

Prospects for an Explanatory Theory of Semantics

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I

This essay reports and reflects on a workshop on *Biosemanantics* — *Semantics within the Biolinguistic Program*, organized by Boban Arsenijević and myself, which took place at Leiden University from 10 to 12 September 2008 and was funded in the frame of the ‘Origins of Truth’ project by the Dutch NWO. The reader can hardly expect that I will now review the state of the art on semantics within the biolinguistic program: Truth be told, there is no field here with a state of the art to report. Do we know even the rough outlines of what an explanatory research program on the origins of human semantics looks like? Everyone present at an informal pre-workshop meeting agreed, in particular, that one aspect of the problem was beyond the scope of current research: the fact that there is semanticity at all — the problem of ‘intentionality’, as philosophers have dubbed it. Syntax allows us to generate complex descriptions of the world (or possible worlds) — but the very term ‘description’ is an intentional one. Syntactic structures actually *mean* something — we can use them to refer to real or possible worlds in highly systematic ways. This is something that syntax as we know it does not explain. This, indeed, is the business of semantics (jointly with pragmatics, whatever the difference between these two may amount to). We can *formalize* semantics, to be sure. Yet, this won’t tell us why it exists or why it does what it does.

That said, I think it is fair to say that the workshop did isolate some questions that we can put on our research agendas. I will summarize these in the final section. Before that, I will report on some of the talks and the discussions that took place. I will embed this within some stage-setting on what I am calling an explanatory theory of semantics. I should warn upfront that my account is necessarily selective and coloured by my perceptions, if not simply by what I understood.

II

Under the broad umbrella of the biolinguistic program, syntax has clearly held centre-stage. This is, presumably, because syntax as a domain of inquiry fits into

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the mould of questions that we know how to tackle. It's the 'easy' part. In regards to syntax, in any case, the biolinguistic program now appears relatively well-defined and the basic agenda can be summarized as follows: Minimize appeals to a genetically defined Universal Grammar, don't proliferate 'modules', maximize appeals to domain-general principles in linguistic organization, from learning algorithms of a non-linguistic sort to general principles of design economy and geometry (e.g., symmetry breaking, prominent in Boeckx's and Di Sciullo's presentations). In short, the basic parameters of inquiry are set, and daily work can take the form of relatively technical, problem-solving routines, as in analyzing derivations using the apparatus of Merge, Move, feature-checking, and, most prominently, phases: This is the equivalent, in linguistics, to Kuhnian 'normal science'.

That said, what *exactly* the domain of syntax involves is quite unclear. As Boban Arsenijević pointed out in his talk, the minimalist operation Merge, given its austerity and utmost generality, may be a bad candidate for what is specific to language or grammar: Rather, grammar may arise as a specific kind of *restriction* on the operations of a basic domain-general computational system based on Merge that subserves cognition more generally. This important question resonated in Boeckx's and Roeper's talks. How do we characterize syntax — as unrestricted Merge or as a particular kind of restriction on it, due to demands of the interfaces?

It also resonated in Zeijlstra's presentation, in whose model syntax clearly isn't the universal cognitive machine, but the real, restricted, and linguistic syntax whose inherent job is to mediate between sound and meaning. We therefore expect its elegance and minimality to be compromised by having to serve two interfaces, which may impose conflicting demands, as well as possible. Here, syntax is *not* a general capacity to generate structures that go with a systematic form of semantics, but a much more idiosyncratic system that has to be rationalized as a solution that meets a very specific design problem in the context of the externalization of cognitive and conceptual structure in a physical medium. If, on the other hand, one thinks of syntax as the very format of human thought (Leiss, in press), one privileges the semantic interface over the phonological one, and one's account of syntax will tend to be different.

A more specific aspect of this broader issue of the interface between language and cognition transpired in Uriagereka's presentation, which asked: How much of linguistically familiar semantic territory does syntax carve out (let's call this 'Uriagereka's Question'; see Uriagereka 2008)? For example, does syntax have to be organized in such a way that it derives — or architecturally reflects — the dimensional layering of concepts as characterized, say, in Vendler's work on verbs? Are Vendler's distinctions, intrinsically, syntactic cuts, as well as, or over and above, semantic ones?

As Hinzen pointed out in his opening remarks on the 'prehistory of semantics', the philosophical and linguistic answer to Uriagereka's Question for nearly 700 years has arguably been: by and large, nothing. Ever since the nominalists under Occam's lead demolished Modistic Universal Grammar in the early 14th century, language was largely regarded as an arbitrary means to express thought: It is deprived of any intrinsic relation to either the structure of

the thoughts conveyed or, for that matter, the external world out there, which language can be used to describe. It is a tool for conveying thought, not its cause. If so, language is deprived of the role it was given by the Modists: that of an instrument of knowledge, a format in which systematic knowledge of the world is possible (see Leiss, in press for a reconstruction of the history).

