.) In the present context, then, $\Re$[IP] is simply the ordered sequence of elements that m-represent the elements of the C-I IP. In that sense $\Re$[IP] is an epiphenomenon. (Incidentally, BR&P discusses the (framework-independent) question of what (sequence of) elements can count as 'first' in determining 'second' position.)

[39] Elements of the C-I system are not, of course, words of Icelandic or any particular language, nor do they have a linear order. However, we need somehow to physically (m)represent these elements on the page and have chosen to use the m-representations employed by the language under discussion.

/er/. (6′) simply IS how (12) is m-represented, given the conventions of Icelandic.

Interestingly, although this account denies that any displacement operation is involved, it indirectly reconstructs what might be regarded as a 'displacement intuition'. It is only against the backdrop of conventions (9)–(10) that the order in (6′) might be seen as in any sense 'a displacement' (it couldn't be regarded as such in a language lacking these conventions). So, insofar as there is a 'displacement' intuition in connection with (6′), it is a COMPARATIVE rather than a DERIVATIONAL phenomenon. It is a matter of comparing the position /gengið/ would have had, given just conventions (9)–(10), and the position it actually has, given the overriding of (9)–(10) by convention (11).

It is important to note that, even though overridden in (6′), conventions (9)–(10) are still operative there. Convention (10) requires that structural relations in the C-I system be m-represented (matched by the transitivity of linear precedence) to the greatest extent consistent with the overall system of conventions. (9)–(10) are best adhered to in (6′). Other imaginable m-representations that conform to (11), but depart from the order ordained by (9)–(10) to a greater extent, are ill-formed:

(15) * … /**eftir**/ > /er/ > /gengið/ > /Laugaveginum/
(16) * … /**Laugaveginum**/ > /er/ > /gengið/ > /eftir/

Representation (15), for example, overrides (9)–(10) with respect to two structural relations, that between EFTIR and ER and that between EFTIR and GENGIÐ (three relations in (16)). (6′), by contrast, overrides (9)–(10) with respect to just one structural relation, that between GENGIÐ and ER.[40] It is the availability of (6′) that makes for the ill-formedness of (15) and (16).

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[40] Similarly in English with (8′) above (section 7.4). This is a complex matter (see Cormack & Smith 2000 and references therein) which is beyond the scope of the present paper. Briefly and speculatively, then: the overriding of (10) in (8′) seems to arise from an interaction of conventions which (a) yields the word /can't/ and (b) ordains that it be placed immediately after /you/, the m-representation (ℜ) of the (PRO_ARB) subject. As regards (a): English allows for two main ways of m-representing the modal predicate [POSS]: either ℜ of JUST that predicate – giving /possible/ – or combined ℜ of [POSS] and [PRES/PAST], giving /can/, /could/. In (8′) we have ℜ {[POSS], [PRES]}. Another convention of English is that ℜ of wide-scope negation (not constituent negation) is a contractable enclitic on ℜ [PRES/PAST] – and thus, in our case, on ℜ {[POSS], [PRES]}. Hence /can't/. Both of those conventions contrast with convention (10) in not being 'strictly' representational (see below). As for (b) – the linear position of /can't/ – assuming (with C&S) that the relevant C-I properties are structured as in (8′b), then convention (10) is best adhered to by placing /can't/ immediately after ℜ [SUBJECT]: although this is not consistent with what (10) would ordain for ℜ [POSS], it is consistent with what it would ordain both for ℜ [PRES] and for ℜ [NEG] (assuming with C&S that [NEG] instantiates [POL(ARITY)] in this case). Thus, although overridden with respect to ℜ [POSS], convention (10) is still operative in (8′). (A further complexity is the (stylistic) choice of /you/ to m-represent the PRO_ARB subject and its role in all this. Choices avoiding /you/ (*It is not often possible to bribe officials* and *Bribing officials is not often possible*) permit greater or total adherence to convention (10).) The competition among conventions

From this rather simple account of so-called 'SF', both the 'subject gap' constraint (Maling 1990, Holmberg 2000) and the locality constraints on SF (Maling 1990, Jónsson 1991, Holmberg 2000) fall out automatically. BR&P discusses these and further issues, including the optionality of SF in certain (other) cases and its complementary distribution with expletive *það*.

All these conventions are 'representational' in the weak sense that they contribute to the determination of what in Icelandic counts as a well-formed phonetic m-representation. But (11) differs from (9)–(10) in not exploiting linear order specifically in aid of m-representing structural relations. That is to say, no property of C-I structure is m-represented by /V_{fin}/'s being in second position. In that sense, convention (11), and the order it gives rise to, are not 'STRICTLY representational'.

The motivation for strictly representational conventions such as (9)–(10) is self-evident, and, to that extent, they explain themselves. Not so with non-strictly representational conventions like (11). Why should Icelandic, among other languages, have a V2 convention? (Equally (see footnote 40), why should English m-represent wide-scope negation by a contractible enclitic, resulting in (8′a)'s not-strictly representational order?) Like Maling, we have explained the SF effect in terms of V2 but do not purport to have explained V2 itself, i.e. the rationale of convention (11).[41] In fact, we are not aware of any persuasive explanation for it.

