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Interview with Hagit Borer for Revista Linguística

Entrevista com Hagit Borer para a Revista Linguística

Isabella Lopes Pederneira[1]

In this volume of Revista Linguística, which has articles on the topic "Synchronic and Diachronic Grammatical Studies," I had the honor of interviewing one of the most influential linguists of our time, Professor and researcher Hagit Borer.

Hagit Borer is a professor of linguistics at Queen Mary University of London. She develops research in Generative Grammar and has also developed her own Theoretical Model of Grammar - The Exo-Skeletal Model which shifts the computational load away from the lexical entry to the syntactic structure. She is the author of several books and articles in linguistics, including the trilogy – Structuring Sense – which details the Exo-Skeletal Model.

Borer got her PhD in linguistics in 1981 at the Massachusetts Institute of Technology (MIT), where she was a student of Noam Chomsky. She is a fellow of the British Academy and Linguistic Society of America. In her solid career, she has been developing studies mainly in three sub-areas of linguistics: comparative syntax, morphosyntax, and language acquisition.

Despite her extensive curriculum, she is an extremely attentive person. I also had the opportunity to get to know her welcoming side while I was in Queen Mary to develop part of my PhD under her supervision. This interview shares with all the readers of this volume her generosity in sharing her knowledge in view of her detailed and very enlightening answers.

REVISTA LINGUÍSTICA: You have developed a theoretical framework called the Exo-Skeletal Model (XSM). Researchers in the area frame your model as a constructionist approach. What are the main features that distinguish your theory from recent developments of the generative enterprise, in particular Distributed Morphology (DM)?

HAGIT BORER: It's a family of approaches, not just mine and DM. The general term now mostly in u]se is Constructivism. At least one reasoning behind the term is to distinguish us from Construction Grammar (itself also in various executions) which is another one of your questions.

[1] Departamento de Letras Vernáculas e Programa de Pós-Graduação em Linguística - UFRJ, isabellapederneira@letras.ufrj.br, https://orcid.org/0000-0002-5884-8472.

As for the differences between my specific approach and Distributed Morphology, several things are important to stress. First, DM has now existed for some 25 years, and there are various diverging versions of it some of which are closer to mine and others more distant. Second, there are significant differences between DM with its various executions and my approach, but there are also some important similarities.

In discussing these differences and similarities, I set aside what I consider to be terminological differences that have no empirical consequences, for instance node labelling, and focus on more fundamental differences and fundamental similarities which may have empirical consequences. The fundamental similarity among all Constructivist approaches is the belief that grammatical structure should not be based on the properties of lexically-listed individual words. The idea in general is to minimize and to the degree possible altogether eliminate the syntactic role of the lexical component. Importantly, this concerns properties of substantive vocabulary, not properties of functional vocabulary or features. Because features or functional vocabulary, in most Constructivist approaches, do have grammatical properties. One could, of course, conceive of a model in which functional vocabulary as well does not have a formal role to play, and it does, indeed, play a minimal role in some Construction Grammar approaches, but in Constructivist models functional items, whether features or actual vocabulary items, do have properties. There are disagreements on how exactly to implement this. For instance, the relationship between features and functional vocabulary items is not agreed on. But that's not a distinction between XSM and DM, but a distinction between both XSM and DM on the one hand, and say Nano Syntax on the other hand. For both XSM and DM, there is a "lexicon", but it's a lexicon of grammatical functions and how they are pronounced. Within this view, substantive vocabulary either doesn't play a grammatical role altogether, or has a very limited grammatical role. Most importantly, and the idea all Constructivist approaches subscribe to, is that major syntactic categories (sometimes called Lexical Categories, such as N, V, A) are not listed with substantive vocabulary. To compare, in the lexicon as it comes to us from *Aspects of the Theory of Syntax* (Chomsky, 1965) and subsequent, you have a (substantive) word, let's say *kick*, and the word *kick* has various grammatical properties. First, it is pronounced as /KIK/. Second, it is listed as a verb (V). Third, it comes with a syntactic insertion frame (sometimes called *subcategorization*), which tells you, specifically, that it requires a complement, and that the complement needs to be the nominal (NP or DP) that gets interpreted as the entity that undergoes the *kick* action. Finally, it tells you what *kick* means, say KICK. So effectively, *kick* is a mini syntactic fragment, as it delivers not only sound-meaning relations, but also a piece of syntax, something like (1):

1. *Kick*: V /KIK/, KICK, [$_{VP}$ *kick* NP]

Overwhelmingly, this view has been preserved to this day in both syntactic and semantic approaches, even if NP has become a DP, VP has acquired more complexity (e.g., *v*P or VoiceP) and insertion frames, or subcategorization frames, have been replaced by theta-roles assignment, or other

event roles associated specifically with listed entries, or by non-committal statements such as '*kick* takes a complement'. The original Constructivist agenda brought in the suggestion that this is not the right way to go, and that the right way to go is to incorporate these syntactic properties into the syntax and have both the complementation properties and the categorial properties emerge from the syntactic structure. These are theoretical desiderata that XSM and DM share. However, as I already noted, DM has now become a family of approaches, rather than just one, and differences relative to these desiderata have emerged. While all Constructivists approaches I am aware of do subscribe to the view that major categories are not listed properties and are syntactically derived, and that listed (non-functional) items, roots, are a-categorial, a lot of contemporary DM executions nonetheless ascribe to roots the ability to take syntactically projectable complements (most influentially HARLEY, 2009, 2014), and others go the extra step of assuming that these complements track the lexical semantics of the roots (EMBICK, 2004; ALEXIADOU *et al*., 2006). I don't subscribe to these views, and continue to maintain that roots have neither syntactic properties, nor syntactically consequential semantic ones. A similar take on the absence of any syntactic or semantic properties for the root is found in First Phase Syntax (RAMCHAND, 2008), although couched within a syntactic approach to event structure which differs from mine.

That's one distinction. The second distinction which is quite fundamental is the interaction between roots and phonology, and by extension, the relationship between syntax and phonology. Relative to this question, XSM is not just different from DM, it is also different from (mainstream) Minimalism. As I have argued at some length, roots are no more than a package of phonological information, beyond which they have no properties. We could think of this phonological package as filling a hole at the bottom of every extended projection, where the phonological information does little beyond tracking the history of the root. In contrast, in many (although not all) DM-type approaches, roots have semantic or syntactic properties, but not phonological ones (see the debate on root suppletion in DORON, 2014).

Even more significant, however, is my view on the interaction between phonological representation and substantive word meaning, what I call *Content*. Within classical generative approaches, words are listed in the lexicon with their meaning. For the entry of *kick* in (1), KICK is in reference to the conceptual meaning of *kick*, and is a shorthand to some specific lexico-semantic representation. The lexical semantics of KICK or other listed substantive vocabulary, however, is only one part of our understanding of the semantic component available in natural language. The other part is that which is computed by formal semantic representations as constructed by the syntax and the properties of grammatical formatives, or grammatical *functors*. For instance, if you take something like "*the*" and you wish to characterize its relationship with the emergence of definite descriptions, we could (informally) say that *definite description* is a semantic meaning that is constructed through the semantic properties of the functor THE, but *definite descriptions* do not exist independently of the syntactic structure, in this specific case DP. By contrast, the listed lexico-semantic properties of

kick, KICK, are by assumption structure-independent. When you look at the classical "GB picture" as well as much of Minimalism, you see that although both lexical semantic meaning and formal meaning are integrated into the grammar, they emerge in distinct ways. One is associated with listed lexical entries and is unaffected by its syntactic environment, and the other is constructed through post-syntactic interpretation assigned to the syntactic output, in a component historically referred to as LF but nowadays mostly referred to as the 'conceptual-intentional interface'.