When Cartesian rationalists resurrected the Universal Grammar project, the nominalist view of language as an arbitrary device for externalizing independently constituted 'thoughts' was kept: Language is still not the format of a specifically human form of knowledge. Neither did this axiomatics change when Chomsky resurrected Cartesian linguistics and syntax became 'autonomous'. As many have understood this claim of autonomy, syntax is an arbitrary formal system, unmotivated in terms of semantics or thought. Even the latest minimalist architectures of grammar reiterate a version of this basic axiom, when syntax is explained as an optimal solution to the 'conditions imposed by thought', a view that entails the independent constitution of thought, which thus can impose 'conditions' to which language is somehow answerable. Syntax, again, therefore, is not instrumental in formatting thought: It is still merely a way of expressing it (though, now, optimally).

The same axiom is found in the opponents of generative syntax, such as cognitive linguists taking the notion of a 'communicative intention' as a starting point of linguistic inquiry, or in the 'language of thought' tradition (Fodor 1975, Pinker 2007). Here thought is again an independent system. Despite the term 'language of thought' it is not structured by natural language, or the basic forms and mechanisms that Universal Grammar provides our minds with (Fodor 2001). Michiel van Lambalgen, at the workshop, provided evidence for a related view, arguing that non-linguistic logical processing entering the construction of a discourse-model is prior to the evolution of language. This makes linguistic organization an instance of cognitive structuring rather than the format of a specifically human form of knowledge.

Taking another viewpoint, Hinzen suggested turning the tables against the nominalists and seeing the computational system of language as the engine of specifically human, propositional thought. Syntax provides the forms of a possible human thought (cf. Hinzen 2006). Effectively, syntax therefore *is* a theory of semantics. If syntax does not constrain the space of semantics — if it does not define the concept of a possible meaning, in the same way in which Universal Grammar defined the concept of a possible language — what could? How, why, and by what, would semantics be constrained in the first place? (It had better *be* constrained, if there is to be an explanatory theory of it.) Where the axiomatic foundations for semantics are broadly those of Fregean philosophy, this is hard to even conceive. What would it mean to say that the abstract realm of Fregean 'propositions' (the realm of 'content'), would be empirically constrained? Propositions are not meant to be mind-dependent objects, and neither are they meant to be subject to constraints coming from the physical world in some broader sense. Some might want to insist on the archaic character of these views, but then, it is unclear what restricts the format of human thought on other views. Human semantics obeys a highly specific format, including the 'duality' of semantics (thematic structure vs. discourse structure) or the duality of

semantic evaluation (reference vs. truth): From which general, non-linguistic constraints might these aspects derive?

The guiding force behind 20th century philosophy was naturalism, involving an opposition to Frege's metaphysics. For semantics this meant, in one version of the naturalist story, that Fregean propositions were stipulated to be necessarily mediated by 'mental representations' (Fodor 1975). Unlike propositions, the latter are definitely internal to heads and brains, hence would seem subject to physiological and physical constraints. Yet, this is hardly how they have been looked at. The 'representational' theory of mind was embedded in the doctrine of functionalism, which, in its characterization of mind, purposefully abstracted from the way in which minds actually depend on brains (see Hinzen 2006). Mental representations therefore were not studied empirically. Their syntax was a logical syntax, their semantics a stipulated, Tarskian recursive definition of truth. The mental representations were thus not empirically but functionally motivated, as a device for representing the propositions or contents that were inherited from the 19th century. A representation in this sense is what it is, precisely because it relates to something external to itself, its meaning or 'content', of which there is no constrained theory at all. A categorical syntax-semantic separation, unproblematic in the logical tradition and arguably even necessary for purposes of logic, was thus kept as a foundational axiom in the analysis of mind. Having been evicted from the 'Third Realm', Fregean propositions were alive and well. Their fate was to be re-located in the domain of 'denotations'. 'Semantic values' is how they were now called.¹

