★ FORUM ★

Knots, Language, and Computation: More Bermuda than Love

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[T]he greater part of what is said and written upon it is mere windy talk, the assertion of subjective views which commend themselves to no mind save the one that produces them, and which are apt to be offered with a confidence, and defended with a tenacity, that are in inverse ratio to their acceptableness. This has given the whole question a bad repute among sober-minded philologists […].
W.D. Whitney, *On the Present State of the Question as to the Origin of Language*

[t]he relevance of Ristad's results, regardless now of their accuracy […]
S. Balari et al. (2012: 94)

1. Introduction

We commend Balari *et al.* (2012; BEA2 henceforth) for managing to wring a 33-page response from a 9-page critique (Lobina 2012a) of the arguments put forth in Camps & Uriagereka (2006; C&U) and Balari *et al.* (2011; BEA1).[1] And we certainly welcome the detail and slow pace; good attributes when the understanding of eclectic minds is at stake. Nevertheless, despite being impressed by the 'form' of their response, we find ourselves unmoved by its 'substance'. In particular, we find no reason to abandon the main conclusions reached by Lobina (2012a), namely that Knot Theory (Knott) has nothing to say about the knot-tying abilities of humans, and perhaps even less about the general nature of human cognition.

Be that as it may, we feel a further response is in order, not least because the argument outlined in C&U and BEA1 has now become something of a moving target. To this end, we begin by reminding BEA2 of the chronology of the arguments they purport to defend, pointing up these arguments as they actually appeared in C&U and BEA1. Section 3 then presents a critical analysis of the specifics of the proposal put forth in BEA2 by way of response to Lobina's criti-

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1 BEA2 also discuss Lobina (2012b), a longer version that remains unpublished. We will only mention the former on a number of occasions here, befitting BEA2's own emphasis.

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cisms; a proposal we show to be entirely novel with respect to those originally presented in C&U and BEA1, and just as unsatisfactory. This demonstrated, section 4 switches tack and offers a broader diagnostic of the deeper structural problem that we take to mark our contending authors, arguing the conceptual framework of BEA2 to rest on an unfortunate and fundamental equivocation. By way of conclusion, section 5 offers some cautionary remarks regarding the moral of the BEA2 story for the biolinguistic enterprise, at least as it relates to evolutionary speculation.

2. To Recap

In Figure 7a of their paper, C&U (p. 47) presented the reader with a simple knot and its reputed transformation from a loose string. In this graphic, C&U assigned symbols to different segments of the knot — the "implied relations" within a knot (*ibid*.) — and suggested that these symbols can be manipulated by a production (string-rewriting) system, thereby reducing knot-tying to a succession of grammatical rules in the technical sense of formal language theory. In rather intuitive terms, C&U contended that one has "to 'hold' and 'skip' [the internal elements of a knot] to be able to lace the knot back into place" (*ibid*.), a process that, according to C&U, cannot be the "consequence of a Markovian process of sequencing adjacent elements" (*ibid*.). In order to formally ground this intuition, C&U drew a link between knot-tying and Knott, a sub-field of mathematical topology, relying on but a single reference to do so: an unpublished software manual meant solely for the studying of Knott (Mount 1985). In this booklet, Mount mused that the Unknotting problem — a particular Knot recognition problem of Knott — could perhaps be modelled as a context-sensitive formal grammar problem.[2] From this, C&U (p. 63) concluded that knots were not describable by a generative procedure of less than context-sensitive power, a conclusion they categorically stated to be beyond "rational debate" (*ibid*.; see below). BEA2 (p. 104) make much of the fact that Lobina (2012a) ridicules this statement, but given the evidence C&U adduced in its favour — the musings of a software manual about a Knot recognition problem that has nothing to say about how a human being actually transforms an actual string into an actual knot — it is hard to know how else a reasonable reader could be expected to respond.[3]

Note that we are here abstaining from judging the general validity of the statement per se; we are simply pointing out that the evidence that was actually provided in its favour was ludicrous. As we will show later on, BEA2 go to great lengths to justify C&U and admonish Lobina (2012a), but they seem oblivious to the fact that this is all bit *post facto*. Put simply, Lobina (2012a) had ample reason to query such a conclusion, there being nothing in C&U to support it.