Conceivably convention (11) has no SYNCHRONIC explanation. In that case, we believe, an m-representational account of V2 would have the advantage over a realizational (and thus syntactic) account of it. Conventional systems for the m-representation of L (i.e. particular languages) are socio-cultural inheritances. It is not inevitable that every synchronic phenomenon in a $CSPR_{(L)}$ should have a synchronic motivation (explanation). Presumably, the initial development of V2 in Germanic was prompted by some factor, and there are various hypotheses; see Kiparsky (1995: 3.3). Under an RH approach, a V2 convention could persist in a community after the original motivation for it disappears. There would be, in this scenario, no 'deeper' synchronic explanation to be found. Nor, in the RH, could that deeper explanation be syntactic. In the RH, linear order is not a realizational epiphenomenon of the syntax, but a first-order phenomenon in the representational domain.

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illustrated here and in the text is reminiscent of Optimality Theory (e.g. Prince & Smolensky 2004). Indeed were we to assume that OT's 'GEN' generates purely syntactico-semantic objects and that the constraints of OT concern, not those objects themselves, but how they are phonetically m-represented (in a given language), Optimality Theory and the Representational Hypothesis would be highly consonant. However, although this strikes us as a plausible way of interpreting the relation between OT's 'GEN' and its constraints, this is not how OT is generally conceived.

[41] Clearly, if adhered to, V2 aids parsing, but then so would ANY (strictly or non-strictly) representational convention.

This contrasts with what is implied by the double-interface, realizational framework of assumptions. If synchronic order, including V2, is a realizational epiphenomenon of the syntax, that framework is committed to there being (and to finding) a 'deeper' explanation for it, in terms of a synchronic syntactic derivation. But then two questions arise, neither of which has been given a wholly satisfactory answer. What syntactic position does the verb move to, synchronically? What synchronically triggers the movement? These are the syntactic 'engineering' problems noted particularly by Anderson (1993) (see BR&P for further discussion).[42]

In Part One above we dwelt at length on the problems posed by SF for the double-interface, realizational conception of the syntax-phonology relation. We won't labour them again here. Conceiving of the relation (non-derivationally) as m-representational and, in the light of that, clearly separating properties of the phonetic *representans* and those of the C-I *representatum* motivates an enriched ($\Re$-) concept of phonology within which the SF effect can be conceived of as a purely linear (not syntactic) phenomenon, treatable in $\Re$-phonological terms. Given the signing theorem, SF presents no interpretative problem: regardless of its linear position, /gengið/ just is – and is inevitably interpreted as – the m-representation of a C-I property in whatever structural C-I position that property is Interpreted.

8. COMPARING THE RH WITH OTHER PROPOSALS

We have argued that the double-interface assumption is not conceptually necessary, discussing an alternative possibility from which it follows that syntax is of necessity phonology-free and outlining a richer (representational) concept of phonology. We now consider three other proposals that have been claimed to keep syntax phonology-free. In each case, we ask whether that claim can be supported and whether it offers a genuine alternative to the Saussurian legacy embodied in standard generative grammar, the DIP assumption and realizational phonology.

8.1 *Lexeme–Morpheme Base Morphology* (*Beard 1995*)[43]

Beard's LMBM model incorporates an idea close in spirit to the RH in that it posits a separation of abstract grammatical properties on the one hand and, on the other, the phonological properties that 'mark or 'express' those properties. This is LMBM's Separation Hypothesis. However – crucially – it

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[42] Chomsky's (1995a: 368) proposal that phonology is responsible for V2 is a response to the problem. This is consistent with the RH – but see our earlier remarks on 'PF movement' and 'phonology'.

[43] What follows is the merest sketch of Beard's rich and elaborate model, for purposes of comparison with the RH. All references are to Beard 1995.

implements this idea only in one area of grammar: in its treatment of 'morphemes', not 'lexemes'. The 'lexeme' ∼ 'morpheme' distinction lies at the heart of LMBM and has consequences that distinguish it from the RH.

LMBM's lexemes are free, OPEN-CLASS objects. They are necessary triples of mutually implied properties: semantic, syntactic, and phonological. In short, they are double-interface objects ('bilateral', for Beard), 'the direct articulation of meaning by sound in the Saussurian sense' (Beard 1995: 45). Syntactically, they are N, V and A. Phonologically, they are stems. They are listed in the lexicon and enter the syntax. It is in admitting of lexemes, so defined, that LMBM contrasts with the RH.

In addition to the above inherent (including phonological) properties of lexemes, purely abstract – i.e. phonology-free – CLOSED-CLASS grammatical features (e.g. gender, person, number, (in)definiteness, tense, modality, aspect, voice, negation, etc.) can accrue to lexemes, either in the lexicon or inflectionally by amalgamation or agreement (copying) in the syntax. Separate from the syntax – derivationally following it – is the Morphological Spelling (MS) module. The function of the MS module is variously described as being to 'express', 'mark', 'spell', 'realize', 'symbolize', 'interpret', 'map' – in phonological terms – the accretion of closed-class (abstract) grammatical features to nodes instantiated by lexemes. The MS module does this by modifying (or not) the phonological form of lexemic stems.