None of this is easy to square with the development of our understanding of syntax in linguistics. We have gone a long way from the 'autonomy of syntax' as understood by some in the 1950s and 1960s. Now syntax is seen much more as a system that intrinsically subserves the purposes of semantics. 'Full Interpretation' at the semantic interface, for example, is a prime principle of Minimalist Syntax. Positing a new head in the hierarchy of the clause is constrained by its contribution to semantic composition. The most basic architectural elements of grammar, such as phases, characterize units of computation as having an identity that is syntactic as much as it is semantic (Chomsky 2007). As Marantz (2000) points out, domains of 'special meaning' — where meaning, prior to becoming compositional, has some wiggle-room and allows for idiomaticity — are syntactically defined (e.g., the 'internal domain of *v*'). Syntactic definitions of so-called lexical categories (e.g. Baker 2003) will naturally incorporate semantic information. Event structure can largely be read off syntactic configurations (see e.g. Hale & Keyser 2002 and Marantz 2007). Quantification and scope are mediated syntactically (Huang 1995). A categorical syntax-semantic separation on purely conceptual or philosophical grounds, in the light of even these few instances of recent theorizing, makes little sense.

One may even make the stronger case, as Chomsky (2007) does, that the

¹ As the movement towards 'embodied cognition' gained momentum in the philosophy of mind, the idea of propositional mental representation in the relational sense above was abolished, but so were the genuine insights in the generative tradition on the highly systematic and structured character of human linguistic cognition.

very reason that operator-variable interpretations of *wh*-expressions arise, is by virtue of the interaction of syntactic principles and configurations: Within *vP*, the *wh*-element is interpreted thematically; given an appropriately minimal theory of movement, the copy theory, plus an account of phasing, another configuration will then necessarily arise in which two copies of the same lexical item appear in two phases, with the lower copy interpreted as a variable and the higher as an operator. This, it needs to be admitted, is a mere correlation between logic (semantics) and syntax. Yet, engaging in such correlation research may be our best bet at the moment, if we are interested in the origin of a logical mind capable of (binary) quantification.

This would solve a problem of ontology, which we face in semantics, but not (or less so) in syntax: We would make progress, as Uriagereka described it at the workshop, on the ‘naturalization of meaning’. The ontological problem in question is simple enough to state. What *is* meaning? Linguistic *form*, while highly abstract and relatively removed from the visible surface of language, is manifestly there. Semantics, by contrast, to the extent that it doesn’t trivially reproduce aspects of linguistic form, is entirely abstract. Linguists tend to have intuitions as to when something is a ‘semantic question’ or a ‘syntactic’ one, but this doesn’t answer the ontological question just posed — a metaphysical question, essentially. To be sure, meaning is ‘real’ enough, an inherent part of the scheme of things, an aspect of nature. Yet it is intangible in a way syntax is not.

What proves this intangibility is the persistent attempt of philosophers throughout the 20th century and even today to get rid of it — say, by redefining semantics so as to reduce meaning to causal relations between words and objects (the ‘causal theory of reference’, coherent with the doctrine of physicalism, and first proposed by Skinner in 1955), or by denying the very reality of meaning, be it through elimination (Quine 1960) or claims of radical indeterminacy (Davidson 1984). No matter which of these paths we take, a ‘science’ of meaning will be a conceptually impossible enterprise. The validity of the enterprise of a ‘science of syntax’ in Chomsky’s sense has been routinely questioned on similar grounds. Yet, in this instance, the arguments seem somewhat easier to refute (Chomsky 2000: chap. 3).

Syntax as a theoretical form of inquiry arises with the insight that human grammar, surprisingly, has a formal structure to it, which can be empirically studied and has a contingent character: It does not follow from any logical necessity. Hence there is such a thing as a theoretical ‘science’ of syntax. A theoretical ‘science of semantics’ in a similar sense is not yet in view, I claim — which perhaps is why semantics, unlike syntax, has primarily been a sub-chapter of logic, and is taught as such in just about any philosophy of language course around the world — courses which, by contrast, often don’t as much as touch upon syntax.

III

When late medieval grammarians around 1300 first tried their luck on a ‘scientific’, and hence universal grammar, their focus of attention was on the parts of speech qua conditions for the possibility of syntax. This was for a good

reason: Consider, as they did, the difference between *curr-ere* ([*v*-RUN]) and *cur-sus* ([*n*-RUN]), or better, between *dol-or* ([*n*-PAIN]), *dol-eo* ([*I* [*v*-feel PAIN]]), *dol-enter* ([*a*-PAIN], ‘painful’), and *heu!* (‘Pain!’). Clearly, while there is an element that is identical throughout these sequences, namely the roots $\sqrt{\text{RUN}}$ and $\sqrt{\text{PAIN}}$, respectively, the semantics of these roots — good candidates for conceptual ‘atoms’ in Fodor’s (1998) sense — does not explain or predict the existence or function of any of the above parts of speech into which these roots enter. Nor would they predict or explain that these parts of speech enter into syntactic configurations in systematic ways. Presumably, non-linguistic beings could have a concept like PRUN, while being incapable of grammaticalizing it in a way that they can systematically distinguish between *run-s*, *a run*, or *run(n)-ing*, and deliberately switch between these three systematically and categorically different perspectives on what is in some sense the same thing, and in the very same external or environmental circumstances.

