Conceptual Structure and the Emergence of the Language Faculty: Much Ado about Knotting*

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Abstract

One perspective in contemporary linguistic theory defends the idea that the language faculty may result from the combinations of diverse systems and principles. As a case study, I critique a recent proposal by Juan Uriagereka and colleagues according to which the evolutionary emergence of the language faculty can be identified through studying the computational structure of knots as present within the fossil record. I here argue that the ability to conceptualize and, thereby, create knots is not parasitic on the ability to conceptualize and create language. On the contrary, these two domains are entirely distinct, unrelatable in terms of their computational complexity, expressive power or, most importantly, in terms of their requisite mental operations and principles. The overall approach defended here can profitably be employed in the study of the relationship between language and thought, as I briefly discuss in the case of the role of language in spatial reorientation.

1. The Elements of Language (and Thought)

In *Rules and Representations* (1980), Chomsky speculates that the linguistic capacity may be an assembly of different systems whose union results in the domain-specific mental faculty of language, a view later expanded, from a comparative and evolutionary point of view, in Hauser, Chomsky & Fitch (2002). Therein, they break the language faculty down into the following elements: a computational system (CS), a finite set of lexical items that the CS combines into more complex syntactic structures, and two external systems that 'interpret' these generated representations, the conceptual-intentional system (C/I; the 'thought systems', roughly speaking) and the sensori-motor system (SM; less roughly, the apparatus responsible for converting the linguistic structure into a physical signal).

This proposal has it that many (or perhaps most) linguistic properties are actually the result of conditions imposed by the two external systems. Thus, linguistic expressions exhibit hierarchy because of an imposition by the thought systems, while the constraints of the SM interface result in a physical signal that is linear and flat.

Note that if hierarchy is to be explained as a condition of the conceptual-intentional interface, a rather rich structure is *ipso facto* ascribed to this component. Granted, linguists have offered a very meagre description of this particular system, but they could not possibly be faulted for this –after all, it falls out of their range of study. All that linguists actually need in order to justify their research program is plausible evidence for the existence of such systems, including a demonstration that hierarchy is central to their operation.

In any case, much evidence for non-linguistic hierarchical cognition can actually be found in the cognitive science literature, a point I will come back to later. Here, it merely suffices to say that it is in fact possible to study the thought systems in the same terms that Chomsky (1980) employs for

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language. Thus, it is plausible to argue that belief-fixation, the raison d'être of 'thought', involves a proprietary vocabulary (i.e., concepts) that is manipulated by a computational system whose operations are compositional and systematic. What is then needed is a description of the rich, amodal representational system capable of merging information from different modalities –the system that accounts, among other things, for the flexibility and productivity of thought.

I will not treat the relationship between language and thought in any great detail, however, even if some relevant issues of their association will necessarily be touched. Rather, this essay addresses a specific proposal regarding the emergence of the language faculty and the expressive power required to conceptualise and/or generate certain artefacts found in the fossil record of the modern human species; namely, that offered by Camps & Uriagereka (2006), and Balari et al. (2011).[1] I will attempt to show that these studies are predicated on a fanciful characterisation of the Chomsky Hierarchy, a misappropriation of Knot Theory from mathematical topology, and a frankly bizarre view of the mind. Furthermore, the examination of this proposal will allow me to discuss some issues on how conceptual and linguistic structure may be related, which could perhaps offer a preliminary explanation for, as I will eventually examine, some much-discussed experimental data on the role of language in cognition.

The essay is organised as follows. In the next section, I summarise some evidence for the existence of thought without language. This will provide a frame for the discussion in section 3, where I will focus on the main issue of this paper: conceptualisation and the fossil record. As a conclusion, section 4 will outline the general approach defended here: a framework that involves characterising cognitive domains/abilities in terms of the assemblage of different 'elements'; an approach that allows us, or so I will argue, to pinpoint the actual cognitive property/ies responsible for a given behaviour.

2. Thought without Language

There is an intuitive sense in which thought is possible without language, not least because of the manner in which the latter appears to underdetermine the former. Slips of the tongue, deictic reference and paraphrases are some of the phenomena in which the employed linguistic vehicles are neither rich nor accessible enough to represent the corresponding thoughts (Gleitman & Papafragou 2005).[2]

Perhaps more clearly, pre-verbal infants are capable, among other things, of perceptual integration and categorisation (Flavell, Miller & Miller 2002), and these are abilities that require mental representations and hierarchical operations. Crucially, the mental representations at hand cannot be the atomic elements of language (words or lexical items) for these are yet to be acquired; rather, it has to be something prior to them: concepts. I am, therefore, defending the postulation of a rich conceptual structure, an amodal representational system in the sense of Fodor's (1975) "language of thought"; that is, a system that can explain, *inter alia*, the flexibility of cognition (as in the possibility of merging information of different modalities in the fixation of belief) and "effability" (the inter-translatability of any propositional content from language to language; Katz 1978; cf. Jackendoff 1978).