This is how LMBM's 'morphemes' enter the picture. In contrast to lexemes, morphemes are NOT double-interface Saussurian objects. A morpheme is the purely phonological spelling-out (expression, marking, realization, etc.) – by the MS module – of the abstract, closed-class grammatical properties. Crucially, morphemes are syntactically/semantically EMPTY. To give a simple illustration: on the traditional double-interface understanding of 'morpheme', English has two distinct morphemes, agentive -er (e.g. baker) and comparative -er (e.g. softer). In LMBM, by contrast, there is just one – syntactically/semantically empty, purely phonological – morpheme: /er/.

It is in this latter idea that LMBM most resembles the RH: mutual independence of purely (i.e. 'empty') phonological properties and purely (i.e. 'abstract') syntactico-semantic properties, the relation between them being effected only by independent rules (RH's conventions) making reference to both. So it is tempting to conceive of LMBM's 'marking'/'expressing' relation as the RH's relation of (m-)representation. Indeed, LMBM's 'morpheme' is equivalent to the RH's m-sign. Its existence is determined in effect by an ℜ-phonology.

But whereas the RH admits only of the m-sign – rejecting the (DIP) s-sign entirely – LMBM operates with both: m-sign for morpheme, s-sign for lexeme. From an RH perspective at least, retention of the s-sign – within a model that (in effect) concedes the existence of (indeed argues for) the

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m-sign – is far from optimal, conceptually. Why shouldn't Separation apply across the board, as a general conceptual principle?

Beard himself concedes (p. 368), in respect of lexemes, that 'the assumption that syntax at any point contains phonological material represents an unpleasant violation of modularity'.[44] He suggests that 'the P-representation of lexemes must be pure abstraction' (for comparable suggestions see Chomsky 1995b: 19, Hale & Reiss 2000). This suggestion reflects the tensions surrounding the double-interface s-sign. On the one hand, if it is so abstract as to rid syntax of phonology, can it count AS a 'P-representation' specifically? On the other hand, if it is not that abstract, we still have phonology in syntax.

As it is, then, LMBM's Separation Hypothesis is seen not to entail rejection of the double-interface assumption. So it would not be accurate to think of 'marking', 'expressing', etc. as 'm-representing' – even in respect of LMBM's morphemes. Since a morpheme is a modification of the phonology of a lexemic stem in its syntactic position, the phonology supplied by MS gets to enter syntactic representations. MS phonology, as much as lexemic phonology, thus contributes to the construction of double-interface (c-)representations.

We have focused on bound morphemes, where the lexeme–morpheme contrast is sharpest (see Beard 1995: chapter 15). In LMBM, free morphemes (determiners, pronouns, auxiliaries, etc.) always mark what is markable, in some language, by a bound morpheme. So, like bound morphemes, free morphemes express closed-class features and are supplied by the MS module, not by the lexicon. In common with lexemes, on the other hand, they are indeed free. So they are not modifications of the phonology of lexemes but – like lexemes – must be assigned their own structural positions in syntax.

To illustrate: [POTENTIAL] is in the universal set of abstract closed-class grammatical features. Being closed-class, it is an abstract feature phonologically expressed/marked by the MS module. It is marked by a free morpheme in English, as in (17), but by a bound morpheme in Turkish, as in the semantically equivalent (18).

(17) I **could** come.
(18) Gelebildim (gel **-ebil**-di -m).
      come-POT-PAST-IST/SG

Given that [POTENTIAL] can (in some language) be marked by a free morpheme, it must be assigned its own structural position. The difference between free and bound here lies in whether or not the given feature

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[44] Note that it is not any issue of modularity as such that necessitates the more fundamental and comprehensive 'separation' envisaged in the RH. See below (section 8.3) on Jackendoff's model.

structurally amalgamates with a lexeme. [POTENTIAL] amalgamates with a lexeme in (18) but not (17).

Now amalgamation affects the syntactic representation. Although in LMBM there is a single (universal) base structure underlying (17) and (18), amalgamation (vs. non-amalgamation) derives distinct (surface) syntactic structures (pp. 358–360).[45] The question then is, as Beard (p. 357) puts it, 'how does syntax anticipate the needs of morphology, amalgamating just those projections which will be marked affixally and preserving those which are to be filled with free auxiliaries?' At least from the perspective of the RH (the asymmetry of the relation of m-representation) – if not of LMBM itself (its Separation Hypothesis) – the question is ominous. In neither framework can syntactic structures anticipate how they are to be marked (LMBM) or m-represented (RH).

LMBM defuses the issue by treating amalgamation (traditionally, syntactic movement) as simply bracket erasure and – crucially – treating bracket erasure as an operation of the MS module itself (pp. 357ff.). Now, technically, this does obviate 'anticipation' by the syntax of the different – English vs. Turkish – marking strategies of the MS module. However, it does so only by allowing the MS module itself to ALTER what it was supposed merely to MARK or EXPRESS. In effect, the syntactic derivation carries on into the MS module itself. Whether or not this 'weakens strict modularity' (p. 357) in LMBM's own terms, it certainly distinguishes LMBM from the RH. It is fundamentally incompatible with the RH that syntax, as the m-*representatum*, should in any way be affected by how it is phonetically m-represented (and it is unnecessary, given the signing theorem).