In short, the principle that allows what the Modists called *constructio*, i.e. syntax, and thereby systematic forms of knowledge and inference, is not meaning, concepts, or denotations. Syntax is the way to warp us out of the immediacy of our experience with the outer denotations of the words we use. Quite a different kind of meaning, call it grammatical meaning, kicks in the moment we categorize concepts and insert them into configurations. At this stage, the rules of combination are not sensitive any more to the semantics of lexical roots: It is categorial information that drives them. There seems to be no way to track this kind of meaning denotationally — there is no outer physical correlate of verb phrases or propositional configurations, for all we know on empirical grounds. This is part of the problem with a ‘referential semantics’ as Chomsky (2000) has exposed it: To whatever extent meaning is internally determined, by principles of grammar, external relations between words and things viewed as mind-independent won’t illuminate it. Yet, the way in which meaning follows systematically from the compositional rules of language — I-meaning, in short — presumably is what a science of meaning is primarily about.

IV

Cedric Boeckx’ presentation, on ‘I-semantics’, addressed precisely this issue. Can we, once we see syntax constrained in particular ways, somehow see semantics falling into place? To sketch his account, say we start from a pure ‘theory of form’: All there is, let us suppose, is unconstrained Merge, triggered by a lexical item’s having an edge-feature (‘Wild-type Merge’). The operations of this function disregard anything else: configurations as much as theta-theoretic restrictions, what ‘makes sense’, etc. The system’s austere beauty consists in its libertarian tolerance of, in fact blindness to, nonsense. “The rule in the Library”, as Boeckx quotes Borges, “is not ‘sense’, but ‘non-sense’, and ‘rationality’ (even humble, pure coherence) is an almost miraculous exception”. Such a beginning seems maximally unsuited to tell a story about the origins of semantics, until the moment that we see syntactic derivations to be phased. Phases, by definition, introduce asymmetries that acquire interpretive significance: For example, the asymmetry between the phasal head, which remains in the derivation and retains

its edge-feature, and its complement, which is transferred to the interpretive components when the phase boundary is reached.

The specific compositionality of semantics, as Hinzen and Roeper noted at the workshop, maybe is a consequence of phasing in this very sense: The meaning of compositionality is that there are constituents in the derivation which have an independent interpretation, which is not changed as the derivation proceeds. Phasal transfer has this consequence: encapsulating a unit of interpretation. A spelled-out item loses its edge-feature — hence the syntax has to treat it as an unanalyzable ('frozen') unit. Phasal transfer, however, must not be too early: For example, not before a head is merged that can serve as a landing site for an argument; and it must not be a transfer of the whole phase but only of its head's complement. The right solution, Boeckx suggested, is that Merge must apply externally twice; only this creates a rhythm of alternating phasal heads and non-heads that corresponds to the relevant interpretive asymmetries: argumenthood (the D-phase), thematic structure (the *v*-phase), and propositionality/truth (the C-phase). And those indeed, are the basic cuts that we see correspondences of when we look at neo-Davidsonian (Pietroskian) event-representations (Pietroski 2005). In short, as the syntax develops a rhythm of periodic phase-non-phase alternations, semantics as we know it swings into place.

The austere purity of Boeckx' architectural model of syntax raises fascinating questions, not least about how we account for syntactic variation: not at all (because there isn't any, in syntax), or by exploiting the rhythm of phasal heads and non-heads just described, and compromising the rigidity of this alternation somewhat, so as to allow for variation in how many non-phase heads can be inserted between two phasal ones. This, as Ángel Gallego argued at the workshop, takes care of some of the comparative data to be captured (see also Uriagereka, in press).

V

In a distinctly different tone, Michiel van Lambalgen formulated an argument against the whole idea of turning to syntax for explaining human semantics. Lambalgen presented EEG data supporting the view that semantics is not fully dependent on syntax, if it is so dependent at all: It is, in particular, richer, in that semantics systematically depends on non-linguistic sources of knowledge and involves processes of non-monotonic inference and causal reasoning that the syntax as such does not support. The view has become prominent in cognitive neuroscience, given the existence of neuroimaging data demonstrating the fast and seamless integration of non-linguistic with linguistic information in linguistic comprehension. Saying *I am an Aristocrat* in a Cockney accent will not easily lead a hearer to incorporate a new truth into his discourse model, for example (see Hagoort *et al.* 2007). On this view, the very notion of semantics, in the traditional sense of a syntax-driven 'interpretive' system in which compositional information is read off the syntactic representation of an expression, has come into question. Rethinking tradition here proves valuable in accounting for the origins of semantics.