Naturally, acquiring a natural language effects a great cognitive upheaval; a languageless mind is an impoverished one. Nevertheless, cognitive enrichment should not be confused with an augmentation in expressive power. That is, many of the relations that linguistic expressions convey must be representable beforehand if they are to be acquired, as it is not at all clear how an organism

[1] The views defended in these publications are partly based on chapter 7 of Uriagereka (2008), which I will also discuss here.

[2] Carruthers (1996, 2002) attempts to circumvent these phenomena by arguing that thought is entertained in terms of LF representations, which are somehow accessible to introspection. He clearly employs LF for something it was not meant to do –i.e., representing thought instead of different structures of the same string– but, anyhow, this now-defunct linguistic level (Chomsky 2005) doesn't range over all the phenomena I have listed. On the other hand, Carruthers (2007) appears to allow for non-linguistic thought in a sense that is closer to what I have in mind here.

could acquire/invent a system of more expressive power than the one it already has, as Fodor (1975, 1979) has forcefully argued (this phenomenon has come to be known as the "learning paradox" in the educational literature; see Bereiter 1985).

Cognitive enrichment comes in degrees, though; therefore, the literature on linguistic relativity can be seen as an attempt to work out the effects that different languages have on our modes of thinking. It is, however, very unlikely that stronger versions are warranted; after all, communicative gaps between languages have not been shown to be paired with corresponding conceptual gaps (see Gleitman & Papafragou 2005 for a review of the experimental data).

The main point of this brief discussion is that "conceptualisation" appears to be independent of language, even if a specific state of the language faculty, an I-language in the sense of Chomsky (1986), to give it a more precise formulation, can have an influence on thought processes. After all, language *can* be used as a tool for thought, as in inner speech (cf. Jackendoff, 1997, ch. 8).

I don't think any of this is controversial, but it is worth discussing some recent views by Chomsky and others, as they seem to have a subtly different take on things. Having said that, it is certainly the case that Chomsky's recent views on the relationship between language and thought have to be gathered up, and almost deduced, from the scattered and brief comments he has made in his most recent writings, and so my interpretation of this view may overstate the discrepancy I see with the position I will be defending here. Nevertheless, the general idea has been adopted and carried forward, I believe, by some scholars close to the so-called "biolinguistics" enterprise, and this is clearly the case for the works I will be critiquing in the next section; hence, the following discussion.

It is important to note that Chomsky *has* allowed for non-linguistic thought, at least in the sense of a depository of concepts in which our thoughts are couched (e.g., 1993, 2000, 2001), which may be part of some of the non-linguistic mental phenomena he has discussed over the years, such as the "common-sense" faculty (Chomsky 1980) or 'the systems of planning, interpretation, reflection' (Berwick & Chomsky 2011, p.20). Further, children acquire the meanings of words so rapidly and, in most cases, after a single exposure, that it is reasonable to suppose that they are mapping what they hear to a pre-existent mental particular; a concept. Despite that, he has also manifested some scepticism for an "independent" language of thought (2007a, p.16), but he seems to discuss the issues at hand in a way that collapses language and thought into one phenomenon. It is one thing to state that the CS underlying language (i.e., Merge) provides the mechanism for generating an unbounded number of structures, it is another thing completely to conflate lexical items and concepts into the atomic units this system operates over. That is, the CS may well be domain-general, but the structures it manipulates do play different roles in cognition, and consequently, they are part of different cognitive domains. Segal (2001) frames this point as a difference between having a structure and putting it to use, as exemplified by the following pair of sentences: *Mary's Ferrari is faster than any Lotus* and *If Mary's Ferrari is faster than any Lotus, Pete will eat his hat* (p.127). Note that the word "any" can only mean "every" in the first sentence, while it can either mean "every" or "at least one" in the conditional. The two different interpretations cannot be derived from the surface structures, however, and even though the language faculty *does* employ underlying operations and principles to match the corresponding "meanings", the latter must surely be previously available in conceptual structure (as in the Fodorian "impossibility" argument alluded to before).

The underlying principles and operations I have just mentioned seem to be what Collins (2007) has in mind when he talks of the "big fact" that language and thought structures don't fully match up, as evidenced by intrinsically linguistic constructs such as raising/control pairs, c-selection, and much more. Still, I suppose he would agree that these theoretical constructs are postulated in order to meet the interface conditions the thought systems impose; if so, the structure underlying thought must be independently given.