BEA1, in turn, offered a very similar argument. Taking their heed from C&U, and attempting to find domains that might "presuppose a 'technical intelli-

2 In fact, C&U claimed that Mount (1985) had shown that "we need a context-sensitive system to *create* a knot" (p.47, our emphasis), a blatant misinterpretation of the reference they quote. We follow BEA2 in writing 'Knot' for mathematical knots.

3 Unsurprisingly, BEA2 abstain from mentioning (or indeed justifying) the employment of Mount (1985) by C&U.

gence' that could well be […] parasitic on the Faculty of Language" (BEA1: 11), the authors drew the reader's attention to the "complexity of *knot-tying*" (*ibid.*; our emphasis), claiming that when making a knot one must "relate a portion in the knot with the background 'figure'" (*ibid.*). This, they "intuitively" continued, involves an operation that implicates "grouping and long distance-like relations" (*ibid.*). Naturally, the mention of such relations is clearly intended to reference those features that make natural language mildly context-sensitive, and, as pointed out in Lobina (2012a), this is then connected to the computational complexity of '(un)tying knots', which "seems to require an underlying computational system of Type 1" (that is, a context-sensitive system; BEA1: 11). It is in this context that BEA1 referenced Hass *et al.* (1999) and claimed that the computational complexity involved in "determining whether any string is knotted is known to have a complexity comparable to the one needed to process linguistic expressions" (BEA1: 11).[4]

Note two things then: (a) Both C&U and BEA1 focused on knot-tying, that is, their arguments centred on how you go from a loose string to a knot; and (b) knots/Knots were claimed to be only describable by a context-sensitive system, even though no actual proof of this was provided; it was merely stated (and, considering the reference C&U used, simply imagined).

It was this overall argument, briefly recapped here, that Lobina (2012a) undertook to critique, and it was a recurrent point of that paper that Knott has nothing to say about how a string is converted into a knot/Knot, for the critical and substantive reason that Knott quite simply doesn't consider how a loose string becomes tied into a knot — which was without a doubt the focus of both C&U and BEA1.

Now, BEA2 (p. 98) seem quite agitated by Lobina's (2012a) suggestion that Knott takes 'tied knots' as a starting point, a statement they consider downright false. Admittedly, the formulation chosen in Lobina (2012a) is perhaps a little clumsy, but we point out that 'tied' is there used as a synonym of *bound*, and not of *knotted* (the antonym would be *loose*). In other words, this definition merely stated that Knott studies closed Knots, together with their relation to the Unknot — and *not* the conversion of strings into Knots/knots. It seems to us that such an interpretation ought to be obvious to anyone capable of a careful analytical reading of either the published Lobina (2012a) or the unpublished Lobina (2012b) given the emphasis therein placed on the irrelevance of Knott for real-life knot-tying; this would certainly have saved us from the irrelevant barrage of random quotes that BEA2 (pp. 98–99) grace us with.[5] Thus, the contradiction BEA2 see between employing the locution 'tied knots' and the Unknotting problem of

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4 However, BEA1 never actually offered any details or references regarding the relevant complexity needed to process linguistic expressions.

5 BEA2 engage in a lot of 'proof quoting' to make their points, but this is not always accompanied by proper interpretation of the material they cite. Consider, for example, BEA2's (pp. 95–96) insistence on the relevance of Knott for the study of real knots, contra an individual statement they select from Lobina (2012a), namely that "the knots that Knott studies have nothing to do with real knots" (p. 74). We, with Lobina (2012a), don't deny that Knots can be regarded as abstract, mathematical models of real knots; rather, the point is that the Knot recognition problem "has nothing to do with the computational complexity or expressive power of (un)tying a knot in *real* life" (Lobina 2012a: 76; our emphasis).

Knott as described in Lobina (2012a) is, for us, the result of misinterpretation, a failure to substantively engage with the issue under discussion.