Turning now to the Constructivist perspective and its anti-listing agenda, there are little immediate consequences for formal semantics from the diminished syntactic information associated with listed substantive items, as formal semantics depends primarily on functors and on structures. However, if we are to eliminate lexico-semantic and syntactic word-specific (or root-specific) meaning, leaving, effectively, only /KIK/ in the listed entry of *kick*, an important question immediately emerges concerning the way in which /KIK/ and KICK come together, as in most utterances, /KIK/ does mean KICK.

Because the overwhelming tendency within generative approaches from at least 1977 onwards has been to severely curtail the impact of phonological information on both the syntax and the interpretational component, the overwhelming Constructivist tendency has been to assume that the emergence of the interpretation which we refer to as KICK must be negotiated by the very same component in which formal semantics is negotiated, be it LF, or the Conceptual-Intentional interface, and as such, it crucially has no access to any phonological information. That, I think, is a mistake. Formal semantics, as negotiated through the syntactic and semantic features of functors and syntactic structures has little obvious use for phonological information (with some residual issues such as *focus* negotiable through a feature spellout system). Specifically, if we assume that 'intentional' is in reference to the formal-semantic computation, viewing it as a post-syntactic interface with no phonological properties appears to be on the right track. Not so, however, for what, broadly, we can assume to come under the heading 'conceptual', which, I assume, covers aspects of meaning, Content, related to the conceptual system, to world knowledge etc. The "conceptual interface", and this goes back directly to Saussure, is the sound-meaning relation or more accurately the sign-meaning relations. If we take as our starting point the T-model as it now stands, this means that the 'conceptual interface' is not at the 'end' of the LF branch, but is linked to some well-defined representation on the PF branch. This is something that I have done some work on reported in Borer (2013, 2014), and this interface with PF is the topic in which I am presently mostly interested. This difference on how to model the emergence of conceptual Content, and how the phonology interacts with both syntax and Content is thus a significant difference between XSM and most Minimalist or Minimalist-oriented approaches, including DM.

There are additional differences. For instance, how exactly is categorization negotiated in a system with a-categorial roots, or what is (or isn't) the distinction between inflectional morphology and derivational morphology, or whether there could exist roots in the structure that remain uncategorized. Although these are important issues, they are less intrinsic to these distinct approaches, but rather

concern issues of execution that ultimately should be decided on empirical grounds. I note one more important matter, concerning the comparison with DM. There are a lot more DM-practitioners than XSM-practitioners, and so there are, in consequence, many more well-developed accounts of phenomena that XSM at present is entirely silent on. One important example is the ongoing research on the licensing of non-direct arguments such as applicatives and others (see PYLKKANEN 2008 and much subsequent), a topic on which XSM approaches have been so-far silent. To the extent that much of this work licenses non-direct arguments through dedicated functional structure it is certainly compatible with fundamental XSM approaches. Not having studied the issue in detail, however, I cannot comment on the extent to which some assumptions or results of that research could be adopted, as such, with XSM.

REVISTA LINGUÍSTICA: Many linguists compare your theory with Construction Grammar due to templates as syntactic structures. Could you clarify the differences between your approach and the one developed by Fillmore?

HAGIT BORER: Ok, this is not just me, this is the Constructivist family in general. And just a little historical note: when Constructivism, at the time it was called Neo Constructionism or Constructional grammar, first came on the scene in the late 90's or early 00's, we did try, collectively, as a group with a shared research agenda, to reach out to Construction Grammar communities. I was personally very enthusiastic to pursue the links, and so were, for a brief period, adherents of Construction Grammar. I remember a visit in 1999 to the University of Illinois at Urbana-Champaign, when Adele Goldberg was still there, and we were both very excited to discover shared theoretical interests. I still have her book with her dedication 'to a fellow constructionist' which she gave me at the time. That didn't last, however, as by the early 00's, Construction Grammarians were already seeking to distance themselves from Constructivism. I recall an article from about 20 years ago, possibly by Laura Wagner, which explicitly stated that the affinity Constructivists (as presently named) find with Construction Grammarians is on topics which are important to Constructivism, but which are peripheral to Construction Grammar, and on the issues that are important to Construction Grammar, there is a wide gap. The approaches are indeed fundamentally distinct in their underlying assumptions, but nonetheless, as there are significant commonalities in the empirical domains investigated, it would have been better had it been possible to pursue both similarities and differences in a cooperative fashion.

As for the differences, a major assumption that is built into Construction Grammar is that there are linguistic objects we can call *Constructions*, which are defined primarily through their communicative function or their discourse function. Take, for instance, *passive*. What does this term mean? In mainstream of Generative Grammar (from early transformational grammar, through Relational Grammar to GB onwards as well as in strictly lexicalist approaches such as LFG), *Passive* is not a linguistic object as such. Rather, the term refers to well-defined grammatical structures and/or

operations. Typically, some sort of a grammatical demotion of the logical subject and a grammatical promotion of the logical object. But if you look at natural language, you find that there are multiple ways of foregrounding the object and backgrounding the subject which do not involve grammatical demotion or promotion. Topicalization, emphatic stress, impersonal (active) constructions with an indefinite pronominal subject, *se* constructions in Romance that serve to communicatively demote the subject and promote the object, and so on. If you look at this picture from the perspective of Constructions as defined by communicative functions, the syntactic differences between these configurations are noted, of course, and may have some grammatical consequences, but at the end of the day, these syntactic differences are explicitly assumed NOT to be relevant to either the overall understanding of these constructions, their use, or their acquisition. Importantly, their *compositionality* is either denied or judged to be irrelevant. What matters is that a particular structural configuration is associated with a particular discourse function, call it Passive, and the relationship is arbitrary and listed. In one language, it may have the English syntactic structure; in another, it may have a completely different syntax, but its discourse function would be the same. This, for Construction Grammar is not only a legitimate result, but a desirable one, as the approach explicitly subscribes to the existence of a 'Constructicon', a dictionary of constructions with their discourse function and meaning as a whole, alongside the possibility of language-specific 'Constructicons'. From that perspective, it is not surprising that the most important evidence for Construction Grammar comes from idioms, where meaning is, indeed, listed in conjunction with non-compositional structures. For *kick the bucket*, to take a classical example, a compositional approach is clearly either hopeless or circular, thus suggesting that at least sometimes, compositionality is not necessary. For that reason, many Construction Grammarians have worked on idioms or on idiomatic fragments (GOLDBERG, JACKENDOFF, and many others).

As an aside, it is really Chomsky (1965) who should be 'blamed' for the emergence this fundamentally non-compositional approach that associates meaning with whole phrases or even sentences, rather than composing it from the meaning of terminals. When one looks at insertion frames such as the one in (1), one sees that 'lexical entries' are in fact listed 'mini constructions' which are bigger than a terminal. But if one is allowed to list fragments of structure of a phrasal size, why stop with (1)? Why not push this approach to its logical limit, and say that the fundamental building block is exactly listed fragments of grammar of phrasal size or bigger, with their fully specified meaning, non-compositional meaning?

Returning to the comparison of Construction Grammar with Constructivism, this is a major difference, as Constructivist approaches are extremely compositional. More compositional than traditional lexicalist approaches, in fact, because so much is invested in the interpretation that emerges from composing functors, functional items. This is not just an issue of execution or formalism. Rather, this is a fundamentally different assumption about what the human linguistic ability consists of. For Construction Grammar, the human linguistic ability capitalizes on memory and the ability to store not just words, but chunks of structure with meaning. We do know human memory is rather good for

this kind of things and that is also the lexicalist assumption – that the role of rules is not as significant as the role of memorized lists. In contrast, Constructivist approaches adhere to the idea, which, I think, is fundamental to generative grammar and to structuralism in general, that the human linguistic capacity is first and foremost compositional, and rule driven. To be sure, *kick the bucket* is listed as such with its meaning. The question, however, is that of balance. For Constructivists, the question concerns the residue that is left once all compositional rule-driven operations are exhausted, and how to represent it. Conversely, for Construction Grammarian the question concerns the pervasiveness of compositional phenomena, and the danger of massive redundancy that non-compositional systems risk. In more concrete terms, one approach subscribes to the belief that we need to discover the rules, and that exceptions, by and large, signify the need to return to the drawing board and come back with better rules. Exceptions, in other words, define a further research agenda. The other approach highlights the fact that there are exceptions, possibly quite many, and because listing is inevitable for these, listing can be further extended making rules redundant.