Like everything else in evolution, any ‘semantics module’ needs precursors. Thinking of language comprehension as the process of updating a discourse model (rather than merely as a module for interpreting syntax and mapping it to truth conditions), van Lambalgen argued that a precursor to this can be found in the systems of executive control that both human and non-human animals exercise when planning a sequence of actions so as to achieve a particular goal in possibly adverse circumstances. This predicts the co-occurrence of deficits in planning and in discourse processing, and it is what we find, in both autism and ADHD. If making a plan is analogous to constructing a discourse model, and language comprehension is that, too, a biological substrate is found, in a non-communicative domain, from which language can develop. A planner doesn’t necessarily talk, yet any model of a discourse that we construct will consist of events and some relations between them — as do plans. The hypothesis can then be that language in the sense of communicative medium evolved through the externalization of planning driven by a need for interactive as opposed to individualistic planning.

Let’s consider one of van Lambalgen’s examples, *The girl was writing a letter*, which, on the view under discussion, leads to the computation of a discourse model in which there exists a completed letter. If this is part of the semantics of this sentence, however, then semantics will include interpretive processes that may or may not co-occur with a particular linguistic expression. Clearly, all that the Tense of this sentence specifies is that the relevant writing event was in the past with respect to the point of speech, and all that the progressive Aspect adds to this is that the event was ongoing at this time. That letters are more often completed when being written may or may not be a statistically accurate observation. The compositional core of the meaning of our sentence, its ‘grammatical meaning’, doesn’t seem to be affected by this (any more than the compositional meaning of *I am an Aristocrat*, above, is affected by the statistical fact, more credible in this instance, that Aristocrats are only very rarely accomplished speakers of Cockney).² Syntax-driven semantic effects are *not* defeasible or default expectations, however, in the way that the completion of a letter is. That the subject of ‘brought’ in the sentence *Who did Mary say brought her the chips* is controlled by *who*, not by *Mary*, *say*, is an interpretive consequence of sentence-internal syntax which has nothing to do with discourse or pragmatics, for example. The question therefore is whether the enriched conception of semantics misses a distinction between interpretive processes, if not a whole area of inquiry revolving around the question of which semantic effects are determined by linguistic form, and how.

Here is what’s needed to refute a hypothesis of syntax-semantics alignment for those forms of semantics that *are* fully systematic: evidence of a semantic process that is systematic, monotonic, and *unsupported* by syntactic laws.

² The following discourse felicitously describes a possible situation: ‘The girl was writing a letter when her friend spilled coffee on the paper. The ink proved coffee-resistant and the letter was completed in no time.’ We can’t tell, from language, how resistant ink is, and therefore we need world knowledge as encoded in a situation to complement the information encoded in the syntax alone; yet, that won’t affect either syntax or its compositional interpretive effects.

Semantic coercion, as in the forcing of an event interpretation of the verbal complement in *John began the book*, has been thought to be such a candidate; yet, whether this is so, is not clear from either the linguistic or the neuroimaging data available (see Pylkkänen *et al.* 2008). In the meantime, the appropriate way to proceed seems to be to approach the syntax-discourse from both sides: asking how syntax subserves the embedding of propositional information in discourse, and how van Lambalgenian processes of discourse model construction tune in. As Valentina Papa argued at the workshop, for example, a thorough analysis of elliptic comparatives reveals that relevant missing constituents in syntactic form may need to be retrieved from context, or be added inferentially. Compositionality in the syntax-semantics mapping is accordingly compromised. Clearly, the task ahead is a principled account of just when, and when not, in the construction of meaning, context can or even must be accessed. The extreme answers, ‘never’ and ‘always’, seem equally implausible. Some meaning is determined by linguistic form, some not.