The position defended in Chomsky (2007a, 2007b), Hinzen (2011), and implicitly in the works I will discuss in the next section, however, seems to depart from the outline I have provided. It seems that these scholars are attempting to somehow modify the Aristotelian dictum that language is sound with meaning into a view of language as a system of meaning (thought) only accidentally connected to sound. *A fortiori*, then, linguistic "derivations" ought to be viewed, according to these scholars, as a kind of a "language of thought". I take this is what Chomsky has in mind when he states that language is primarily an "instrument of thought" (2007a, p.17), but there is a certain circularity in the suggestion that language is the vehicle of thought *and* the overall attempt to explain how linguistic derivations meet the conditions imposed by another system of the mind, the C/I interface.

Be that as it may, and for the reasons I have outlined above, I consider postulating an independent conceptual structure as unavoidable. Consequently, I view the derivations that linguists propose not as the vehicles of thought itself, but rather, as the possible mappings to the thought systems; hence, two different but related cognitive domains. I will elaborate on this point below, but for now the discussion provided in this section suffices.

3. The Fossil Record and Expressive Power

Camps & Uriagereka (2006; C&U) propose a way of determining when the language faculty emerged in the evolution of the human species that is based on an analysis of specific behaviours and the mental machinery necessary to account for them. Naturally, the relevant behavioural phenomena would have to have a 'cognitive base' that can be said to be in a 'causal correlation' with the linguistic capacity; if so, such behaviours could plausibly constitute indirect evidence for an 'underlying linguistic prerequisite' (p.35).

In order to study this 'causal correlation', they focus on certain computational properties of the CS at the heart of language. Taking their heed from the classification of formal grammars and languages that Chomsky (1956) delineated –the so-called Chomsky Hierarchy, CH hereafter– C&U reinterpret this construct, following Uriagereka (2008)[3], as a hierarchy of the memory capabilities of different mechanical automata. Furthermore, and supposedly in concord with well-known results in mathematical linguistics, the CS for language is taken to be an information-processing mechanism (Balari et al. 2011, p.2) of context-sensitive power, where "context" is to be understood as the 'history of rule application' (C&U, p.40). Moreover, they tell us, we can study the 'mathematical structure' of some of the objects found in the fossil record (C&U, p.45) in order to establish the algorithm that computationally describes/generates the rule-governed behaviour that created these objects (Balari et al. 2011, p.10). If the fossil record exhibits objects that would require a context-sensitive CS, this is one way to determine, these scholars claim, whether a given hominid was in possession of the language faculty (ibid.).

According to C&U, the fossil record *does* exhibit such objects, as in the case of knots, a type of artefact that is to be found, among other settings, in the binding of projectiles to their shafts (p.58). In order to prove their point, they link their discussion to a subfield of mathematical topology –Knot Theory (Knott, for short)– which in very general terms studies the mathematical characteristics of knots. Citing a work from this literature (viz., Mount 1989), they affirm that knots can only be created/described by a context-sensitive system, a conclusion they take to be 'not subject to rational debate' (p.63). The corresponding discussion in Balari et al. (2011) is somehow more qualified, but they essentially reach the same conclusion; citing another study from Knott (i.e., Hass et al. 1999), they conclude that determining whether a string is knotted or not is of a computational complexity comparable to the processing of linguistic expressions (p.11). Given that evolution doesn't, apparently, generate identical structures (C&U, p.45), the ability to create knots may be parasitic on the computational power of the language faculty. Furthermore, knots are not found in the artefacts of the Neanderthals (ibid.), only in modern humans, and this would be another reason to believe that the appearance of knots may corroborate the stage in evolution in which hominids evolved a language faculty.

[3] Curiously, C&U state that their description of the formal properties of computational systems is based on ch. 7 of Uriagereka (2008), which they take to be a 'current linguistic perspective' (p.36), even though, as I will show below, it is clearly a radical reinterpretation of the CH (and was not even published at the time).

The overall argument, then, is clear enough. It is, however, an entirely fallacious one, predicated on a fanciful reinterpretation of the CH, a misappropriation of Knott, and an incoherent view of the mind. I will now discuss these three points in order.

As mentioned above, ch.7 of Uriagereka's *Syntactic Anchors* book (2008) constitutes a reinterpretation of the CH. Therein, he makes the correct point that the CH pertains to what has been called weak generative capacity. Indeed, Chomsky ranked formal grammars, qua production systems, in terms of the sequences of symbols (i.e., strings; the corresponding formal languages) that they could generate. Proceeding in this way, he showed that certain linguistic expressions could not be generated by some of the grammars he identified, but he did this by abstracting away from the underlying structure of these sentences –that is, the linguistic expressions were paired with sequences of lower- and uppercase symbols (for terminals and non-terminals, respectively). The overall conclusion, then, was a point about expressive power: it identified the sort of strings a given formal grammar could generate, which says something important about the language faculty (see infra). Furthermore, the original classification included a progression from finite-state to context-free and then context-sensitive grammars (more grammars were to be discovered later), but the word "context" here makes reference to the material that appears on either side of the arrows of rewriting rules, not to the history of rule application; an entirely different matter.