REVISTA LINGUÍSTICA: Constructivist models have as main feature the fact that (certain aspects of) lexical content of lexical items is negotiated with the syntactic environment in which such items occur. In your theory, we can see that the morphological marking itself depends on the syntactic structure. How do you delimit and formalize the grammatical rules within this context?

HAGIT BORER: I am not sure I fully understand this question, as I no longer assume that lexical items have any syntactic properties?

REVISTA LINGUÍSTICA: The question is about the derivation of the sentences. Where, in the sentence can we say that a noun will be a noun, a verb will be a verb? Beyond that, when do these categories receive meanings, formal meanings and contextual meanings?

HAGIT BORER: I addressed at least some of it in the answer to the previous question, but to recap, I think that the essence of the root is some phonological properties, and these are not just phonological properties of the root in isolation, but also the root's ability to select particular allomorphs of functional elements that merge with it, and the ability of functional items that merge with the root to select a particular allomorph of the root. There is a lot of remarkably interesting discussion on such selection and the locality conditions that apply to it. That has not divided XSM from various DM proposals. There is a lingering puzzle here which, likewise, both approaches share, as it is not clear how to state local syntactic conditions on post-syntactic phonological insertion. This problem exists not only for DM with its late insertion, but for XSM as well. Although in XSM roots come with phonological properties, the phonological properties that are associated with the root are not a phonetic representation, but a reference to a phonological set of properties that are only fully articulated post-syntactically. For me, the association of roots with phonological properties is a device intended to ensure phonological faithfulness, which is to say, to ensure that the syntactic derivation

will *not* fundamentally change the phonological properties of the root. To illustrate, such faithfulness would altogether exclude or severely restrict a derivation in which e.g., *kill* is derived from the root which in other contexts would be realized as *die* (e.g., assuming that /KIL/ is the realization of [CAUSE [√DIE]], or in reference to specific arguments that I made in Borer (2003) and later, having the nominalization of a root such as √WRITE, say [NOM [√WRITE]] be realized phonologically as *essay*.

There is an ongoing debate on this issue, which centers on the availability of root suppletion, and the best place to consult this debate is Doron (2014). Focusing in particular on the *go-went* pair in English, the question is whether *go* and *went* share a phonologically unspecified root which is realized as *went* (or *wen*) in conjunction with PST, but as *go* elsewhere, or alternatively, these are different roots, and pairs such as *go-went* represent the merging of two defective paradigms – one associated with the root √GO which is lacking past tense realization, and the other with √WEN or possibly √WENT, which lacks realizations for all but past tense. This said, there is nothing in XSM that would prevent the phonological properties of the root from including suppletive realizations, as an option. The fundamental reason why I do not opt to integrate such suppletive pairs into the same phonological root package is because despite considerable research on the topic, we still do not have any insight as to why root suppletion (as opposed to root allomorphy) is so typologically rare, why, with few exceptions, most suppletive forms impact light verbs and auxiliaries, and how to precisely block such cases as the nominalized form of √WRITE being realized as *essay.* In view of this, my perspective is that unless proven indispensable, root suppletion is to be excluded. This is the strongest position, and therefore, even if wrong, is the most productive starting point for the investigation.

Turning to the question of Content, where by Content I mean conceptual non-formal meaning, I believe the most fundamental question regarding roots and Content is whether the roots have Content, and beyond that, whether such Content, where it might exist, interacts with the grammar in any way. My answer to this question is an unequivocal "no", and this is quite fundamental to the way in which XSM treats conceptual, so-called lexical Content. But there is no inherent reason to exclude this view within DM (although to the best of my knowledge there are no contemporary DM-grammarians who have fully endorsed it). The closest to this perspective comes from Harley (2014), who subscribes to the view that the root has no inherent meaning, but comprises a set of alternative meanings to be contextually chosen. (See also the alloseme account in MARANTZ, 2013 and subsequent). At the other end of the spectrum, and possibly not quite strictly within DM, we have the position taken by Malka Rappaport-Hovav (see RAPPAPORT-HOVAV, 2017 for representative argumentation), who argued in some detail that roots may incorporate significant lexical semantics erstwhile attributed to verbs, which in turn has a determining role in their emerging syntax (and see BEAVERS and KOONTZ-GARBODEN, 2012 for some perspectives and critique). In the middle of the spectrum, you find people such as Embick (2004) or Alexiadou *et al*. (2006), who classify roots into different lexico-semantic classes, allowing significant underspecification, but nonetheless assuming that such lexico-semantic classes for roots are syntactically informative in delimiting the occurrence of roots in syntactic structures.

A lot of scholars who attribute lexico-semantic properties to roots do so to solve the enduring overgeneration problem for root-based systems, including XSM. In the absence of syntactic and/or semantic properties for the root, the prediction is that any root could occur in any syntactic context, and while this is certainly more so than strict lexicalism would have it, it is not universally so. If roots, or lexically listed items, have insertion frames or lexical semantics that restricts their complementation possibilities, overgeneration vanishes. So, for instance, once we say that the meaning of √THINK allows a propositional complement, but not so the meaning of √EAT, an infelicitous utterance such as #*I ate that Mary arrived late* is immediately excluded.

The cost for approaches that place syntactic restrictions on the distribution of roots, either through listed complementation structures, or through their lexical-semantics, comes from the massive proliferation of available verbal and nominal syntactic contexts for many, if not all mono-morphemic forms in English, by assumption co-extensive with roots. To accommodate such variable behaviour, a lexicalist approach, and one that seeks to derive the syntactic distribution from lexico-semantic considerations, would have to postulate multiple homophonous entries, each with its own insertion entry. This massive redundancy is eliminated by removing syntactic information from listed entries, roots. However, such elimination creates over-generation, as already noted. And XSM, indeed, over-generates.

This said, I have done at least a partial survey of the proposals currently under consideration within Constructivism that attempt to solve the over-generation issue, some mentioned above, and my sense is that they do not solve the problem. All of them exclude some cases of over-generation, but not others, and all of them, conversely, under-generate. To illustrate, Embick's (2004) proposes that roots are either eventive or statives, and only the former can take an event argument. This would suffice to correctly exclude (2a), under the assumption that *know* is stative. However, it would also exclude (2b), as *think* is equally stative, but (2b) *is* grammatical in English. It would also run into problem with verbs such as *touch, love* and others, which emerge as either 'stative' or 'eventive' depending on their syntax, as in (3)-(4):

2. #Jane knows to the store
   Jane thought the book to Mary (following GLEITMAN, 1990)

3. The wall touches the fence (most saliently stative)
   Kim loves Robin (most saliently stative)

4. The wall touched the fence twice (e.g., as a result of the wind) (most saliently eventive)
   Kim loved Robin twice (most saliently eventive)

Alternatively, all such cases of variability, cases in which more than a single insertion frame is available for the same root, but potentially with a distinct overall interpretation emerging, would require postulating that e.g., *think* or *touch* each requires postulating two or more homophonous entries, each with its distinct syntax and semantics. As early as 1994 and in considerable subsequent

work, including work on acquisition, this is exactly what I opted NOT to do, much as I was from the very beginning aware of the over-generation issue. While I made some proposals to resolve at least some over-generation cases through the presence of covert affixation (notably in CHAPTER 10 of BORER, 2005b), I do believe that for Constructivism this remains an open question without a satisfactory solution, as of yet. My own belief is that a full answer must involve a better understanding of word complexity, on the one hand, and the conceptual system and its interaction with computational grammatical systems, on the other hand.