Be that as it may, let us accommodate BEA2 and employ their definition of the Unknotting problem; the overall point made in Lobina (2012a) still follows. BEA2 describe the Unknotting problem — recall, a Knot recognition problem — in terms of an explicit question, namely: "Is this projection of a Knot a projection of the unknot [sic]?" (p. 99), a problem that involves "telling apart the unknot [sic] and any knot [sic] projection" (p. 100; see also the relevant figures in Lobina 2012a and BEA2). Clearly, on this or anyone else's definition, the Unknotting problem is not *prima facie* related to the conversion of a loose string into a knot/Knot in the sense in which either C&U or BEA1 seem to imply; moreover, neither set of original authors offered any plausible reasons to relate the computational complexity of the Unknotting problem with that of natural language processing. This, at least, was the main point that Lobina (2012a) tried to convey and which BEA2 seem to have entirely missed.

3. The "All Tied in Knots" Recognition Problem

So much, we hazard, for the original claims critiqued in Lobina (2012a, 2012b). As noted above, however, whilst BEA2 apparently set out to defend and justify the ways of C&U and BEA1 to man, they primarily do so by presenting a new means for relating the Unknotting problem to real-world human knot-tying. That is, whilst the argument presented in C&U and BEA1 is clearly centred on the possibility of modeling the conversion of a string into a knot as a specific sort of formal grammar problem, this is not quite the case within BEA2, where an entirely novel argument, based on their presumption that human artefacts are cognitively transparent with respect to the "cognitive resources" that produced them (p. 79), is poured forth as if it were smoothly related to that which went before. Specifically, they argue that, in order to tie a knot, you have to first visualise the particular knot you are going to create, and such a visualisation is an instance of visual object recognition, a process BEA2 regard as analogous to the Knot recognition problem of Knott. We turn to this argument now, but forewarn its ultimate failure to offer a coherent response to the original criticisms. Indeed, as best we can tell, the actual defence mounted by BEA2 seems to rest on two evidentially dubious steps, and a rather persistent confusion between the computational complexity of string recognition and the parsing complexity of language processing.

With this in mind, the initial step in their argument involves the claim that tying a knot requires a prior act of visualisation, 'knot production' instantiating "at least a particular case of the more general problem of object recognition" (BEA2: 97); a claim that follows from observations which BEA2 themselves note to be "admittedly scant" (*ibid.*). The authors adduce two such observations, in fact, opening with a reference to some empirical evidence which they claim indicates human beings to be particularly poor at learning to produce knots by simply observing motor sequences. BEA2 then connect this evidence to their own personal experience of knot-tying, suspecting that knowledge of complex knottings is taught "by resorting to mnemonic techniques" (*ibid.*) which involve the working out of the number and direction of the relevant crossings.

Unfortunately, neither set of observations seems to withstand much scrutiny. Take, first, their supposed experimental 'evidence', at least with respect to the specific citation from Michel & Harkins (1985; BEA2: 97). Having taken the liberty of following this study up, we are more than a little disconcerted to find it more than a little misrepresented. So, whilst it is true that only 37% of the subjects successfully learned all three knots, the figure BEA2 would have us focus on, a full 97% still managed to learn at least one knot (Michel & Harkins 1985: 598), the subjects doing so based on observations of a mere five demonstrations per knot, each such demonstration lasting a mere 15–20 seconds each (*ibid.*). To our mind, such unconducive learning conditions hardly constitute evidence against learning by observing motor sequences. Even more disconcertingly, it seems that when handedness was taken into account, *the actual object of the study*, a full 90% of subjects somehow managed to learn at least two of the knots (*ibid.*); that is, observational learning was greatly enhanced when teacher and student were either *both* left-handed *or* both right-handed. At the very least, this suggests that any of the evinced difficulties may actually have resulted from having to observe a teacher who utilised what was, for the differently-handed subjects, a non-natural set of hand co-ordinations.[6] Quite how BEA2 take this to be evidence for their proposal is beyond us. Indeed, according to the logic of their own argument, motor sequence internalisation supposedly goes hand-in-hand with successful observational learning; since, therefore, we would actually seem to have evidence of successful observational learning, what BEA2 actually offer up is evidence of successful motor sequence internalisation.[7] We are much obliged.