VI

Formulating detailed mapping hypotheses between syntax and semantics that make this particular mapping come out as a motivated one is an instance of this broad task. In a perfect world, the semantic interface should at least be *transparent*. But syntactic structuring should be *meaningful* in regards to how it is interpreted beyond merely allowing for transparent compositional mappings. This basic intuition — that the semantic interface should be well-behaved in some ways — can and has been very fruitfully explored. Ideally, *nothing* should be *non-interpretible* on that side of the grammar, and Kayne (2007) has argued that this is indeed the case (though Zeijlstra’s talk at the workshop entailed the opposite stance). That syntactic structures are not only mapped compositionally into semantic structures, but in a particularly ‘strict’ way, with heads only depending for their composition on their respective direct complements, has been argued by Larson & Segal (1995). As Uriagereka pointed out at the workshop, this aspect of linguistic architecture can be rationalized as a way of facilitating the learning of new lexical items that appear in such strictly compositional structures.

Using a different approach, Ronnie Wilbur showed that syntactic decompositions of event structures may be transparent not only in semantic respects but even phonologically, namely in sign languages, as per Wilbur’s Event Visibility Hypothesis. Evidence was presented from American Sign Language that externalization may here reveal morphological structure unrecognizable in spoken languages. This is progress towards the ‘embodiment’ of semantics. Speaking on the same issue of transparency, Heather Burnett argued that there is no need to assign different semantic types to the same quantificational expression in cases of ‘Quantification at a Distance’ in Québec French, as long as one takes into account the *phase* within which they are merged. If so, semantic content and syntactic position align again. These scattered examples all appear to point us towards a deeper understanding of the semantic interface, according to which that side of the grammar is rather well-behaved indeed.

Uriagereka also developed a novel argument that compositionality in the syntax-to-semantic mapping is not in fact the point. It can't be, because one could apply the relevant composition functions top-down rather than from the bottom-up, reversing them (e.g., we could start by composing Agents with verbs, rather than Themes). However, there is a significant *asymmetry* between (for example) Agents and Themes, leading us to 'first-merge' Themes rather than Agents. In short, there is a derivational *directionality*, with Themes coming earlier, and Thematic Structure as such coming earlier than discourse-relevant structure, giving rise to crucial asymmetrical interpretive dependencies. These asymmetries, again, directly inform the child's learning, now not only in terms of what composes with what, but also in terms of what composes first. One way, therefore, of doing the compositions of meaning is syntactically *viable*, while another is not, and this speaks to the concern of 'naturalizing' semantics.

Proceeding on this course, Uriagereka showed that we can naturalize other aspects of semantics — in fact, we can do so in areas where one might have expected this least, such as the rigidity of reference, which Kripke (1980) still grounded in metaphysical and externalist considerations. Arguably, though, rigidity follows from no aspect of the external environment, or 'semantics', at all. Viewed as an aspect of linguistic form, and specifically of how names function, there needs to be some internal mechanism that achieves this effect. Crucial to Uriagereka's naturalization of it is an important distinction between atomism, in the sense of Fodor (1998), and rigidity. The former is due to the mechanism of lexicalization, which consists in the idiomatization of a complex syntactic structure and thus the creation of an encapsulated item that, unlike most sentences, is listed in the lexicon. Rigidity, by contrast — arising, unlike atomism, at the outset of language acquisition rather than its final stages — is embodied most crisply in pointing, and thus it needs to be due to something else. What is it about names that they have got rigidity in addition to atomism, while nouns don't, and how can sentences be rigid even though they lack atomicity? Enter phasal Transfer: Periodically, as with Boeckx' model, descriptive information contained in the derivation is discarded, or lost. The way to think about this, Uriagereka suggests, is as the elimination of parameters in an algebraic equation: Taking them away does not allow us to independently vary different descriptive factors any more, and the inflexible or 'flat' object resulting, complex as it may be (in the case of sentences), cannot refer but rigidly: It cannot *adapt* any more to the specific features of an object, as required when referring descriptively. In the case of names, this Transfer is induced by the very lexical nature of names.

VII

Remarkably, no one at the workshop even raised the issue of semantic variation, or parameterization. Meaning as configured in language may be so abstract that it simply cannot be subject to cross-linguistic variation, any more than, on some current minimalist views, the computational system of language is. But, if this is so, one wonders where semantics comes from. Again, if syntax is not its format and cause, what is? Picking up on the task of distilling semantic effects of linguistic form, Anna Maria Di Sciullo asked the bold and topical question of

why sentences have truth values whereas words do not. Her answer was a purely structural one: Intrinsically, a word is a unit of morphological organization that as such is structurally too restricted to support propositional information. We could imagine a creature that had words only, but no phrases or sentences. Such a creature would have Merge, it would have compositionality, it would understand scope relations, there would be uninterpretable feature checking, and its words would be structured by the three broad categories that structure the human clause: predicate-argument structure (e.g. *writ-er*), aspectual modifications (e.g. *re-write*), and operator-variable relations (e.g. *th-*, *wh-*, *-ed*). This creature would equally grasp asymmetry and its intrinsic role in semantic interpretation; in fact, as Di Sciullo argued, it would have it in a stricter form than syntax does, which has symmetries as well, as in predicational [XP YP] configurations.