At the root of the contrast between the RH and LMBM (and other models) here is the notion of derivation. LMBM is traditional to the extent that it conceives of the relation between syntax and the phonology of the MS module in derivational terms. Syntactic (c-)representations are, as it were, 'handed over' to the MS module for phonological 'spelling' ('marking', indeed 'realization') of their abstract grammatical features and thereby (cf. bracket erasure) for turning into syntactic surface structures. This derivationality reflects LMBM's overall double-interface character.

M-representation, by contrast, simply isn't a derivational notion. It involves no 'handing over' of syntax to phonology or phonetics (and there is no place in the RH for any notion of 'surface structure'). While the technical distinction between (a) syntactic-anticipation-of-MS and (b) syntactic-alteration-by-MS might be crucial in derivational terms, (a) and (b) are equally impossible (and unnecessary) in m-representational terms.

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[45] In effect the universal is derivationally converted into the language-particular. In this, LMBM contrasts with the m-representational conception of the relation between the universal and the language-particular.

8.2 *Distributed Morphology* (*Halle & Marantz 1993*)

As Halle & Marantz (1993: 111, 121) note, Distributed Morphology (DM) is a framework influenced by LMBM (as presented in Beard 1966, 1991) and it is motivated by the same general kind of empirical consideration, namely lack of 'sound–meaning' isomorphism (pp. 114f.). In DM, as in the RH, the problem is seen to arise from the assumption that p-features enter the syntax and the solution to lie in separating out phonology and syntax. As discussed, this entails rejecting LMBM's 'morpheme'/'lexeme' distinction – and DM rejects it (their note 10). It might therefore appear as though DM indeed represents a more decisive break with the Saussurian double-interface legacy. The question is whether DM in fact achieves this decisive break. In what follows, we argue that it clearly does not.

DM shares with LMBM a derivational conception of the syntax $\sim$ phonology relation. This undermines its claim to have separated the two. Syntactic objects are 'handed over' to a Morphological Structure (MStr; 'MS' in Beard and in Halle & Marantz) component and thence to phonology (for Vocabulary Insertion). MStr has a role comparable to Morphological Spelling in LMBM in that it paves the way for the insertion of vocabulary items. MStr does this by altering the trees handed to it by the syntax, operating on terminal nodes by processes of merger, fusion, fission and head-movement. As in LMBM, then, the syntactic derivation in fact continues on – beyond what is described in DM as 'syntax' – into the MStr component.[46]

It is on the basis of the syntactic structure derived by the MStr component that DM's 'vocabulary items' enter the picture. Vocabulary items are inserted at appropriate terminal nodes in the syntactic tree. This is a phonological operation: in inserting vocabulary items, the MStr component inserts p-features into the tree.

As a result, the general model has two very traditional interrelated properties that distinguish it from the RH: (1) it derives double-interface (c-)representations and (2) the phonology is realizational. As regards (1), while DM may have eliminated phonology from what Halle & Marantz call 'syntax', vocabulary insertion nevertheless yields a c-representation incorporating both syntactic and phonological properties. As regards (2), Halle & Marantz (p. 122) themselves use 'realize' in this context – rightly in our view. Consistent with 'realization', the syntactic representation (derived in part by MStr) determines the gross distribution of p-features. However – METHODOLOGICALLY (as is necessary in realizational models) – it is in fact p-feature distribution that determines the syntax: in DM the function of the

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[46] Indeed, Olatra-Massuet & Arregi (2005: 44) characterize 'Syntactic Hierarchical Structure All the Way Down' – namely 'Morphemes are organized into hierarchical structures determined by the principles and operations of syntax' – as one of two central hypotheses of DM.

MStr component is to get the syntactic representation into such a shape that it can be isomorphically 'realized' by observable PF strings. DM, then, merely offers an alternative (architecturally re-organized) implementation of DIP assumptions.

There is a further respect in which DM implements DIP assumptions. It attributes morpho-syntactic features to vocabulary items themselves. As Halle & Marantz write (p. 122), 'the entries that make up the Vocabulary of a language are each composed of two distinct sets of features: phonological and morphosyntactic/semantic'. In other words, DM's vocabulary items are themselves double-interface objects. See Jackendoff (1997: 90) for comparable observations.

As with the RH, Halle & Marantz insist that syntax is non-linear (1993: 115). However, the fact that DM is a derivational theory (one in which double-interface c-representations are derived) renders the non-linearity of syntax fundamentally problematic. When it comes to Vocabulary Insertion, we have to conceive of LINEAR objects being somehow inserted into NON-LINEAR syntactic structure – and conceive of them being linearly ordered with respect to each other within that non-linear structure. We take this to be conceptually incoherent. In this respect, DM rather clearly illustrates the problems attendant on the DIP assumption and derivationality. Here, again, we would urge that an m-representational view, by contrast, coherently captures both the radical distinction and the relation between syntactico-semantic hierarchical structure and the temporal linearity of phonetic forms.