This leaves their citation of personal experience; a particularly strange sort of evidence to present and have taken at face value. Nevertheless, since this is the kind of evidence we are apparently allowed to present, we dutifully note that, though neither of us is, has been, or likely intends to be a boy scout, at least one of us used to sail in their youth, and distinctly recalls learning knots sometimes by copying the movements they observed others making, sometimes by being explicitly taught (usually in terms of confusingly metaphorical rabbits, trees, and holes), and sometimes by both methods. Such, indeed, is the value of scant observations. Indeed, even taken together, we doubt that these two sets of observations, at least as presented, can seriously lend *any* kind of substantive support to the claim that "knotting abilities have little (if anything) to do with the accurate internalization [sic] of a motor sequence" (BEA2: 97), or that "to make a

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6 And this is not even to point out that three different types of knot were demonstrated, with the 'magic' and 'butterfly' knots each being learned faster than the 'sheepshank' knots (Michel & Harkins 1985: 598–599), clearly allowing for the possibility that the final knot was of a completely different order of difficulty.

7 Just to be clear, the issue we point out here is entirely framed according to the apparent internal logic of BEA2 itself, based on the argument and evidence they present. It would clearly be injudicious of us to read overmuch into Michel & Harkins (1985) given our non-intimate familiarity with that specific line of research. Unlike BEA2, who seem able to make sweeping claims based on a modicum of evidence, we genuinely worry about issues of interpretation. So, for example, we wouldn't dream of using a couple of arbitrary papers to claim that a complex phenomenon such as dyslexia can be neatly reduced to a "visuo-constructive deficit" (BEA2: 85).

knot, one needs first to represent it and to represent it one needs to figure out its topology" (*ibid*.). As far as we are concerned, therefore, this part of the argument is little more than strained supposition; a sort of proof by speculation.

Having failed to establish the plausibility of knot recognition with respect to actual knot-tying, the next step in BEA2's argument involves their assertion that this assumed process of knot recognition — by which they mean the explicit mental recognition of a knot's formal properties — can be substantively related to the Knot recognition problem of Knott. In this case, BEA2 literally offer no evidence for connecting the two. Rather, BEA2 merely admit the possibility that "knot recognition in humans is a totally different thing from Knot recognition" (p. 102), and then proceed to claim the connection between the two to be a "hypothesis that is as informed as it could possibly be" (*ibid*.). Informed by what? We are not being purposely confrontational here, the authors really do offer zero grounds for connecting these two *prima facie* disparate phenomena.[8] Recall that, for BEA2 (p. 99), the Unknotting problem (a Knot recognition problem) is explicitly characterised as a yes-or-no answer to the question "Is this projection of a Knot a projection of the unknot [sic]?". How can *that* have anything to do with the visual recognition of a knot? Is the suggestion seriously to be, for example, that boy scouts check the success of their knot-tying activities by mentally visualising an Unknot and then determining whether their particular knot is, indeed, such a projection?

This leaves us with the final key issue in BEA2's novel argument, namely the supposed relation between parsing and recognition, together with their respective computational complexities. That is, having failed to plausibly argue for the involvement of knot recognition in knot-tying, and having failed to make any kind of case for a *prima facie* link between knot recognition and Knot recognition, BEA2 next attempt to dismiss Lobina's (2012a) claim that *parsing* a sentence is not quite the same thing as *recognising* a string of a formal language and, since the computational complexity measures we do have refer to the latter and not the former, there are quite simply no grounds for comparison between Knot recognition and language processing.[9] To this BEA2 (p. 90) respond with a some-what arbitrary quotation in which parsing and recognition are equated, enabling them to carry on without an apparent second thought for the actual nature of the matter under consideration. Unfortunately, BEA2 show a remarkable ignorance of basic issues in psycholinguistics.

Given the referential pyrotechnics of BEA2, we assume they would at least be moderately aware of Berwick & Weinberg (1989), for example, a publication that treats the relationship between parsing and recognition more carefully. As Berwick & Weinberg clearly state, sentence processing "involves associating a meaning with a phonological string", which "demands parsing, not just recognition" (p. 252, n. 13). That is, parsing means the recovering and assigning of structural descriptions to the linguistic input (p. 264, n. 55), and from this

8 At this point, BEA2 write that their hypothesis is not a possibility that "Lobina has been able to really call into question" (p. 102). That is quite right, but also rather insincere, considering that this is an entirely novel proposition, not present in either C&U or BEA1.

9 Recall, nevertheless, that neither C&U and BEA1 provided any evidence or references regarding the computational complexity of processing linguistic sentences.