For his part, Chomsky has been very explicit in stating these results to be very limited (e.g., 2004, p.175), as the linguist is fundamentally interested in structure generation; that is, the study of strong generative capacity. The problem is that no-one has been able to formalise the notion of strong generative capacity (see Frank 2004 for discussion), and even though Uriagereka (2008) attempts to re-focus the CH for a perspective that is sensitive to these concerns (p.489), his analysis is inadequate and unhelpful. He doesn't, as a matter of fact, offer any formalisation; what he does offer is a (misguided) attempt to relocate the CH to another level of explanation, as I will now argue.

A standard description from mathematical linguistics would state that (formal) grammars generate (formal) languages, and automata are those abstract computing devices that can *recognise* the generated languages (see Hopcroft et al. 2007). Note that Automata Theory introduces a number of variables, such as memory and time, that don't apply to the study of the formal grammars. That is, the study of the latter focuses on the mapping function between input and output and the formal properties that derive therefrom –what is usually called the *theory of the computation*– while Automata Theory studies the so-called *models of computation* –in the latter, the focus falls on how algorithms are evaluated in terms of time and space growth (Aho et al., 1974, p.2). The preoccupations of Automata Theory, then, revolve around discovering the inherent computational difficulty of various problems.

Naturally, a hierarchy of automata can be outlined so that it closely corresponds to the grammars and languages of the CH, and this is effectively the strategy that Uriagereka (2008) follows. His proposal, however, is beset with numerous infelicities and misunderstandings. Firstly, he mistakenly characterises automata as structure-generating machines (p.490), but this is not correct; automata are *recognising* machines: they accept those strings that are in the class of languages that each automaton is specified to recognise. Secondly, and as a consequence of the first point, he restates the CH in terms of the different memory capacities of different automata, applying its consequences, piecemeal, to a study of *competence*, even though this level of explanation abstracts away, by definition, from considerations of memory load. Strikingly, he offers not a single reason to believe that memory capacities matter for a study of competence (apart from an allusion in the Introduction of his book); more importantly, focusing on automata hierarchies and different memory capacities does not *ipso facto* turn the CH into a study of strong generative capacity, it merely moves the theory to another level of explanation, mistakenly.

Nevertheless, what one finds in the remainder of chapter 7 verges on the bizarre. He proceeds to describe the internal elements of a competence-level analysis in terms of the different memory stacks available, so that the two varieties of Merge (External and Internal) end up at different levels in the CH (p.504), but only because the "context" of context-free/context-sensitive grammars is redefined as the history of rule applications. Consequently, different structures require different memory stacks (so that the right rules can be kept in memory for the derivation to proceed), placing External Merge at the level of a context-free system and Internal Merge as a context-sensitive system (ibid.). In the end, the competence/performance distinction is so blurred that he connects the "derivational" memory of the CS to what psychologists call procedural memory (p.507), but the latter is surely and purely a matter of processing. The CH is finally reduced to a "formal" characterisation of computational systems in which sets of objects are finite-state, syntactic phrases are context-free and "chains" (the relation between a moved element and the copy it leaves behind) are context-sensitive (p.545).

Moreover, he makes much of the relationship between mathematical and linguistic structures, but with very little justification as to why they are to be related at all. Indeed, the overarching feature of this chapter is its wild metaphorical interpretation of many mathematical constructs, which Uriagereka employs in a way they were not meant to be used –losing their explanatory power. At one point, he emphasises the connection between number "planes" (real > hyper > hyper complex) and the linguistic "spaces" he sees in thematic ordering in language (achievement > activity > state), but it's not obvious what insight, if any, this provides. He eventually takes each Aksionart to be a sort of (mathematical) "manifold", and I suppose there is a short step from here to the connection between language and knots.[4]

Going back to Uriagereka's reformulation of the CH, there is a distinction to be had between a *theory of the computation* and a study of *models of computation*, which is as valid in cognitive science as it is in computer science. Much has been gained in linguistics, in fact, by treating competence as a function in intension that specifies, starting from lexical items, sound-meaning pairs.[5] In a study of these characteristics, one postulates abstract computations, but these are treated in functional and therefore abstract terms. Thus, the derivation is divided into atomic components, cost units can be assigned to each, and this results in a sequence of stages. Nevertheless, this just means that one stage is followed by another, making the immediate predecessor relation the central feature of the analysis. Further, by employing a size metric and an asymptotic notation, additive and multiplicative factors can be eliminated.[6] This is precisely the type of work that generative linguistics has undertaken in the last 60 or so years. In short, sequential operations should not be confused with memory stacks and the like; furthermore, no connection should be drawn between strong generative capacity and the computational complexity of automata, as they pertain to different levels of explanation.