8.3 *Parallel Architecture*

Ray Jackendoff's (1997, 2002, 2003) Parallel Architecture (PA) proposal and the RH make contact at several points. As noted, both point up the incoherence of double-interface objects ('mixed representations'). This – together with Jackendoff's objection to what he calls the 'syntacticocentrism' of Chomskian models – motivates the leading idea of PA, namely 'representational modularity':[47]

> The overall idea is that the mind/brain encodes information in … distinct formats, or 'languages of the mind'. Each of these languages is a formal system with its own proprietary set of primitives and principles of combination, so that it defines an infinite set of expressions along familiar generative lines … Each of these modules is domain specific … and … each is informationally encapsulated. (Jackendoff 1997: 41)

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[47] For Jackendoff, 'representation(al)' has the (c-)sense usual in generative grammar, not the RH's (m-)sense (2002: 19f.). More on this below. References here are to Jackendoff, unless otherwise stated.

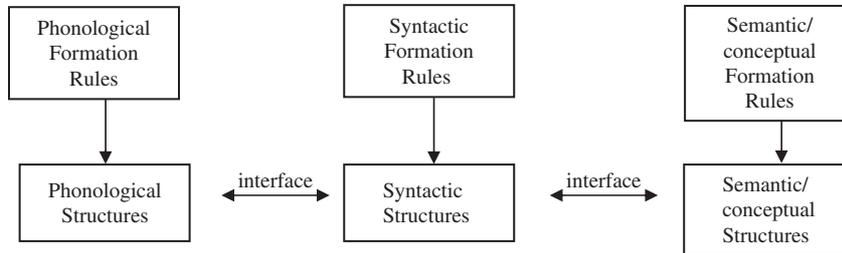| Phonological Formation Rules		Syntactic Formation Rules		Semantic/ conceptual Formation Rules
↓		↓		↓
Phonological Structures	←interface→	Syntactic Structures	←interface→	Semantic/ conceptual Structures

Figure 2
Jackendoff's Parallel Architecture

Three modules are posited: phonological, syntactic and 'conceptual' (1997: 39, figure 2.1) or 'semantic' (2003: 659, figure 3).[48] Separate from these modules are sets of 'interface' rules. These establish 'correspondences' (a) between phonological and syntactic structures, and (b) between syntactic and semantic/conceptual structures.[49] Crucially, 'lexical items' are themselves interface rules (albeit very specific ones). See figure 2.

The PA thus appears to offer an alternative to DIP assumptions. In establishing, by separate interface rules, 'corrrespondences' among the structures, the PA distinguishes objects from relations. It rejects derivational 'handing on' (of syntax to phonology) and thus a realizational concept of phonology. Also, if, by 'its own proprietary set of primitives and principles', Jackendoff means that the respective properties of the structures are, in our sense, sortally incommensurable, then the interface rules must be regarded as conventions. All this – and the PA's (non-'item') approach to 'lexical items', and the rejection of lexical (and therefore p-feature) insertion into syntactic structures (1997: 89–91) – is consistent in principle with the RH.

The question then is whether the PA and the RH are the same idea. While we find much to agree with in Jackendoff's discussions, we believe this is not the case. Here we explore the differences. Inevitably, this involves appraising the PA proposal from the perspective of the RH. The reader must judge whether our observations have any force independently of that perspective.

A fundamental difference lies in the fact that the RH – in its m-representational conception of 'sound–meaning' relations – offers a clear alternative to the mereological pairing of the DIP assumption, one which is

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[48] We comment on 'semantic' vs. 'conceptual' below.

[49] A third set of interface rules deals – without the mediation of syntax – with direct correspondences between conceptual structure and certain phonological structures (e.g. *hello*, *wow*, *ouch*) and with topic and focus when 'marked phonologically by stress and intonation with no specifically syntactic effects' (1997: 95). Since we are not concerned with these here, we have omitted them in figure 2.

incompatible with it. The PA, by contrast, offers only 'correspondence'. This is rather vague. It is even less specific on the nature of the relation than the mereological DIP conception itself and, for that reason, is not actually inconsistent with it.[50] The fact that it is not felt necessary in the PA to articulate any concept of the relation which is clearly incompatible with DIP is among our reasons, further detailed below, for thinking that in fact a fairly traditional double-interface view remains, implicitly, in the conceptual foundations underlying (and at variance with) the architectural superstructure of the PA.

With nothing said about the nature of the 'correspondence' between them, nothing explicitly follows concerning the cognitive function or theoretical rationale of Jackendoff's three modules. In fact, if (as Jackendoff suggests) they are informationally encapsulated, it is difficult to see what their function or rationale is. Why should the mind/brain 'encode information' in such a multiplicity of distinct 'languages of the mind'? This issue arises as a matter of Ockham's Razor and particularly in connection with the PA's syntax module. We will take the modules in turn: the phonological module first, then the conceptual/semantic, and finally the syntactic, in each case addressing its nature and its rationale.

The phonological module of the PA would seem to be either (a) not encapsulated (contra representational modularity) or (b) not warranted. The crucial question here is: what structures does the phonological module generate? Consider a phonological ∼ syntactic correspondence that Jackendoff (2003: 659) gives.