Berwick & Weinberg conclude that parsing is harder, computationally speaking, than recognition. Whilst this is certainly true, we would nevertheless like to point out that parsing and recognition actually divorce in a much clearer and more principled way.

Pointedly, for instance, a central result of psycholinguistics has it that sentence processing proceeds incrementally, by which it is meant that partial meaning interpretations are computed during parsing (i.e. before the end of a clause). Consequently, the sentence processor carries out many valid parses that would have no string recognition equivalent; indeed, many of these parses relate to incomplete 'chunks' rather than full expressions per se. And there are many more interesting cases, each pointing to the clear divorce between parsing and formal language recognition: such as ungrammatical sentences which are nonetheless successfully parsed (or at least provided with some sort of interpretation), as in the missing verb effect (Frazier 1985); such as grammatical sentences that are nonetheless unparsable, as is the case with reduced relatives (Bever 1970); and such as the clear existence of grammatical illusions (Phillips *et al.* 2011).

In other words, though they have clearly read some of the formal grammar literature, BEA2 seem completely unable to distinguish the interests of psycholinguists from scholars working within the discipline of computational linguistics. Traditionally, the latter have been more interested in the computational complexity of language recognition, whilst the former have recently started to investigate the parsing complexity involved in the cognitive processing of linguistic utterances; a very different type of investigation, despite the superficial similarity that might otherwise be implied by their mutual use of the word 'complexity'. As such, BEA2 simply cannot rely on the 'complexity' of language recognition as a measure of the 'complexity' involved in the parsing of linguistic structures, and they are just hopelessly confused when they state:

> [I]t is the task of psycholinguists to incorporate these [computational complexity] results when building their performance models, that whatever memory limitations they postulate, whatever parsing strategies they propose, etc, should take into account the inherent structural/computational complexity of natural language. [BEA2: 94]

This is simply false: The memory limitations and strategies implicated in sentence processing are a matter of *sui generis* discovery, and they are clearly independent of the structural and computational complexity of formal languages.[10] Rather, the role of the psycholinguist is to find out how the performance systems *cope* with the linguistic input they receive, which is to say that psycholinguistics aims to discover what strategies and memory limitations human psychology exhibits when undertaking the business of actually processing a sentence. In this sense, the results of formal language theory have *prima facie* very little to do with such an investigation.

[10] BEA2 use the expression "structural complexity" for what Lobina (2012a) termed the "expressive power" of a grammar (that is, the set of strings a grammar can generate). We think the former formulation is somewhat misleading, but won't dwell on this point here. In what follows, we'll use both terms interchangeably.

This is a matter of principle, in fact, for the products of linguistic cognition are not solely the result of the underlying grammatical system, they are produced by a much larger complex; a conglomerate that likely includes a parser (as distinct from the grammar), working memory, and perhaps other systems. Consequently, there is little reason to believe that a proper analysis of linguistic productions will be able to rely solely on the results of formal language theory. On the contrary, if we are to take seriously the proposal that cognitive artefacts are transparent with respect to the "cognitive resources" that produced them, we must also include processing considerations in our models, these considerations distinct from (and perhaps bearing *at best* only an indirect relationship to) such theory. What is certainly not the case, despite BEA2's misconstrual, is that the expressive power of natural language must be built *into* performance models.[11]

This point was already clear as far back as Miller & Chomsky (1963) where the relationship between the grammar and the parser was explored, two linguistic constructs that need not be, and quite probably aren't, isomorphic. So, for example, among the many things Miller & Chomsky discussed was the working memory capacity involved in the processing of centre-embedded sentences.[12] After reviewing some experimental results, Miller & Chomsky concluded that subjects could successfully parse up to seven centre-embedded clauses, a limit they linked to the 'magical' number 7, in reference to Miller's (1956) now-classic study of working memory's capacity to recode information into manageable chunks. Note, then, that the memory capacity Miller & Chomsky assigned to the processor bears no direct relation to the expressive capacity of language. That this is so is demonstrated by the human ability to actually produce the centre-embedded construction itself; the capacity of human memory relating only to the *number* of central embeddings it can cope with. Thus, the clear implication is that the memory capacity of humans can only be determined by the measuring devices and experimental paradigms the psycholinguist has at their disposal.