At heart, C&U and Balari et al. (2011) are confused about the nature of the system they are studying. The latter, in fact, reference classic papers by Putnam and Fodor when they define language as a natural computational system in terms of an information-processing mechanism, but the position defended in the papers they cite is in fact not compatible with the object that biolinguistics –the approach they claim to be following– is concerned with. Biolinguistics studies the function in intension that generates sound-meaning pairs –the theory of the computation underlying language– while Fodor and Putnam have defended the so-called computational theory of mind, the thesis that real-time mental processes are computational in nature. This could be framed in terms of a distinction between *faculties* à la Chomsky, and processing *modules* à la Fodor (see Collins 2004 for a similar point). Given this confusion, there is a small step from the statement that creating a knot involves a context-sensitive system to the claim that this complexity is 'comparable to the one needed to process linguistics expressions' (Balari et al. 2011, p.11). The very last quote is clearly misplaced, however.

[4] I realise that Uriagereka is carrying out a conscious exercise in speculation in this chapter, but this often results in a succession of "eureka moments" –or "uriagereka moments", for wont of an appropriate antonym– that is likely to hamper and obscure research, no enlighten it.

[5] This is effectively the characterisation Chomsky (1980) provides for the locution "knowledge of language", a view of the subject-matter of linguistics that finds some support in philosophy of language studies (e.g., Smith 2006, Matthews 2006, Collins 2008).

[6] This is clearly the approach that is followed in much of theoretical computer science (e.g., Knuth 1997), and it should not be confused with practical, engineering matters (cf. Johnson & Lappin 1997). I take that many theoretical constructs within the minimalist program (such as the "minimal link condition", "probe-goal" relations, etc) are meant to circumvent memory-like considerations in precisely this manner.

We know that the expressive power of natural language is (mildly)context-sensitive (Joshi et al. 1990), but the system underlying linguistic comprehension is clearly a finite-state automaton, something that was already pointed out more than 50 years ago (Chomsky 1963, p.309). That is, the computational complexity of a problem is clearly a different matter altogether from the expressive power of a grammar. This brings me to the next problem with the account in C&U and Balari et al. (2011): the misapplication of Knot Theory.

As in any other subfield of mathematics, Knott is a rather narrow and technical discipline, which should already cast doubt on whether it can tell us anything about such worldly things as tying a knot in real life. Indeed, the knots that Knott studies have nothing to do with real knots, as these are closed structures; basically, a mathematical knot is an embedding of a circle into Euclidean 3-space (Burde & Zieschang 2003, p.1). Moreover, the main line of research in this field is extremely narrow: they try to figure out which two knots are isotopic and which are not, where two knots are said to be isotopic if one of them can be transformed into the other by following step-by-step moves. This is called the knot recognition problem, and it involves working out the formal equivalence of two knots. A special case of this problem concerns the so-called "unknot", a closed loop without any knot in it, as shown on the left-hand side of Figure 1 below. The "unknotting" problem, in turn, involves specifying an algorithm that can recognise the unknot in a figure like the one found on the right-hand side of Figure 1.

Figure 1. The unknot and a non-trivial knot

Relevant to the issues I am unearthing here is the so-called Reidemester moves, a set of well-defined combinatorial moves that can disentangle a knot without damaging it. There are three such moves: twist/untwist; move one strand over another; and move one strand over/under a crossing (Manturov 2004, p.12).

Naturally, none of this has anything to do with how you go about tying a knot, let alone the computational complexity required to do so. The actual details of Knott go completely unmentioned in C&U; its relevance to real-life, knot-tying abilities just assumed. Balari et al. (2011) do point out that Knott deals with 'elastic, closed, and tangled knots' (p.11), but they go on to claim that 'formal details aside' (as if they were of no importance),

'the task of determining whether any string is knotted is known to have a complexity comparable to the one needed to process linguistic expressions' (ibid.; Reference: Hass et al. 1999).

and a bit later:

'(un)tying knots (or determining whether a tangled string is knotted) seems to require an underlying computational system of Type 1' (ibid.; Type 1 in the CH: context-sensitive).

There are two things at fault here. First is the claim that Knott involves 'determining whether a string is knotted', something that is clearly *not* the case, as Knott takes tied knots as its starting assumptions –indeed, Knott narrowly focuses on the equivalence problem outlined above. The other problem is to treat (un)tying a knot and determining if a string is knotted as if they are equivalent, but there are no reasons whatsoever to believe so. Furthermore, the reference Balari et al. (2011) include in relation to this (viz., Hass et al. 1999) is clearly misrepresented. Rather, Hass et al. (1999) proved that an algorithmic solution for the unknotting problem is in the complexity class NP, which is to say that the algorithm will define multiple ways of processing the input without specifying which one it will take, in polynomial time. This has *no* relation to the context-sensitive expressive power of language, or the complexity involved in language processing; it also has *no* relation to the complexity of (un)tying a knot.