As a digression, I note that we have several thousand years of recorded philosophical discussions on the nature of concepts, to which we can add, in the last 150 years, insights by linguists and psychologists. This it's not exactly a new concern and yet very little progress has been made on understanding concepts, especially in context. That turns out to be a very tenacious problem, and that is before we get to the second tenacious problem, which is the characterization of the relationship between concepts and their labels, a massive philosophical discussion. If you assume that concepts could have a label, call it *word* or sound/sign, the relationship between that label and the conceptual meaning is extremely tricky. One commonly discussed problem has to do with polysemy, where a single label may be associated with two concepts which bear some relation to each other, but are clearly distinct concepts. An example which I give a lot has to do with words/labels such as English *pinch*. Depending on its context, it could mean 'to squeeze between two fingers', or 'to grab', or 'to steal'. It could also mean, as a noun, 'very quick' (*in a pinch*), or 'be in a tight spot', or 'very little' (*a pinch of salt*). Other meanings are available as well. But conceptually, the relationship between 'be in a tight spot' and 'steal' is quite simply non-existent, and between 'squeeze' and 'really quickly' at best yields itself to some vague metaphorical extension. Rather, it seems like the label *pinch* is in reference to what can be accomplished, literally or metaphorically, between two prongs, possibly but not necessarily, fingers, which are in proximity and may close and open. Whether the above description falls under our general understanding of *concept* is highly questionable. But even if it does, that so-called concept has zero value in informing the use of *pinch* in most of its occurrences. This illustrates quite clearly that labelling does not reliably track what we think of as *concepts*. And this is all before we integrate these labels into our grammatical representations, where phrasal factors play additional havoc in our understanding on how to associate these labels with meaning (*subsective and non-sective* readings being one example). It goes without saying that I no more have a solution to these issues than the thousands of scholars who precede me. It does, however, guide my avid conviction that concepts with or without their labels cannot reliably inform our grammatical abilities.

ISABELLA PEDERNEIRA: In Brazilian Portuguese we have many words like that, verbs, name and so on, in Brazilian Portuguese more than in European Portuguese.

HAGIT BORER: That's interesting. I offer the speculation that the distinction from European Portuguese is because Brazilian Portuguese is less canonized. To draw an analogy, it is well-known that children, when they are about 5 years old with nearly adult-like grammar, go through a very creative phase of language use, particularly in creative word formation, and allowing vocabulary items in non-canonical syntactic contexts. Once they go to school, however, they become much more conservative, because there is a lot of social pressure to conform to canonical, established vocabulary.

Returning to the issue of root meaning, and as I noted, the question is whether roots have meaning, and how root meaning (or lack thereof) is handled in various Constructivist approaches. I already noted that in my view roots do not have grammatically consequential meaning, and in the majority of cases, have no meaning of any kind. Substantive meaning, or Content, in XSM, is matched with units that are bigger than the root, and may include multiple derivational affixation (but not inflectional affixes). To be sure, the Content associated with complex forms that are bigger than the root must be listed somewhere, and I cannot see how a list of sign-Content pairs is avoidable in any approach to language that accepts the arbitrariness of the sign. This is not the issue under debate, however. The issue under debate is whether in the sign-Content listed pair, the 'sign' is a root. For XSM, the answer is *no.* The sign in the sign-meaning pair is always bigger than the root, and at the very minimum, includes a categorized root in contexts that do not include sign augmentation (e.g., in XSM, $[_N \sqrt{\text{TABLE}}]$, or within an affixal approach to categorization, $[_N \varnothing [\sqrt{\text{TABLE}}]]$.)

As for DM, what is paradoxical to me is the frequent claim that the DM notion of what a *root* is, is modelled after the root in Semitic languages. Semitic words have roots, which have been argued to be an essential part of the morphological system (specifically of Arabic) at least since the philological investigations in the XVII-XVIII century. It's intuitively very clear that this is the case, especially for verbs, and it is hard to see how the morphological system can be explained without assuming such roots. These roots clearly do not have a category, which makes them an ideal starting point for theories such as DM or XSM that postulate an a-categorial listed item as the basic (non-functional) syntactic terminal.

However, and as has been pointed out by Constructivists and non-Constructivists alike (at the very least starting with ARONOFF, 1994), the Semitic root doesn't have a fixed Content. While some roots appear to give rise to a family of words that have a related Content (including metaphorical extensions), such as $\sqrt{\text{KTB}}$, whose derivatives tend to bear some relationship to writing, my own investigation revealed that when we exclude possible derivational relationship between distinct morphological verbal instantiations of the same root, 40% of cases exhibit some meaning affinity (sometimes quite loose and unpredictable), and in the other 60% of cases there is no meaning relatedness whatsoever. For the 60% of the roots where no meaning relatedness of any sort is attested, it is extremely difficult to see how an alloseme-like account (MARANTZ, 2013, HARLEY, 2014) could be explanatory, as it would require a circular listing of root meaning as based on the attested (unique) meaning of complex morphological forms, effectively boiling down to the (indirect) pairing of complex morphological signs with meaning. To illustrate some of this circularity, one only needs

to look at HARLEY (2014), who proposes that roots, including some that have no independent Content in English, are associated with a particular alloseme in their morphological/syntactic context, frequently unique. For example, the root √CEIVE is associated with the alloseme RECEIVE in the context of the prefix *re-* but with the alloseme DECEIVE in the context of *de.* Not only does that effectively amount to listing meaning as associated with complex listed words, rather than roots, it sometimes requires choosing an allseme for the root in a non-local context. To illustrate, √JECT, an English root, must receive its interpretation in context, as it has none in isolation, and hence the claim would be that √JECT → SUBJECT in the context of the prefix *sub-*, but √JECT → REJECT in the context of *re-*. Consider however the complex English word *conjecture*, presumably with the structure in (5a), or, one might argue, in (5b):

5. a. [[[con [√JECT]] ure]
   b. [con [[√JECT] ure]]

As it turns out, not only is √JECT devoid of independent meaning, so are *conject* and *jecture,* the potential analytic parts of *conjecture*. As meaning, by this system, must come from choosing the correct alloseme for the root, we are forced to the conclusion that √JECT in *conjecture* is assigned the meaning CONJECTURE (either verb or noun) in one of the non-local domain in (5).

For proponents of root meaning, the other potential solution to the fact that a large percentage of roots do not share meaning across their instantiation (60% of Hebrew roots, as noted) would be to claim that e.g., in Hebrew, 60% of the roots have two or more homomorphs, each associated with its own meaning, and each, perforce, independently listed with its unique morphosyntactic insertion frame. Once again, such a system boils down, effectively, to the sign, in the sign-meaning pair, not being the root, but a bigger domain, which is inevitably more complex and categorized. To summarize, meaning, or Content, could not possibly be a grammatically significant part of the Semitic root. If, indeed, the Constructivist root is modelled after the properties of the Semitic root, one could not assume that it has Content without a significant loss of explanatory power. In turn, the only fail-safe generalization that applies to all instances of any one Semitic root is phonological. This is exactly the conclusion Aronoff (1976) and subsequent reaches as concerning not only the Semitic root, but also English non-functional morphemes such as √JECT, √DUCE, √SCRIBE and similar.