A similar point applies with respect to parsing strategies. Naturally, of course, it is true that the grammar must be somehow related to the parser, for if this weren't the case, the parser would be unable to assign linguistic structure to the linguistic input; that is, the parser needs to have access to a grammatical 'knowledge' base if it is to be properly operative. However, what does not follow from this is that the parser implements the rules of the grammar in a direct and transparent manner; in fact the computations that the parser implements need not be isomorphic to those of the grammar at all (see Bever 1970 for a relevant discussion of this last point). We would, then, be talking about two very different types of 'computations': those underlying the sound–meaning pairs the language faculty generates in the technical sense employed within generative grammar, and those implicated in the operations of the parser during real-time processing. This is, again, clearly so in the case of nested structures: Whilst the grammar in principle allows for unbounded centre-embedded structures, the ability of the

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11 Indeed it is perfectly possible, at least in principle, that human language considered in terms of a specific knowledge base demonstrates any number of expressive properties that are never realised due to a mismatch with the capacity of the parser to implement these properties.

12 Naturally, the data Miller & Chomsky (1963) analysed are now somewhat dated; however, our interest here focuses on the underlying idea borne out by their paper.

parser to process structures like these is hampered by processing constraints. These constraints determine, to a certain extent, the character of the computations the parser effects.

Some of the processing constraints we have in mind here would at least include perceptual strategies, memory limitations, the design of mental architecture, the role of context, frequency effects, and so on. Now, whilst it is at present very uncertain how all these factors conspire into an overall model of language comprehension (not to mention production), we can nevertheless highlight one theoretical perspective in order to bring the general point home, that of Townsend & Bever (2001). Therein, the authors present an analysis-by-synthesis approach according to which the processor undertakes, first of all, a preliminary analysis of the signal by imposing a Noun–Verb–Noun (NVN) template onto the input. This stage is then followed by the application of the rules of grammar, in some fashion or other. What interests us here is the postulation of the NVN template, a *sui generis* perceptual strategy based on the high frequency of such configurations (at least in English). Critically, for our purposes, this specific parsing strategy is proposed in order to explain data produced by experimental studies of online language comprehension and, contra BEA2, has no direct basis in formal language theory.

Simply put, psycholinguists will do well, as they already do, to ignore the misguided advice BEA2 bestows upon them, focusing instead on the effectively *sui generis* complexity involved in the actual parsing of linguistic products (and quite independently of the abstract formal complexity these products might have). So, it is entirely in the spirit of Miller & Chomsky (1963), for instance, that the psycholinguistics literature has recently provided a number of studies which have sought to discern the *parsing* complexity involved in recovering and assigning the right structural description to the linguistic input. To name but two examples, Gibson (1998) calculated parsing complexity in terms of the number of new discourse referents that are introduced in a sentence, whilst Hawkins (2004) focused on the number of syntactic nodes required to handle a particular piece of syntax.[13] Of course, it is far too early to settle on a specific measure or to favour a given proposal, but if the computational complexity involved in processing language is to be related to that of another cognitive phenomenon, we believe that the focus should lie on the sort of approach that Gibson (1998) and Hawkins (2004) advocate, and not on formal language recognition as understood within the technical sense of the theory of computation.

In any case, the manner in which BEA2 treat the issue of formal language recognition has its own problems, and it may be worth at least mentioning them here. Thus, BEA2 take up ample space in showing that whilst the structural complexity of a formal language can only be assessed directly via "the devices that are capable of specifying" a language (p. 89), and then tell us that the computational complexity involved in recognising a formal language can be so determined; that is, independently of any formalism (pp. 92–94). Yet they hardly argue in favour of such a position, merely offering an off-hand reference to Ristad (1993; cited therein). We do not, of course, doubt the value of Ristad's