(19) (a)
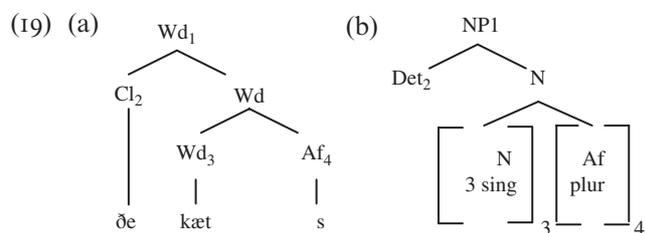
On the basis of examples like this, it might be supposed that the phonological module generates JUST structures that do in fact 'correspond' with syntactic structures. On that supposition, phonology clearly can't be encapsulated with respect to syntax (or vice versa, if the correspondence is symmetric).[51]

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[50] M-representation can't be the relation Jackendoff has in mind, if only because he assumes that the relation effected by the interface rules is symmetric (see figure 2). M-representation, by contrast, is antisymmetric. In some contexts his 'correspond' seems to mean 'is the same as'; see below.

[51] And we need to ask what work remains for the interface rules if it is in virtue of their intrinsic properties that phonological and syntactic structures correspond – as (19a–b) suggests – for then 'correspondence' is tantamount to 'similarity' (contrary to

On these terms, one might as well think of the rationale of phonology as being to specify the phonology-OF syntactic objects, and therefore think of the overall system in realizational, double-interface terms – as globally generating objects which are assigned their various properties by the various modules. Consistent with this interpretation, Jackendoff himself writes: 'In the [PA], a sentence is well-formed when all three of ITS structures – phonological, syntactic, and semantic – are independently well-formed and a well-formed correspondence among them has been established by the interfaces' (2003: 658, our emphasis).

Alternatively, if the phonological module really is informationally encapsulated, it must generate blind, and thus wildly over-generate. It must then be left to the interface rules – for each particular language separately – to select a SUBSET of the p-structures generated and put them into correspondence with syntactic structures. But this leaves the phonological module itself without any function. It implies that all phonology (all the constraints of language-specific phonologies) is in fact located, not in the module, but actually in the (language-specific) phonology ∼ syntax interface rules themselves.

Were we to (i) dispense with the PA's phonological module, (ii) conceive of the interface rules as themselves constituting the phonology, (iii) conceive of them as conventions that refer to producible/perceivable phonetic material rather than to generated phonological c-representations, and (iv) enrich mere 'correspondence' into m-representation – then we would indeed be looking at the (radically separate) 'ℜ-'conception of phonology and its rationale. A phonological system on these terms is not a 'language of the mind' and is not encapsulated; it is a system of conventions for the external/perceptual m-representation of THE 'language of the mind' – and as such necessarily has access to that language. As it is, the PA and the RH seem clearly distinct.

Consider now the 'conceptual' or 'semantic' module. We need to distinguish 'conceptual' and 'semantic' here. Take 'conceptual' first. Regarded as generating pure 'conceptual structures', its nature/function is, in general terms, clear. Such a 'module' is for thought and, being recursive, for cognizing thought itself (one's own and others'). It must (we agree) be autonomous with respect to (innocent of) the PA's syntax and phonology. As 'conceptual-intentional', it HAS semantics itself but it doesn't specify (ISN'T) the semantics-OF anything (see page 597 above). However, on these terms, it is surely not distinct from the central conceptual system itself, and this – by definition – is not an encapsulated module. The term 'semantic', on the other hand, is relational. It suggests that the module is distinct from the central conceptual system in being dedicated to specifying the SEMANTICS-OF

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representational modularity and our construal of the interface rules as conventions). See also below on linear 'correspondence'.

something – expressions generated by the syntax, presumably. But this implies that those expressions have semantic as well as syntactic properties – i.e. in terms of the 'representational modularity' thesis, they are 'mixed representations' (see Jackendoff 1997: 226, note 3). Equally, a mechanism that specifies the semantics-OF syntactic expressions cannot be encapsulated with respect to syntax. Its rationale lies in syntax, being effectively 'interpretative' of it (as in models the PA claims to repudiate). Furthermore, if its function is to specify the semantics-OF syntax, a separate set of syntax∼semantics 'correspondence' rules seems redundant.

If syntactic expressions have properties that are genuinely 'semantic', in the sense of being conceptual-intentional, should they not be the structures generated by the conceptual system itself, as suggested above? As noted, this is consistent with Jackendoff's suggestion that 'we must consider the domain of linguistic semantics to be continuous with human conceptualization as a whole' (2002: 282), but it goes further in implying the non-existence of a distinctive (modular) syntax. However, the PA is committed to the existence of distinctive syntax because the PA attributes to it properties that (we agree) cannot be properties of the C-I system.

Consider, then, the PA's syntax module. The PA is intended as a corrective to the 'scientific mistake' (2003: 654) of 'syntacticocentrism' of Chomskian models. 'Syntacticocentrism' can be understood in two related but not identical ways: (a) as locating all generativity – all combinatorial capacity – exclusively in syntax (see e.g. 1997: 1.3, 2002: 107, 2003: 655) or (b) as assigning syntax a central and necessary role in the articulation of 'sound∼meaning' relations.

It is not clear to us that Chomskian models are syntacticocentric in sense (a) – see Freidin (2003: 677f.). The generativity that has come to be associated with phonology is not inconsistent with those models. Equally, we cannot imagine that anyone who admits of a conceptual/semantic system would deny that it has at least as powerful a combinatorial capacity as Chomskian syntax. That Chomsky himself doesn't explore such generativity doesn't imply that the model itself is syntacticocentric in sense (a).