This said, the occurrence of some form of Content relatedness in 40% of cases could not be a coincidence. To be sure, 40% is not sufficient to establish grammatically rigorous Content relations, even more so as the actual specific nature of the Content relatedness is idiosyncratic, ranging from synonymy to metaphorical extension, and grammatical properties such as argument structure realization cannot be predicted from it. It does suggest, however, that at least for these 40% of roots, some Content, however loosely captured, is cognitively encoded. A fuller understanding of that encoding requires a better understanding of the relationship between labelling and Content, a problem I already flagged as persistent and tenacious. My only claim, as relative to that encoding, however characterized, is that it is *not grammatically represented, nor does it play a grammatical role*.

And so, because the Content of roots is at best non-systematic and does not generalize, roots cannot be the basic building block in the construction of Content. Rather, my belief is that Content is matched with syntactic units of delimited size, which are always bigger than the root. Empirically, this claim is shared by DM. However, although the domain of Content is frequently the same in DM and XSM, the logic by which Content is associated with that domain is different. For DM, as noted, Content remains the prerogative of the root, but the specific root alloseme is chosen locally, in the domain of the immediate categorizer (but see *conjecture* and related discussion). In XSM, on the other hand, the domain of Content could extend to include all derivational affixation, thereby allowing the Content CONJECTURE to be associated with either structure in (5) in its entirety. This difference between DM and XSM is both empirical and theoretical. On the empirical level, it involves the existence (or lack thereof) of non-compositional Content that requires domains bigger than that of immediate categorization (beyond *conjecture*, also cases such as *civilization, existentialism* or *committal* with the meaning of BURIAL). On the theoretical side it involves claims about the nature of functional vocabulary, and whether categorial functors (typically in English derivational suffixes) are inherently similar or distinct from functors which are segments of extended projections (typically inflectional markers such as plural, tense, aspect, etc.). The other distinction is the one which I already discussed, and which is theoretical, and involves a fundamentally different view of the root. Is the root a Content set, or a set of allosemes, as Marantz (2013) and Harley (2014) contend, thereby putting forth a strong hypothesis of Content compositionality constructed from the root upwards, or alternatively, as I believe, roots have no Content, and Content, rather, is always associated with phrases of a prescribed size. For XSM, then, expressions such as *receive* and *deceive* are therefore assigned the Content RECEIVE and DECEIVE as complex formations, and the fact that they share a root translates into shared phonological properties, but neither syntactic ones, nor Content related.

You need to add to this the fact that under my approach, Content is not just assigned to phrases bigger than the root, but it does so based on partial phonological representation, and so what *re-* or *de-* are contributing here is first and foremost phonological, insofar as the internal syntax of *receive* and *deceive* may be identical, but the phonology is not. Similarly, while the internal syntax of *commitment* (DUTY) and *committal* (BURIAL) may be the same ($[_N [_V$ con $[\sqrt{\text{MIT}}]]$ NOM], their phonology is different in allowing NOM to be realized either as *-al* or as *-ment*, and with a different Content emerging. This is a real significant difference between DM and XSM, in terms of understanding what Content is, and how it is modelled relative to roots, word-internal syntax (=morphological structure), phonological realization, and the relationship among these factors.

REVISTA LINGUÍSTICA: The Exo-Skeletal approach proposes a deep split between the semantic-grammatical meaning conveyed by the functional structure and the grammar-independent Content conveyed by the roots. What is this grammar-independent Content carried by roots and how may the roots, which do not have any grammatical properties, interact with grammatical structures?

HAGIT BORER: I already covered much of this question in my previous answers. For me, as I already said, roots are just bundles of phonological properties. This is something I have been saying

for about 10 years, but I want to elaborate on this and clarify it, as it is frequently misunderstood. I will take as my starting point *Jabberwocky*, Lewis Carroll's poem from *Alice through the Looking Glass*, a brilliant linguistic experiment which lends huge support to the Constructivist agenda. In the first stanza of *Jabberwocky*, there are no recognizable Content words, and the only established English words are functional vocabulary items (both free and bound). Nonetheless it is clear that *Jabberwocky* is a well-formed chunk of English grammar, and it is also considerably more comprehensible than one would expect, given the absence of any recognizable Content. If, as lexicalism would have it, structure projects from lexical-semantic terminals, explaining *Jabberwocky* would require thrusting one into some sort of analogical loop, which, I submit, would be overlooking this striking result.

But what I want to do is to take the *Jabberwocky* experiment one step further. (Written) sound combinations in *Jabberwocky* that are place holders for substantive items, such as *brill(ig), tove, slith(y)* etc. do obey English phonological restrictions, but they are not paired with any Content, and that is precisely the point of the Carroll experiment: labels without Content, sign without meaning. However, if these signs have no Content, why are they there, phonologically? Why don't we just have silence, where a substantive element would normally be? And so, the thought experiment I would suggest is to try to construct a sentence, a paragraph, in which only functional items have phonological representations. One could restrict oneself to free functional items, to avoid the problem of requiring a host, say, for the plural morpheme. A sample of such an experiment would be as in (6): The crossed-over items in (6a) are *Jabberwocky* nonce vocabulary items. In (6b), they have been eliminated. What is striking about the contrast between (6a) and (6b) is that (6a) is English. (6b) is not only not English, but, I submit, not a possible utterance in natural language:

6. a. I would ~~(*gimble*)~~ many a ~~(*tove*)~~, but you didn't ~~(*outgribe*)~~ every ~~(*borogove*)~~
   b. I would many a , but you didn't every

And so, the *Jabberwocky* extension in (6a) is an utterance in natural language, and specifically in English, even though the substantive (non-functional) items have phonology but no Content, but if you eliminate the phonology, what you end up with is no longer natural language. It doesn't mean that a computer couldn't parse (6b) and assign it structure. To the contrary, I am sure it could, and that in fact, any linguist could assign it structure as well. But humans, including linguists, would still refuse to accept it as an utterance of natural language. It is worthwhile noting that our grammar does allow for phonologically silent spaces, produced through various forms of ellipsis or through the distribution of null elements with specific well-defined properties (null pronouns, null determiners, null complementizers, etc.). However, our licit silent elements are either designated functional elements, or alternatively, require a phonologically realized antecedent, thereby making them different from the Content-less silences in (6b). Another way of querying the oddity of (6b), then, would be to ask why we do not have null DPs with (non-functional) Content no matter how salient, or null 'gapping' without an antecedent, interpreted contextually within their syntactic environment, just like '*gimble*' would be, presumably.

The conclusion I draw from this thought experiment is that there must be some phonology at the bottom of the tree, or at the bottom of each extended projection. Don't ask me why, because I don't know why, but this seem to me to be an inevitable conclusion. Altogether, what it looks like, is that you start with some piece of unstructured sound, or sign, which the syntax then molds and shapes into something to which we can give Content. However, this is not a conclusion that our present-day syntactic theorizing is built to capture, and how to characterize this requirement without reevaluating radically the relationship between at least some aspects of phonology and syntax is not obvious. Methodologically, and at the very least from the mid-70's if not earlier, much theoretical care was taken to sever all aspects of phonology from the syntax, with the result that our present computational systems, and regardless of their merit, are specifically built to exclude statements such as 'first merge must involve a chunk of phonology', and perhaps it is time to reevaluate this.

XSM is by no means the only contemporary approach that is attempting to reintroduce phonology into the grammar. You see it quite prominently in recent work by Kayne, roughly from 2005 onwards, which is looking to create a phonology-syntax structural continuum. There are contact points between these agendas, the primary of which is the belief that altogether, the research methodology which generative syntax has followed at the very least since the mid 60's has overlooked the potentially essential part that sound or gesture play in the emergence of well-formed structures, including syntactic ones, in natural language. To summarize, the progression of generative research methodology, in principle, and into present day Minimalism and including DM, has involved an absolute separation between the phonology and the syntax and the phonology and the interpretation. While this may have been a beneficial theoretical move at its inception, there are more and more reasons, and more and more research, which indicate that it is high time to reconsider this perspective. I want to highlight the fact that such reevaluation is not an easy task. Because so much of our theorizing in the past 60 years has involved such strict separation, the formal tools for introducing phonological considerations into the syntax are very much in need of development, and I, myself, would very much like to see more research focused precisely on such formal questions.