13 There are other possible variables, of course, such as number of words or simply time sequences.

results, at least for formal language theory, but it seems to us that they are of very little application to either linguistics or psycholinguistics. After all, to abstract away from the device that specifies a language is to focus on an infinite set of strings (the formal language), and the latter has no cognitive relevance whatsoever. We should perhaps remind BEA2 that the important concept in linguistics is the 'grammar' and not a formal language as a set of strings whose recognition is, presumably, between P and PSPACE (BEA2: 93). That is, since it is the grammar that is postulated to be mentally represented in the minds of speakers and hearers, the computational complexity of processing language ought to be closely related to this construct. As a matter of fact, Lobina (2012a) made the point of referencing a recent summary of formal language theory (viz. Pratt-Hartmann 2010), wherein it is stated that different grammatical formalisms are in different computational complexity classes. Further, Lobina (2012a) pointed out that none of the complexity measures he was aware of matched the computational complexity of the Unknotting problem, as determined in the single reference BEA1 had employed (namely, Hass *et al.* 1999). So why are BEA2 so sanguine about Ristad (1993)?

It seems to us that it has nothing to do with Ristad (1993) *per se*; it is merely based on the fact that Ristad's conclusions are simply convenient for BEA2, as Ristad determined, according to BEA2 (p. 94), that the computations underlying natural language are NP-complete, which is precisely the complexity class of the Unknotting problem BEA2 would have us focus on. Had BEA2 decided to follow Pratt-Hartmann (2010) and settle on a specific grammatical formalism — such as tree-adjoining grammar, for instance —, they would not have been able to connect the computational complexities of such disparate phenomena, a major point that we return to in our final section. In other words, if we postulate that it is the grammar and not the set of strings that is mentally represented (and the latter couldn't be because we quite simply can't represent infinite sets of strings), then the computational complexity of processing language *has* to relate to the mental reality of the grammar — and therefore its formalism.

Taking all of the above into account, therefore, the lesson for BEA2 would seem to be that the actual "cognitive resources" involved in the processing of a human artefact cannot be so easily discerned through a formal mathematical analysis of these artefacts. And there is a very simple reason for this: In order to determine the appropriate cognitive resources, what we actually need to postulate is a plausible cognitive model. Such a model, if we are right, will necessarily involve properties which go beyond an artefact's formal properties and which cannot be assumed to bear any kind of transparent relationship to these properties. This is a critical claim in the context of our reply to BEA2, and something to which we return in section 4. In order to provide some further support for this claim within the context of the present section, however, let us make a final point and close with an important caveat.

Regarding the former, take the case of the structural complexity underlying language. As is well known, Chomsky (1956, 1963) was able to show that the expressive power of language had to be at least context-free because a finite-state grammar couldn't account for unbounded nested structures. Critically, however, Chomsky did so by relying on *both* theoretical argumentation *and* grammatical judgements over this sort of sentences, and grammatical judgements are each a type of experimentation *and* performance data. Naturally, had Chomsky only had access to linguistic artefacts in isolation from any grammatical judgements, as in printed material or corpora transcripts, a finite-state system would have sufficed to account for the data, as corpora and the like do not even hint at the possibility of unbounded nested structures. *Mutatis mutandis*, we here claim, with respect to the analysis of the fossil record.

As for the caveat, imagine that the formal equivalence of knots/Knots and linguistic expressions had, in fact, been demonstrated. What should we take this to actually mean? If we were to specify the structural complexity of a language — that is, the set of strings a grammar can generate — we could, of course, employ either a grammar or an automaton for this purpose; but do we have an analogous analysis for knot-tying? The answer is a clear "no"; or at least it is not something that C&U, BEA1, or BEA2 provide. If, on the other hand, we were to determine the computational complexity of language recognition, we would be probing the rate of growth of space and memory resources as manifested in an automaton; but is there a similar study in the case of knot-*tying*? The answer is "not quite". That is, whilst we do have a Knot recognition problem for which a complexity measure can be calculated, BEA2 have nowhere demonstrated that knot recognition can plausibly construed as Knot recognition. Furthermore, we have here insisted that formal language recognition has very little to do with the actual processing of natural language. That is, even though a formal language may be recognised by an automaton, the expressions of a natural language must be parsed by humans, and the latter is crucially a very different matter. In this respect, and once more, what of knot-tying? Well, we have argued that no reasons have been provided to warrant the supposition that knot-tying is preceded by knot recognition. And, be *that* as it may, it seems to us that knot-tying is surely a case of knot *production*, meaning, surely, that the relevant line of comparison to pursue would more likely be language 'production', rather than language 'recognition'. Now, whilst we won't pursue the last speculation in this paper, our admonition here is simply that the computational properties of language and those of knots/Knots/knot-tying do not seem to match up at *any* level.