On the other hand, we agree that Chomskian models are syntacticocentric in sense (b). Furthermore, insofar as syntacticocentrism in sense (b) is intrinsic to the double-interface conception, we have argued at length that this is indeed a mistake. However, it is not obvious that the PA itself is not syntacticocentric in precisely this sense. The PA not only posits a distinctive syntax but assigns it a central position mediating between the phonology and the conceptual/semantic 'modules'. And, as just discussed, those 'modules' can be seen as specifying, respectively, the phonology-OF and the semantics-OF expressions generated by the syntax.

Apart from this mediating function (entirely comparable to the syntacticocentrism of double-interface models), it is deeply unclear what the rationale of the PA's syntax module is. Were this module really encapsulated, the

very nature of its primitives would be obscure. What is an encapsulated syntactic property, in and of itself? Consider (20).

(20)

Phonology:	Syntax:	Concept/Semantics:
ðə	**Det**	**DEF**
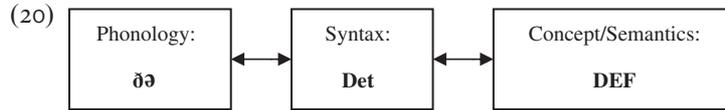
The respective natures of /ðə/ and DEF are clear enough. (And so, we claim, is the unmediated m-representational relation of the former to the latter.) But Det? On the traditional DIP view, Det is indeed an object distinct from both the phonological piece /ðə/ and the concept DEF: it is the (γ-) object constituted by their mereological pairing. The two members that comprise the pair are properties-of that object. Its phonology provides for its phonetic 'realization'. This account of Det – and distinctive syntax in general – is coherent iff (and it's a big 'iff') you accept the DIP assumption. But it is wholly at odds with what is claimed for the PA, and Jackendoff offers no other account. It is difficult not to conclude, then, that the nature/rationale of Det in the PA (and its 'syntax' generally) is exactly as in the syntacticocentric double-interface models the PA claims to reject.[52]

This is even more obvious with the object subscripted '4' in the syntactic representation (19b). It is constituted by two properties, neither of which is syntactic in a sense consistent with the claims made for the PA: 'Af' (for Affix) is morphophonological and 'plur' is conceptual. In other words, it is distinctively 'syntactic' – distinct from both the phonology and the concept – precisely because it is the object constituted by their pairing, just as in the double-interface conception of the rationale of syntax.

As noted, Jackendoff assumes that his syntactic representations have linear order. This, we have argued, radically undermines the claimed rejection of 'mixed [double-interface] representations', by importing into syntax a property only interpretable as phonological/phonetic. Indeed, Jackendoff straightforwardly acknowledges this (and generally the DIP nature of the PA's 'syntax') when he writes 'for the most part syntax has the linear order of phonology but the embedding structure of semantics' (2003: 659).

In this connection ('for the most part', in particular), consider:

(21)   If syntactic constituent X_1 corresponds to phonological constituent Y_1, and syntactic constituent X_2 corresponds to phonological constituent Y_2, then the linear order of X_1 and X_2 preferably corresponds to the linear order of Y_1 and Y_2. (Jackendoff 1997: 28)

When the syntactic and phonological orders in fact 'correspond', 'correspondence' has to be construed as meaning 'being one and the same' – in

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[52] We discuss the status of 'Det' in the RH below (but see above (footnote 37) on 'verb' and the 'definite article').

which case a separate 'interface' statement of the fact is surely redundant. Paradoxically, then, the force of this rule would seem to lie in 'preferably', which acknowledges that in fact there is no necessary 'correspondence' (identity) between syntactic and phonological order. But this raises several questions. If the linear order of syntax IS that of phonology, why is it MERELY 'preferable' that they 'correspond'? On the other hand, if they don't correspond of necessity, why should it be 'preferable' that they do? Equally, on what basis do we assign to the syntax any other (less 'preferable') order? Conceptual structure is of no help here since it is agreed that conceptual structure has only 'embedding structure'.[53]

Comparing the PA and the RH, notice that, while the RH is SEPARATIONAL as regards phonology/phonetics ∼ syntax (on sortal and m-representational grounds), it is not MODULAR in spirit or implication – quite the reverse, in fact. And, as regards 'syntacticocentrism', the RH is positively 'centrifugal' by comparison with the PA. Our claim has been that, with phonology excluded from (because m-representational of) syntactic structure, distinctive syntax loses it rationale (and the above discussion of Det would seem to bear this out). It is precisely its role in serving the PF interface that distinguishes this 'syntax' from that of the C-I system. In terms of the RH, there are just two conceptually admissible domains: that of the phonetic m-*representans* (determined by ℜ-phonology) and that of the syntactico-semantic m-*representatum* (i.e. conceptual structure). Properties thought in the double-interface tradition to be syntactic-as-such have to be re-assigned in terms of that m-representational distinction/relation – disentangling the m-sign from what it is a sign of. In the RH, this is a conceptual imperative, not a matter of the division of labour (Jackendoff's 'balance of power') among constituent 'components' (2003: 659) of a single system.