Nevertheless, this is not to say that (un)tying a knot may well involve a non-trivial computational system, but we don't have an account of this (and the authors themselves do not even bother to attempt one). At one point, however, Balari et al. (2011) envision what may actually be involved in making a knot; one must relate, they tell us, a segment of the knot with the background, and this may well involve 'grouping and long-distance-like relations' (p.11).[7] This insight comes from C&U, in fact; therein, they briefly describe a possible transformation of a string into a knot by assigning a specific number to each segment, and these symbols can in turn be manipulated by a (context-sensitive) grammar. They don't provide a proof of this, but the underlying idea is not incoherent. Turing (1954) discusses a similar issue in relation to solvable and unsolvable problems in Knott. As noted, a knot is a closed curve in three dimensions, but it can also be accurately described 'as a series of segments joining the points given in the usual (x,y,z) system of coordinates' (p.585). Further, a set of symbols can be employed to represent unit steps in each coordinate direction (say, *a's* and *d's* for the X-axis, and so forth) so that transformation moves can be modelled by substitution rules of the production systems variety.

This is, in fact, the terms in which C&U claimed that Mount (1989) showed the necessity of a context-sensitive system to create knots; a conclusion, it will be recalled, supposedly 'not subject to rational debate'. In fact, Mount (1989) is an unpublished computer manual for a program devised to assist mathematicians in the study of Knott. At one point (p.4), he discusses the Reidemester moves I outlined above, and remarks that the transformation of one knot into another may be reduced to a grammar problem, in precisely the terms Turing (1954) discusses. Later on, it is again remarked that 'the Reidemeister moves could be rephrased as some kind of context-sensitive grammar' (p.5).
Note what is actually being claimed here. First, that the Reidemester moves could be modelled by a context-sensitive grammar. That is, for the (narrow) purposes of Knott, a production system may be employed to study the knot recognition problem. Again, this has nothing to do with the computational complexity of (un)tying a knot. More importantly, this is not even a proof, but a passing comment; no more than that. It is rather astonishing that this could become a conclusion 'not subject to rational debate'.

Be that as it may, I have so far focused on technical issues. Let us grant them, for the sake of the argument, that (un)tying a knot does in fact involve some sort of CS. The relevant question is: why would this have any relation to the faculty of language? Why couldn't knot-tying be a domain of other systems of the mind? C&U briefly consider this possibility, but reject it, for various reasons. One of them, it will be recalled, had to do with their supposition that evolution doesn't duplicate identical structures (p.45). We can dismiss this qualm right away, given that there is no evidence that knot-tying and language have the same expressive power, or share an equal computational complexity in processing. Another reason for dismissing the domain-general possibility follows from their rather strange view of the mind. Not only do they seem to assume that whoever produced a knot had modern syntax (as if the CS as applied to lexical items was literally employed in other domains); they,

[7] The rather obscure reference to long-distance relations is meant, I suppose, to relate to the fact that the expressive power of language is at least context-free because of long-distance relations in syntactic structures. However, this is impossible to relate to the way they actually frame the computational issues, as they don't focus on expressive power, but on the different memory capabilities of different automata.

moreover, appear to believe that evidence for a "mental module" specific to knot-tying would have to be provided to refute their hypothesis (ibid.). At this point, it is worth discussing some general features of mental architecture.

I have already pointed to a distinction between Chomsky's faculties and Fodor's modules above, even if these two positions are usually taken as paradigmatic examples of "modular" approaches to the study of the mind. Whilst it is true that both inherit certain principles and properties of old-style "faculty psychology", they have been influenced by different traditions within it. Thus, Chomsky (2006) credits the work of the 16th-century humanist Juan Huarte as a pioneer in the study of the intensional generative systems he is interested in, while Fodor (1983) acknowledged the influence of Franz Gall in the outline of the vertical systems he proposed (basically, only peripheral, perceptual systems were said to be modular). The discrepancy is perhaps clearest when we take language as the object of study, as Chomsky's approach focuses on the principles and properties that generate sound-meaning pairs, while Fodor is interested in the operations of the linguistic parser – hence, the identity conditions he put forward for modules (they are fast, mandatory, etc), which don't quite apply to the language faculty. Indeed, these two constructs –faculties and modules– do not only respect different identity conditions, their study involves differing methodological and explanatory constraints. In this sense, they are sort of different mental realities. It is not obvious at all which construct C&U have in mind when they talk of a knot-tying "mental module", as no further details are provided.[8] Still, I will now argue that we should not be so compelled to search for a domain-specific system in order to reject their hypothesis.