Last comment on this topic - the problem I am highlighting here is *different* from the question raised, for instance, by Ramchand (2008), as to whether roots are needed in the syntactic representation altogether, alongside the possibility that what we call a 'root' is associated with a more elaborate syntactic structure, without the need for a particular terminal. Nor is it the same question as that put forth by De Belder (2011) and De Belder and Van Craenenbroeck (2015) who propose that roots are empty terminals which are formally forced to exist by the properties of first merge. The reason these are *not* the same question is that for Ramchand's system, and however stated, some phonological representation remains essential and obligatory, albeit in the context of a structure, rather than a terminal. Similarly, for De Belder (2011) and De Belder and Van Craenenbroeck (2015), the root may be a formal empty terminal in the syntax, but by the end of the derivation, one way or another it must be populated by some phonology. Some phonological representation for non-functional material

is thus obligatory in these systems as well, regardless of how we model its availability, and it is an explanation for this obligatoriness which remains missing.

REVISTA LINGUÍSTICA: Your own work, at least until the mid - 90's, was guided by Lexicalism. What problems in lexicalist formulations made you propose a new syntactic approach to word structure?

HAGIT BORER: There were certainly important theoretical reasons, but I want to start from a personal perspective. We tend to set aside personal preferences when we do our research, and for a good reason, because reasons to adopt a theoretical perspective or an analysis must be grounded in rigorous reasoning and robust empirical evidence. But personal inclinations still matter in terms of which directions we choose to pursue. These may or may not pan out, of course, subject to more robust criteria of research methodology, but as this is an interview, not a linguistic article, I thought I should say that sometime in the early 90's, when I started considering the possibility that it is all just syntax, I became very excited, as such a wholly structural, top-down approach fits well with the general way I think. But it is important to note that my personal preferences aside, it is not always the case that syntactic structural systems are more coherent or explanatory than systems that are built, in our specific case, on lexical information. Take for instance a comparison between GB and LFG as they existed in the mid '80s. LFG was – and still is - a profoundly lexicalist model, in the sense that there were no longer any mapping operations on phrase structure, transformations. There are only mapping operations that take as their input information stored in lexical entries, and their output are modified lexical entries. By the mid to late 80's this was a much more theoretically coherent model than GB. GB during the period was a mixture of lexicalist assumption and grammatical non-lexicalist assumption, and the distribution of labor between the syntax and the lexicon was not entirely principled. There was also massive redundancy between lexical generalizations and syntactic phrasal ones. I will give you an example I frequently discuss. In (1986), Levin and Rappaport published their seminal adjectival passive article where they showed, to my mind conclusively, that the argument of an adjectival passive like "the window is (completely) broken" is syntactically external and hasn't moved there from the object position. They did not exclude the possibility that *window* may have started as the logical object of the verb *break*, but whatever mapping had transpired to land it in the subject position of *is broken* was not syntactic. The very same empirical tests that showed conclusively that *the window* is syntactically external in adjectival passive, however, also show that in "the window was broken by the children", verbal passive, *the window* is internal, and *has* moved from the object position. And so, here is the theoretical conundrum: sometimes what looks like the logical object of the verb, say the *theme*, IS the object of the verb and needs to undergo syntactic movement to become subject, but at other times, what looks like the *theme* ostensibly associated with that very same verb has stopped being the object of that verb, if it ever was, well before the syntax, and rather, is mapped directly into the subject position without the need for syntactic movement. But

why should there be such duplicity of functions, with the promotion of the *theme* sometimes lexical and sometimes syntactic, and why should the lexical one give rise to an adjective, but the syntactic one to a verb? Why not the other way around? Why not both lexical? Why not both syntactic? GB, in the mid 80's, had no answer to these theoretical questions, and the result was a certain degree of arbitrariness, and considerable redundancy. By contrast, in LFG everything was an operation on lexical entries, and as a result, the redundancy and the need for two distinct promotion operations did not exist. It was, in other words, a cleaner and more coherent model. There was, however, a hitch – the mixed picture in GB, however theoretically sloppy, was empirically sound, insofar as the *theme* (if that is indeed what it is) is external in adjectival passive, but internal in verbal passive. This empirical result quite simply had no theoretical explanation in GB. The uniform, more rigorous LFG system, on the other hand, had an empirical problem, insofar as an identical lexical mapping operation did give rise to a distinct set of syntactic diagnostics. And so, LFG had an empirical problem, but GB had an in-principal theoretical issue – it had two parallel formal systems that effectively did the same thing, and which system would operate in which construction was a complete stipulation. If you look at much of the ensuing research in the following decade within both GB and LFG, you see attempts, within each one of these approaches, to address their respective problems. In GB (and subsequently early Minimalism) an attempt to eliminate argument structure mapping operations from the lexicon, and in LFG attempts to create a more complex representational structure that could in principle lend more nuanced relationship between operations of lexical mapping and the syntactic output.

You can see from this example at least some of the problems with GB that I and many within my cohort started to focus from the mid/late 80's onwards. On the one hand, GB was, to my generation, a huge structural playground, especially as it successfully integrated an incredible wealth of empirical results and structural observations inherited from Relational Grammar, and with multiple exciting empirical, rule-governed results all there ripe for the picking. At the very same time, and beginning with Pesetsky (1982), lexicalism acquired a lexico-semantic angle, as a result of which the role of lexical entries as, effectively, mini-constructions, was significantly strengthened. Reconciling these two trends was not necessarily an easy matter, and to some extent, you see different scholars making different choices as to how to distribute the labor between the syntax and the lexicon. For me, quite early on, and at least as early as (1984), and well before the emergence of the elaborate functional structures we have at our disposal today, the direction to go was to transfer to the syntax, as much as possible, what erstwhile was lexical, starting with inflection, which, following Halle (1973), came to be regarded as strictly lexical. In what was at the time a very controversial move (but most certainly no longer is), I argued in a NELS paper from 1984 that rules of inflection are syntactic. Particularly significant in advancing the cause of moving away from the lexicon in the 80's was Mark Baker's work (1985, 1988). Baker did not argue for syntactic rules of word formation then, or for that matter now, but his morphological structures were constrained by syntactic structures and interacted with them in such a way, that sooner or later, one had to ask why they are lexical, and not syntactic. Similar questions cropped up relative to the seminal work of Hale and Keyser (1993),

which proposed, effectively, a syntax of word formation lodged in the lexicon, but as subsequent work repeatedly showed, a failure to integrate it into the syntax proper may have helped with some lexical exceptions, but that was at the cost of major structural and theoretical redundancy. Work integrating Hale and Keyser (1993) into the syntax proper therefore started almost immediately, including my own earliest contribution from 1994, and within the budding DM framework and in particular, in Harley's (1995) dissertation.

It is worth noting that adjectival passive remained a tenacious problem. In the late 80's, with the emergence of an articulated analysis for verbal passive which took UTAH as its cornerstone (BAKER, JOHNSON and ROBERTS, 1989), it also became clear that this very approach absolutely cannot handle the argumental properties of adjectival passives, forcing adjectival passive, as a mapping operation, back into the lexicon. It is an appropriate illustration of the enduring force of lexicalism that it took Mark Baker some 15 years to concede (in his 2003 book) that adjectival passive indeed is a problem for UTAH, itself fundamentally entrenched in the existence of articulate lexical entries complete with syntactic information.