Yet, for all that, our morphological creature would not refer to the world in the way we do, and it would have a discourse interface of a radically different sort. This we see from the fact that symmetries in syntax, where they arise, are resolved quickly through movement, as per Moro (2000). As movement correlates with discourse semantic properties as opposed to thematic structuring, that is not something that the domain of the word will therefore support. It also follows that words will exhibit a radically impoverished contribution to information structure. Neither can words be arguments or exhibit referential specificity: As noted already in Di Sciullo & Williams (1987), words remain 'generic' in meaning. They cannot locate a specific predicate in time (cf. **At six, John is a writer*; and neither can *bank robber* be used to talk about an act of *robbing a bank*), we cannot extract from a word (*John admires Nixon* allows for *who does John admire*, but *John is a Nixon-admirer* does not allow for **Who is John an admirer*), a form of referential opacity that also transpires in the inability of anaphoric pronouns to pick up on the external reference of parts of words (**Book-shelving I like, especially if they [the books, or the shelves] are not heavy*); see Di Sciullo (2005) for more discussion.

Morphology, in other words, is a possible language in its own right, but it lacks what philosophers at least since Russell have paradigmatically associated with words, namely reference. It takes the fully expanded structure of the clause, specifically [ForceP [EvalP [CP ...]]] on Di Sciullo's model, for structures to qualify for a mapping to a truth value. This can be viewed as a formal specification of the pragmatic interface of the grammar; it is a structure that yields our 'sense of truth'. The asymmetry we see between words and sentences in regards to truth value evaluation reduces to a structural difference, an aspect of linguistic form. This is an internalist account of truth, in the sense of Hinzen (2006, 2007), also continuing the tradition of an internalism about meaning and the pragmatic determination of truth values, as characterized in Paul Pietroski's work (Pietroski 2005b).

While not addressing the issue of truth directly, Roeper and Hollebrande's take on recursion at the workshop may well stand in an intimate relation to it. Note that recursion as such is simply a formal property of human grammars that is just as 'autonomous' with regards to interpretive asymmetries as compositionality as such is: In fact, recursion as a mathematical property need

not as such intrinsically relate to semantics at all. And yet, as featuring in human language, it does, as Roeper showed: Recursion, in language, it turns out, is inherently a way of organizing and constraining semantic information (cf. also Hinzen 2006, 2007). Interpretive options are more open, if, rather than choosing for embedding one sentence in another, we let the two sentences form a discourse.

As Roeper and Hollebrandse amply documented, recursion is a construction-specific and language-specific phenomenon. Germanic languages have recursive nominal compounds, Romance languages lack them. English has recursive possessives, German, Swedish, and Dutch lack them. Bantu languages have recursive serial verbs, English doesn't. Most importantly, recursion, in whatever forms it does exist, is highly restricted. Most obviously, it is only particular domains of syntactic organization that productively 'recur' at all: Specifically, the cycle, or, in other terminology, the phase, is what can occur in itself. We don't get a recursive embedding of T in T, say, obtaining a hierarchy of the form $V-v-T-T-T...C$, or a recursion of $v-V$, as in $V-v-V-v-V-v-T-C$. Neither does C recur, directly. Any C-C-C sequence is in fact mediated by full phasal expansions in between: $[C-T-v-V [C-T-v-V [C-T-v-V...]]]$ (Arsenijević & Hinzen 2007). In the nominal domain, as well, DP doesn't embed DP, unless something else intervenes (**Bill's knowledge Fred's knowledge John's knowledge is incorrect*), as Roeper pointed out. *Within* a phase or cycle, nothing recurs at all. So recursion depends on the cycle, and if a cycle is thought of as a phase, then it is not even the case, strictly, that a phase occurs within a phase: For, as a new phase starts off, a large part of the previous one is already transferred, hence it is never in fact the case, at any one derivational time, that a structure contains two phases embedded in one another.

Restrictions on recursion can also be of a theta-theoretic sort (*the city's destruction by the enemy* or *the enemy's destruction of the city*, but not **the city's enemy's destruction*), or they can reflect on the extent to which embedded propositions can be evaluated (cf. **John considered Bill to consider the food to be tasty*; **John knew Bill to know Fred to be a liar*). Clearly, the task here is a typology of recursive constructions, and to test the claim that recursion, wherever it happens, is 'indirect', occurring via a phase boundary, as hypothesized by Roeper and Hollebrandse.