The general outline of the mind that Fodor (1983) defended will be useful for my purposes here. As mentioned, he ascribed modular status to perceptual systems (such as vision, hearing, the language parser, etc), while the so-called "central systems", the locus of belief fixation, were stressed to be non-modular, as in principle any source of information may be relevant in reasoning, planning, etc. Now, the overall linguistic capacity, that is, the system that is composed of both the CS that creates sound-meaning pairs, and whatever mechanisms and operations that put these pairs to use (that is, the parser), would obviously cut across this modules-central systems space, as the generation of sound-meaning pairs involves interfacing with external (to the language faculty) systems.[9] More importantly, as already noted, the components of the language faculty (and perhaps those of the parser too) are very likely domain-general mechanisms (expect perhaps the feature-matrices of lexical items). Therefore, it is possible to theoretically identify those systems that account for a given behaviour *without* claiming that such-or-such behaviour is parasitic on such-or-such domain, unless the necessary assembly of mechanisms identifies the same domain-specific conglomeration.

In the case of knots, there doesn't appear to be any reason to believe that any more than conceptual structure is required to account for the evidence. An account of how knot-tying proceeds might well be something like this: external information would be transduced by the visual system into the mind, the information so gathered would then have to be conceptualised into the right mental particulars (the string concept, the knot concept, etc.), and a CS would combine these concepts into whatever structures underlie knot-tying (including the "plans" that would then be transformed into motor responses).[10] Naturally, it is very likely that knot-tying would proceed in a trial-and-error fashion, with a constant updating between perceptual information and its conceptualisation. Crucially, all I am postulating here is a grouping involving a CS, a set of concepts, and two interfaces: perception and motor skills. What makes language language, however, is the assembly of a CS, a set of lexical items, and the C/I and SM interfaces. Thus, we should never come to believe that if a different cognitive domain makes use of *some* of these systems, it is *ipso facto* making use of

[8] I won't discuss the "learning modules" of Gallistel (2006), which C&Y also mention, but these are much closer to Fodorian modules (after all, they are Turing Machine-specified mechanisms) than to Chomskyan faculties (despite Chomsky's approval of these systems; Chomsky 2005).
[9] I ignore the issue of whether the central systems can be divided into so-called "conceptual" modules, as defended by many scholars (for an overview, see Carruthers 2007). Nothing hinges on this here, but I do take the view, following Samuels (2006), that Fodor (1983) is more or less right.
[10] This is an idealisation, obviously. It is very possible that many knots in history came about accidentally (Stabler p.c.).

intrinsically linguistic properties, no matter how cutely corresponding certain problem-solving tasks appear to be. As I have tried to show, the connection between the expressive power of language (more specifically, the specific rules of its underlying grammar) and the computational complexity of knot-tying is more assumption than fact.

4. Language and Thought

The overall strategy of decomposing cognitive domains (in this case, the language faculty and conceptual structure) into atomic elements is rather fruitful for elucidating the relationship between language and thought. Take, for instance, the particular case of the role of language in spatial cognition.

In a series of papers on spatial re-orientation tasks –tasks in which subjects are disoriented inside a room, and then asked to find their away around by employing geometrical and non-geometrical (featural) information–, Hermer-Vázquez and colleagues (Hermer-Vázquez et al. 1999, Hermer-Vázquez et al. 2001) have argued that natural language plays a crucial role in combining these two sources of information, as exemplified in sentences line 'the ball is to the left of the short white wall'.

Apparently, children under the age of 5 cannot conjoin these sources of information in certain experimental settings, perhaps because they are yet to acquire the relevant linguistic representations. Similarly, adults also fail to adjoin this information if they carry out a secondary linguistic task such as speech shadowing, but not when the concurrent task involves rhythm-clapping shadowing. This suggests, to Hermer-Vázquez et al. (1999) at least, that language mediates the union of these two non-verbal representations, even if they *can* separately be entertained in a language of thought. In this sense, language augments your representational power, as it conjoins representations that cannot be so merged in the language of thought (for some unspecified reason). In a perhaps stronger flavour, Carruthers (2002) suggests that language in fact links up the different (conceptual) modules of the mind; it is the inter-modular language of the mind.

There are many doubts about all of this. Firstly, note that the three works just referenced allocate modular status to these bodies of information, perhaps an unwarranted assumption. After all, it is not clear that either of those two sources of information forms a self-contained body of structural properties mediated by domain-specific mechanisms, let alone that they need any special mechanism to connect them. As mentioned, Fodor (1983) described modules in terms of a rather restricted set of properties (such as speed, automaticity, encapsulation, etc), and this had the advantage of individuating modules as a phenomenon so narrow as to make explanation possible –indeed, it is because they are modular that they can be scientifically studied at all. Again, Fodor allocated modular status to peripheral systems only; that is, psychological mechanisms that operate regardless of general information (and many other factors).

Moreover, it turns out that languageless creatures manage to accomplish the merging of geometrical and non-geometrical information just fine (see references in Twyman & Newcombe 2010), and there is no reason to expect the same not to be true of nonverbal infants. In fact, 18-month-olds demonstrate just this capacity when the size of the experimental setting is big enough (ibid., p.1324).