Jackendoff writes: 'Suppose … that syntactic structures had no properties of their own, and that all their properties were derivable from phonology and conceptual structure. Then the syntactic formation rules in [figure 2] would in effect be vacuous, and we would have a maximally minimalist syntax – the sort of syntax that the Minimalist Program claims to envision' (1997: 39f.). We agree entirely. However, he considers the possibility only 'for fun', dismissing it (ibid.) on the grounds that 'there are certainly properties of syntactic structures that cannot be reduced to phonology or semantics'. If Jackendoff is correct, this would be worrying – and not just for the RH – because, notice, the above quote effectively equates 'syntax' with the inexplicable. It tells us only what 'syntax' ISN'T. What 'syntax' IS – or could be, once relieved of any double-interface rationale – is completely obscure.

In general it is cross-linguistically variable phenomena that Jackendoff cites as not being 'reducible to phonology or semantics'. The project for the

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[53] We are concerned only with conceptual and architectural issues here, not with the empirical matter of how (21) might bear, for example, on SF or on the ordering of /can't often/.

RH is to show that in fact these should/can be conceived of in terms of ℜ-phonology. Since this is an open-ended project, we will do no more than briefly review some phenomena, cited by Jackendoff, which we have already touched on.

Linear order and syntactic category are phenomena that Jackendoff cites in this connection, including 'where the verb goes' (ibid.). We have argued that linear order is ONLY interpretable as the temporal order of articulatory-perceptual events, determined by the phonology (as the above quote from 2003: 659 concedes). We have also suggested that categories such as 'noun' 'verb', etc. are in fact terms for phonetic pieces having regard to their function as m-signs. For example, 'definite article' in English linguistics is a label for /ðə/. 'Det' classifies a more general set of phonetic m-signs, namely those that have the same linear distribution as (and are thus unable to co-occur linearly with) /ðə/. We deny that these labels name elements in – let alone figure in – semantically interpretable syntactic configurations. As regards 'where the verb goes' (in a particular language), 'verb' must for Jackendoff (as for us) refer to something other than an element of C-I interpretable structure. The only evidence for 'where the verb goes',then, must be where in the linear PF order the verb is heard (in a particular language). On these terms 'verb', even for Jackendoff, names something hearable – a phonetic piece, not a 'syntactic' piece.

Jackendoff also cites 'the presence or absence in a language of *wh*-movement' (1997: 40). In this connection, recall that in section 3.4 above we distinguished between the phonetic (perceptual) phenomenon of '/wh/-displacement' and the syntactic *wh*-chain phenomenon, having scopal effects in C-I structure. As noted, these don't coincide (e.g. in Chinese). Jackendoff must here mean the phonetic phenomenon of '/wh/-displacement': it is this (rather than WH-chains) that may be present or absent in a language. Poole & Burton-Roberts 2004 (and BR&P) argue that, if and when present in a language, '/wh/-displacement' is a PF phenomenon that serves (declaratively, without movement of PF material) to m-represent a distinct syntactico-semantic phenomenon, the WH-chain (formed by internal merge).[54]

As Jackendoff (2003: 659) comments, 'The differences among languages … are not predictable from semantics, and children have to learn them'. We suggest that they could only learn them on the basis of utterance phenomena

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[54] Jackendoff (2002: 5.8–5.9) also cites subcategorization – *eat* vs. *devour* – as 'syntactic' (and thus as not derivable from phonology or semantics): semantically, each is a two-place predicate, but syntactically *devour* is transitive while *eat* is transitive or intransitive. The RH suggests an ℜ-phonological account: simply, English ℜ-phonology specifies that, when the second argument has arbitrary reference, this argument must be phonetically m-represented with *devour* but needn't be so with *eat*. On this account the facts are ℜ-phonological but their explanation may lie in a conceptual distinction. Devouring is telic, eating may or may not be. Possibly, the second argument must be explicitly m-represented with *devour* because the endpoint of a telic two-place predicate is only defined by its second argument.

and how they are m-representationally deployed. The RH suggests a new conceptual basis for, and justification of, Chomsky's (2001: 2) Uniformity Principle: 'In the absence of compelling evidence to the contrary, assume language to be uniform, with variety restricted to easily detectable properties of utterances'.

9. CONCLUSION

In this paper, we have attempted to draw together a range of issues, problems, and questions arising from the supposed necessity of the Saussurian double-interface assumption – the assumption that phonological and syntactico-semantic properties are conjoined in lexical items and more complex structures. We approached this through a critique of Holmberg's feature-dissociation and Cormack & Smith's Split Sign Hypothesis. On several grounds – conceptual, analytical and technical – we have questioned whether such properties should be, or can be, so conjoined. As a counterpoint to the 'virtual conceptual necessity' of the DIP assumption, we have presented – in the Representational Hypothesis – an alternative way of conceiving and modelling 'sound–meaning' relations. We have argued that the implications of the RH radically contrast with standard generative and minimalist thinking, though they would seem to be consistent with minimalist goals. We have shown that several other generative frameworks, although sharing certain aims and concerns with the RH, still perpetuate aspects of traditional Saussurian thought.

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