The issue of constraints on recursion is interestingly connected to another issue raised by Andrea Moro in discussion after Hinzen's talk. Moro (1997: 198) noted a curious fact about English. Consider the pair of sentences *John is sad* and *it's that John is sad*. On Moro's analysis, the structure of the latter sentence is the following:

- (1) $[_{IP} it_i \text{ is } [_{SC} [_{CP} \text{ that John is sad}] t_i]]$

That is, *it* is a raised pro-predicative element in what Moro terms an inverse copular construction, which in this case is interpreted as involving the assignment of a sentential predicate to the embedded proposition. Basically, this is the truth-predicate, given that *it's that John is sad* means *It's that it's true that John is sad* or *It's the fact that John is sad*. That said, it is interesting that the

construction in question does not recursively iterate: **It's that it's that John is sad*. This would be predicted if Hinzen was right that the evaluation of a sentence for truth is, in fact, the last thing that can happen to it: After that, recursion and compositionality stop. No truth-evaluated sentence (i.e. a declarative sentence whose truth is being asserted) can occur embedded: In *John believes Bill behaved suspiciously*, for example, the embedded clause must not be evaluated for truth, if the whole sentence's truth value is being determined.

As Roeper phrased this in discussion, truth may be like specificity. If something is referentially specific, it doesn't embed in something that's also referentially specific. If something has obtained full specificity, this is the end of it, and recursive structure-building stops. Unlike the *word* 'specific', which is a compositionally interpreted constituent of a larger structure, specificity is not, and neither is truth. Interestingly, if we say that it's true that it's true that John is pale, and then that it's true that it's true that it's true that John is pale, recursion is like an idle wheel: There is no semantic effect at all, virtually, and 'it's true' behaves like an identity function mapping one object back onto itself.

VIII

A final issue in any future 'biosemantic' enterprise, which Andrea Moro urged should be pursued relates to the question of what must a brain be like in order for it to process propositional, truth-evaluable information? This question depends on our ability to map propositions to brain space in the first space. New neuroimaging data that Moro presented (Tettamanti *et al.* 2008) suggest that we may be approaching a time where we can study the assignment of truth values to propositional units at a neurological level, hence a specifically propositional form of semantics as opposed to a lexical or conceptual one (which is rather standardly targeted in current neuroimaging work on 'semantics'; see again Pylkkänen *et al.* 2008). Arguably, negation is, inherently, a propositional operator, which reverses truth values. Using functional magnetic resonance imaging, brain activity was measured during passive listening of sentences characterized by a factorial combination of polarity (affirmative vs. negative) and concreteness (action-related vs. abstract). It turns out that sentential negation transiently de-activates a left-hemispheric action-representation system normally correlating with the processing of the negated, action-related sentence. If blocking access to the relevant area reflects the reversal of truth value, this study may provide for a fascinating inroad into caverns of the mind where propositionality and truth are housed. Indeed, on the assumption, defended by Moro, that negation is not available to non-linguistic species, this corner of our mind would be uniquely human as well.

IX

Form the above, it seems clear, at least philosophically, that there has been a major shift, away from locating 'semantics' in a relation between language and world, to locating it in the various *interfaces* that the grammar forms with extra-grammatical cognitive systems, particularly at the syntax-discourse interface. Within this frame, the following research questions seem particularly pressing:

- (A) What exactly do the domains of syntax and semantics involve, and what is uniquely human about them? If on the syntactic side this is recursive Merge, what makes this operation so fundamentally restricted across languages? What, on the semantic side, are the basic combinatorial operations?
- (B) How much of what is uniquely human about semantics depends on syntactic structuring? (Uriagereka's Question: How much semantic territory does syntax carve out?) For example, is the *word* a distinctive domain of semantic organization?
- (C) On what path can the naturalization of semantics proceed? On what relations between the respective ontologies of semantics and of syntax does a natural science of semantics depend?
- (D) Which aspects of meaning should (or shouldn't) involve access to extra-grammatical/mind-external factors like discourse contexts, inferences, reference to the external world or entities out there?
- (E) Can we discard the idea of semantic parameters and variation?
- (F) How can we make progress on the neuroscience of propositional and compositional semantic processing? What is it about the brain that makes it capable of propositional semantic processing?

Overall, it seems that these short remarks and reflections indicate a rich research landscape with an agenda that is relatively clearly defined and that calls for an open-minded community of interdisciplinary research.

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