Problems internal to GB were definitely a reason I myself as well as others were moving away from the lexicon as a grammatical entity, but this tendency was very strongly enhanced by the fact that generative approaches to morphology have shown tremendous progress starting, roughly, at the mid 70's, with incredibly important contributions such as Aronoff (1976), Selkirk (1982), Lieber (1980), Williams (1981a,b), Pesetsky (1979), Kiparsky (1982) and many others. There is a bit of an ironic twist here. In Remarks on Nominalizations, Chomsky (1970) suggests that morphology is not a generative component. Lexical, to be sure, but without sufficient regularity that could serve as a foundation for a rigorous rule system. However, except for Jackendoff (1975), this was summarily rejected by the generative community, including Halle (1973). Rather, a flurry of work has emerged charting and documenting the myriad of ways in which morphology does lend itself to a generative, rigorous treatment. In turn, the more generative and rigorous the treatments of morphology, the more it all started to look an awful lot like syntax, with constituent structure and combinatorial constituent building operations, cyclical rule operations, and asymmetric structures involving heads and projections, all raising the distinct possibility that what is being constructed is a parallel structural system, with all the properties of the syntax, but, as it remains in the lexicon, still allowing exceptions to be stated more easily. To be sure, some syntactic structures as we perceived them at the time couldn't be accommodated within complex words, and most particularly, the X'-schemata of projection and obligatory complementation, but beyond that, it frequently appeared like the reason WF remains in the lexicon and is not integrated into the syntax is because of the existence of item-specific exceptions. By some rarely articulated consensus, then, morphology became a rigorous rule component (still distinct from the syntax) which nonetheless more easily accommodated 'exceptions', because, by virtue of being placed in the lexicon, it had access to item-specific information. The inherent contradiction between the existence of a rigorous rule system of any sort and in any component, and the license for that rule system, but no other, to access listed exceptions, is evident once spelled out.

We are now more or less at the early 90's, and several accounts start emerging which attempt to take on board the syntactic properties of word formation. Particularly noteworthy is Lieber (1992) and Ackema (1995) and subsequently Ackema and Neeleman (2004). These accounts developed along almost opposite lines. Lieber (1992) accommodated the difficulties WF had with X'-theory by integrating into her syntax phrase structure rules that were, in effect, specific to word formation, and not otherwise attested. Ackema (1995) and Ackema and Neeleman (2004), on the other hand, assumed that while word formation is syntactic in nature, it operates at the sub-zero level of the X'-theoretic projection, and therefore below the complementation level. I was myself developing a model at the time called Parallel Morphology. The idea was that rules of word formation are combinatorial but not syntactic, as such, and hence problems with both X'-theory or insertion frames do not emerge. However, these rules could apply to the output of syntactically generated structures, or alternatively, to lexically available terminals, pre-syntactically. The output would differ, because in one case the combination created by the word formation rule would be associated with a fully developed syntactic structure and its properties which it could not impact, while the 'lexical' output could be inserted into the syntactic structure as such without requiring additional syntactic structure. To illustrate, a verb, V, if it comes together with some nominalizing affix NOM would give rise to the structure $[_{N}$ NOM $[_{V}$ V]]. This structure could be inserted into the syntax as N, and have the syntactic properties of N. The V embedded within it would be syntactically inert. However, $[_{N}$ NOM $[_{V}$ V]] could also be inserted to the output of a syntactic operation that would bring V and NOM together, say head movement, as in the simplified syntactic structure in (7):

7. $[_{NP}$ V+NOM ….. $[_{VP}$ (subj) $\nVdash$ obj]]

In (7) V is not inert, because regardless of its incorporation into NOM, it has a copy which projects syntactically, and which allows arguments. And so, the word formation rule and the morphological output may be identical, but given its interaction with different syntactic structures, the properties of the entire configuration may be different.

My original account, in 1991, of the differences between Grimshaw's (1990) complex event nominals (also labeled as process nominals, or argument structure nominals) which preserved the syntactic structure of the verb embedded within them, and so-called result nominals (also labelled R-nominals) where the verb embedded within the nominal appeared to be a non-projecting terminal with no associated VP or argument structure) was exactly based on this particular view in which Word Formation operations were parallel to the syntactic derivation.

Fundamentally, that has remained my position, in the sense that I still think Argument Structure nominals preserve the syntactic structure of the verbal constituent, while R-nominals only contain a bare, non-projecting verb. What has changed, however, is my view that word formation rules are sufficiently formally distinct from syntax to warrant a formal separation, and what cause that change were developments in syntax. In particular, X'-theory, a problem for Lieber (1992), and a major reason for postulating a distinct WF system both for Parallel Morphology and for Ackema and Ackema and

Neeleman, vanished from the syntactic landscape with the emergence of Bare Phrase Structure, and with its disappearance, a significant number of problems for integrating morphology into the syntax vanished as well. At the very same time, the emergence of models of argument structure that were less and less dependent on lexical entries solved the problem for syntactic word formation presented by the vanishing of 'obligatory' complements internal to words. I myself endorsed non-lexical models of argument structure at least as early as 1993, (in print in 1994), making the existence of *syntactic* structures without complements (especially for verbs) less problematic. And so, sometime in the mid '90 I realized that I had run out of principled reasons to assume an independent Word Formation component, as syntax from the mid-90's onwards could integrate the complexities of word structure with little need for word-specific compromises, especially once one endorses the view, as old as generative grammar itself, that full phonological realization is post-syntactic.

There remained, of course, the last tenacious reason to distinguish word formation from syntax, having to do with the prevalence of non-compositional Content for combined terminals, so-called morphology, when compared with syntactic phrasal outputs. While the generalization, to begin with, has been challenged, in particular by proponents of Construction Grammar and on the basis of the prevalence of phrasal and sentential idioms, I do think there is fundamental truth to the generalization. However, as I already discussed in some detail, there are at present a number of approaches to the modelling of non-compositional Content, all of which have considerable promise. As is the case, I believe, with other 'exceptions', whether they concern the non-compositionality of words, or of phrases (=idioms), I firmly believe these are not 'problems', but rather, these are names for research questions to pursue.

Perhaps some comments on my own personal linguistic development might be of interest here. I had this idea in summer of 1980 which I pursued in my dissertation, that all language variation stems from what at the time I referred to as grammatical formatives, and what now I would call functors (including both inflectional and derivational). As you know, the idea eventually became very well-known and universally endorsed, although it did take some 15 years for that to happen. From my perspective when I proposed that idea, and much as I continued to think it had to be right, my ability to pursue it was limited, because, as I realized almost immediately at the time, I didn't really know anything about morphology. And so, I started making it my business to understand morphology. And understanding morphology, or to be a bit more realistic, understanding morphology better, is an extremely challenging task. On my way to my 2013 manuscript, which is probably as close as I am ever going to get to 'understanding morphology', I discovered that I had to understand lots of other things, so that I can see better whether they are morphology or not. And so, I had to understand, first and foremost, argument structure, which eventually allowed me to sever the verb from its arguments, a definite progress toward understanding morphology. I had to understand DP structure, not because it had anything inherently to do with morphology, necessarily, but because my analysis of DP structure is what laid the logical foundation for pursuing the radical stripping of insertion frames from lexical items. Linguists have always taken it for granted that e.g., the existential vs. generic meaning of *dogs*

does not come about because of dual lexical listing for *dogs*. But at the same time, the assumption that, say, *move* on its completive reading, *move* on its activity reading, and *move* on its transitive reading are distinct listings was explicitly or implicitly endorsed throughout the bigger part of the past 60 years. And so, I was hoping that once I showed that generics and existential, specific and non-specific, and most notably, mass and count polysemy does not entail double lexical listing, taking these conclusions to the verbal system would become more compelling.

Finally, I had to understand the nature of syntactic categories and the nature of the functional/substantial divide.

During this long journey, there were, to be sure, some digressions, and in particular my work on the maturation of syntax, my work on subjects and control, my work on verb movement in Hebrew and my work on inversion, some of which make for another coherent research complex, are not 'morphology'. Just about everything else, and in particular the Exo-Skeletal trio of books, all came about as a result of following the morphological scent.