Nevertheless, the experiments suggest possible *processing effects* due to language and these ought to be explained in the terms I have described before; that is, in terms of what elements language and thought share, and how they interact in real-time processing. The regressive study carried out by Hermer-Vázquez et al. (2001) points in this direction, in fact. Therein, they attempted to find correlations between possible factors, and found that the only one that reached statistical significance was the linguistic production of phrases involving terms like *left* and *right* (the sample was rather small though, N=24; furthermore, this presupposes having the concepts LEFT and RIGHT, not a trivial matter). One ought to emphasise that it was linguistic *production* that was found significant; that is, a performance phenomenon, not a (transparent) fact about mental organisation.

The relevant question to ask, then, is: how is it that production (as in the speech shadowing condition) blocks merging of geometrical and non-geometrical information? It cannot be that verbal shadowing constitutes a secondary task that impedes the primary task of merging these two sources because they both share a specific component (productive language). The evidence alluded to above clearly shows that this integration can be done in thought, and certainly without any sense of being conscious of it. If it were not so, one could then wonder about the actual character of the sentences that subjects represent to themselves. The example I mentioned earlier on –viz., 'the ball is to the left of the short white wall'– is of course only one way to codify this information, but there are equivalent ways to do this: 'the short white wall is to the right of the ball'; 'the wall is short and white', 'the ball is right next to it'; 'the ball is to the west of the non-black, little wall', *e così via*. Are we to ascribe/project any of these representations to the participants? Would this have any effect on the interpretation of the data? If we are being serious about linguistic production being the necessary link between these "modules", surely this would be relevant.

Actually, the only plausible interpretation is that adult subjects may well be representing the task to themselves in inner speech, and speech shadowing may consequently be interfering with this; that is, these tasks may be probing the effect of inner speech and speech shadowing on attentional mechanisms, including the conscious reflection on how to solve a task. Nothing beyond this appears to be warranted.

Samuels (2002) is almost right when he points out that speech shadowing is a language production task that, according to standard accounts, involves, *inter alia*, the integration of communicative intentions. He then points out that there are in fact two sort of integrations to carry out in the speech shadowing version of the experiment: on the one hand, the integration of geometrical and non-geometrical information; and on the other, integrating the communicative intentions behind the linguistic message. It is plausible to claim that both integrations take place in the "central system" (in the language of thought), which would surely cause a significant memory load; or, at least one greater than in the rhythm-clapping condition; hence, the different results.

To be more precise, speech shadowing is a *type* of linguistic production, as it certainly does not incur the same integration load as normal production. It *does* involve some load in this sense, but it also engages whatever cost comprehending the sentences to be shadowed imposes. That is, speech shadowing is not an automatic parroting of the material you hear. Marslen-Wilson (1985) provides evidence that both fast and slow shadowers process the syntax and semantics of the input material before they integrate it into the sentences they output. That is, response integration and its execution are important factors in speech shadowing. It may not be a full-blown case of comprehension *or* production, but properties of both processes are operative. We should then expect that this causes a working-memory overload stemming from the two cognitive processes at play: perceptual integration and decision-making, and language comprehension and production.[11]

It will have been noted that Samuels frames his discussion in the very terms I have advocated. Indeed, he makes an effort to decompose the task into atomic operations (possession of the right concepts, perceptual integration, communicative intentions; and what I added regarding comprehension) to then place them in the right mental loci (syntactic parsing would involve the language faculty and its parser only, while semantics and intentional content would clearly engage the thought systems; plausibly, perceptual integration is carried out in the central systems). This is a necessary step if we are to understand what is going on in the experiments. A corollary of all this is that the data seem to be telling us much more about processing modules and their cognitive load than anything about the organization of mental faculties, but this is only evident once we distinguish between faculties and modules and focus on the components they might share (and on how they might relate). This may well be a very fruitful way of studying the relationship between language and

[11] Surprisingly, Carruthers (2002) dismisses Samuels's point while ignoring the evidence that speech shadowing involves both (a type of) comprehension and (a type of) production; indeed, a type of linguistic performance that *does* involve the central systems. He specifically denies all this, referencing an out-of-date study of speech shadowing (p.712)

conceptual structure, as I have tried to show in the case of figuring out the mental abilities underlying the creation of certain objects, with all that follows in the study of language evolution, etc.

The discussion I have provided in this paper, however, should not be viewed as a criticism of the biolinguistics project. If this project is to be understood in the terms defended in Lennenberg (1967) and Berwick & Chomsky (2011), then its overall aims and methods have much in their favour. What I have done here is critique a specific proposal coming from biolinguistic circles, as I believe it to be based on fundamental error –both in terms of how mathematical constructs are to be applied to the study of cognition, and in its characterisation of mental architecture and properties. If so, the present essay should be viewed as an attempt to engage biolinguistic concerns in the study of general cognition.

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