REVISTA LINGUÍSTICA: With the Exo-Skeletal approach you have developed a strong isomorphism between syntax and semantics.

HAGIT BORER: This is not entirely accurate. I don't think I have an opinion about the relationship between syntax and semantics that's different from mainstream Minimalism. Some people have explicitly challenged it, but overall, I would say that alongside many others, I believe that a syntactic structure *A* has a unique interpretation call it *I[A]*. Specifically, you take syntactic structure *A* of whatever relevant phrasal size, and you map it onto a semantic formula that would yield its interpretation, *I[A]*. And that's the only possibility: *A→I[A]* via some rigorous semantic route. But not the other way around. There is no direct mapping between some specific well-defined semantic meaning and syntactic structure, and the same semantic meaning could correspond to distinct syntactic structures. For generative semantics there was full isomorphism, so that synonyms had to have the same syntactic structure. I don't subscribe to this view, because I don't think it is empirically or theoretically tenable, and I don't believe my fellow formal syntacticians subscribe to this view either. A good example I can give you is generic expressions. So, if you look at the myriad of syntactic structures which express genericity within the same language, not to mention across languages, you see singulars both definite and indefinite *a lioness protects her cubs*/*the lioness protects her cubs*, some bare plurals *lionesses protect their cubs* and finally, even in English, a definite article plus plural in contexts that do not allow bare generics (*the extinction of the dinosaurs)*. One could sit and tear out one's hair to try to figure out whether there are semantic differences between these expressions of genericity, and sometimes there are, and sometimes there aren't but at best they are very subtle. So here you go. Are there sufficient grounds for saying that we are dealing here with completely different type of genericity, or alternatively, as seems self-evident to me, we can conclude that generic meaning can be expressed in many syntactic ways. But if that's so, there's no isomorphism.

(Isabella Pederneira: Now, it's clearer. Thank you.)

HAGIT BORER: I would say that's the belief of many, if not most formal syntacticians. There is a debate, however, concerning the relative strength of the uni-directional claim. Specifically, some would question the universality of the claim that *A* → *I[A]*. For example, Chierchia (1998 and subsequent) and quite a few in his footsteps subscribe to the view that this uni-directional statement is too strong, and the interpretation of *A*, as a syntactic structure, may be parametrized. Thus, for Chierchia, in Italian, an NP structure could only map onto a predicative or property interpretation, but the very same NP structure in Chinese is interpreted as *kind*. The range of variations is of course delimited, but I still think that Chierchia, or, say Bošković (2008 and subsequent), would not agree that the same syntactic structure always gets the same interpretation. It follows, of course, that they cannot subscribe to the view that the same semantics would be translated into a unique syntactic structure.

REVISTA LINGUÍSTICA: Do you think that diachronic linguistic studies can provide important answers to a generative analysis approach?

HAGIT BORER: Of course. Why not? What people spoke 500 years ago or 1,500 years ago is surely natural language? The question is not whether they can provide important answers. The question is how to study languages with no native speakers, and how to reason about diachronic development. The absence of native speakers is crucial, methodologically. I see with some sadness the capitulation, within theoretical syntax, to claims that grammatical judgements are insufficient as sources of evidence, to be replaced with corpus studies, or clumsy experiments to justify the simplest empirical generalizations. To be sure, some experiments are crucial to establish intricate nuances, particular contrasts and so on. But the overwhelming number of both grammatical and ungrammatical judgements reported in your average linguistic article, if we were to drag 40 undergraduate students to a lab to test them, would be at close to ceiling, and even in subtle cases well above the threshold of 'statistically significant' as set up in most published psychological work. It is precisely the ready availability of data, through judgements, which has allowed generative syntax to give rise to so many results, and so many novel observations about the structure of language that have been overlooked during the prior millennia. In the absence of native speakers, diachronic linguistics, just like corpus studies, faces two problems. The first has to do with the fact that the body of data is not 'clean', by which I mean it represents, indiscriminately, multiple speakers who may vary from each other. Synchronic data as obtained through grammaticality judgements is 'clean', because it reflects the single, coherent grammar in the mind the judgement giver. Although, of course, judgements may vary across the community, relative to any given speaker, it is possible to construct their individual grammar coherently and consistently, with subsequent accommodations made for inter-speaker variations. For corpus data and diachronic data, an outstanding problem is the fact that it is not possible to ascertain whether variation, whenever attested, resides in a single grammar, or is a generalization across a speech community with potentially diverse individual variations. Therefore, both diachronic data and corpus data give us evidence about what Chomsky has labelled E-language,

not I-language. The second problem concerns the absence, in both diachronic data and corpus data, of ungrammatical judgements, which have been crucial in delimiting the proper description of any grammatical system. All this does not mean that both diachronic data and corpus data could not be studied fruitfully, especially, in diachronic cases, when the historical development is well charted. Latin and its derivatives are particularly good example of the benefits of diachronic studies. It does, however, mean that conclusions drawn from both corpus data and diachronic data may only yield themselves to incomplete formalization.

REVISTA LINGUÍSTICA: You have written a trilogy - *Structuring Sense* - and you have been working on morphosyntax, language acquisition and syntax-semantics interface within a grammatical theory you developed. What can we expect as future works?

HAGIT BORER: I should start with a brief comment on the emergence of trilogy. Most of what's in these three books, some 1,400 published pages, was all going to be one book. The oldest part, when I started working on the book in the year 2000 was the analysis of derived nominals, which I started working on in 1990, and that was going to be chapter 5 of the book. Chapter 2 (after the introduction) was going to be DP structure, and chapters 3-4 were to be the argument structure chapters. However, when I was done writing the draft of what was to be chapters 2-4, I already have well over 400 pages. At that point, I decided to set aside the derived nominals part, because this was getting too long, and furthermore, while I was working on the event structure part, I changed my mind about how argument structure works, and the derived nominals part, already written, needed some redoing. Even for what has already been written, OUP suggested 2 books, which with separate introductions and conclusions and some elaboration on some of the analyses made for two full size books. And so, the two books went to press, with the 3rd book to materialize shortly, or so I thought. The 3rd book, however, ended up taking very long time as my thinking went well beyond the derived nominals part to develop a much more articulated perspective on morphology, and in retrospect, that 3rd book, *Taking Form*, should have also been two books as well.

Anecdotally, I wanted to call the trio of books *The Exo-Skeletal Trilogy*, but OUP wouldn't let me as with that title, they claimed, the books would end up in the science fiction section of bookstores...

The next stop, in this XSM journey, I already spoke about, and that's the syntax-phonology interface. There is a lot of important work that's been going on for many years on focus, prosody, and intonation, but that's not what I want to do. I want to look at much smaller structures involving word-internal correlations between structure and phonological realization. To follow on things I said already, why do roots have to have phonology? What does it mean, exactly, that Content is matched with annotated phonological structures? These are all claims which are in my 2013 book, but which I need to understand better. I have work in progress which specifically asks whether the periphrastic divide that we find, phonologically, in participial passives in English, Romance, quite

a few other unrelated languages, corresponds to an important syntactic domain, and if so, can we find reflections of it in synthetic passive, such as that attested in Hebrew or Arabic. To argue that in such cases as well there is some sort of a boundary which, in English is periphrastically expressed but in Hebrew or Arabic is not immediately phonologically visible, requires delving into the details of the relevant phonological systems of these languages, and this would be new to me. I haven't done any serious phonology since 1979. But even if for whatever reason my own efforts do not pan out, I remain convinced that the syntax-phonology and the phonology-Content interfaces are the next linguistic frontier.

REVISTA LINGUÍSTICA: I thank you immensely for participating in this interview. It is very important to me. It was very enriching, and the Linguistic Post Graduate Program also thanks you very much for your participation.

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