Relatedly, BEA2 are certainly proceeding too fast (and too loose). After all, the fact that two computational problems happen to be in the same complexity class does not mean that they are actually related; or that they share the same "cognitive resources". We can certainly recognise that C&U and BEA1 attempted to model knot-tying as a grammatical production system, even though no demonstration was in fact provided. We also recognise that it is one of the aims of BEA2 to show that the Knot recognition problem can be modelled as a grammatical problem, but it is telling that the closest they come to achieving this is by using one the references included in Lobina (2012a, 2012b), namely Turing (1954). In fact, Lobina (2012a) was not sceptical of the possibility of modelling the Unknotting problem as a formal grammar problem — that is why the reference to Turing (1954) was included in the first place. What's more, we are not even sceptical of the possibility of modelling, among other things, *knot-tying* as a grammar problem. Our point is twofold; firstly, the key notion is *knot-tying,* and it is this that is the phenomenon that needs to be modelled as a grammatical problem, but so far no such thing has been provided; and secondly, even if the latter were to be successfully undertaken, it is not the case that by describing two *prima facie* un-related problems with the same formal machinery (i.e. the tools of formal language theory), the common features of a postulated underlying computational system are *ipso facto* unearthed. Formal analysis, after all, is no substitute for substantive argumentation. (The latter is an important issue, to which we turn in the next section.)

As a final point, let us mention that towards the end of their paper (p. 104), BEA2 remind us of the precise claim which C&U considered to not be subject to rational debate, namely that "knots are *not describable* by any generative procedure that does not have enough operational memory to count as context-sensitive" (C&U: 63). BEA2 take this statement to mean, rather banally and with no insignificant liberty of interpretation, that the "inherent complexity" (BEA2: 104) of knots is not controversial, a complexity that can apparently be related to the structural and computational complexity of natural language; a torchlight for the biolinguistics enterprise, it seems to them (*ibid*.). This, according to BEA2, is the message C&U tried to convey, and it is their message too.

This would be all good and proper, if it wasn't for the blatant disingenuousness. To recap once again: C&U suggested, albeit incompetently, that a string can be converted into a knot by following a series of grammar rules — which, we suppose, is what is meant by a knot being 'describable' — but their final conclusion, cited supra, was based on a preposterous reference to an unpublished software manual in which it was mused that the Unknotting problem, and not the conversion of a string into a knot, could be so modelled. Clearly, C&U were not entitled to hold such a belief; *a fortiori*, they were not entitled to affirm that it was not subject to rational debate to discuss such a conclusion, the latter a ridiculous claim rightly ridiculed in both Lobina (2012a) and Lobina (2012b). BEA2's 'description' of a knot is instead based on visualising a knot first, a process they claim to be related to the Knot recognition problem of Knott. Note that such a 'description' of a knot is completely different from that of C&U; note further that it is also different from the conceptualisation of knot-tying advanced in BEA1 in terms of relating a knot to its background figure, which includes a number of the grouping and long-distance relations that arise thereby (in any case, a 'description' that BEA1 didn't justify either). Recognise, then, that BEA2's argument is very different indeed.[14] Clearly, too, BEA2 are not entitled to use their novel argument to justify either C&U or BEA1, as this can only be regarded as an entirely *post facto* justification. The overall result is nothing more than a gigantic increase in the volume of fog that no amount of 'new ordering' will fix. It is no wonder, then, that C&U, BEA1, and BEA2 have got themselves so tangled up; and no wonder that they are seemingly incapable of *recognising* their predicament.

All in all, then, BEA2 fail to make a reasonable case for relating the subject-matter of Knott with formal language theory, linguistics or psycholinguistics. In particular, Knott has nothing to say about how one produces a knot from a

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[14] In fact, given BEA2's insistence on the need to visualise a knot before tying it, and their dismissal of the role of motor sequencing in learning how to tie a knot, they are unsurprisingly pretty much indifferent to the actual action of tying a knot!

string. There is also no reason to believe that a knot is visually recognised or represented before tying a string into a knot; and even if this were the case, we have not been offered any grounds to relate such ability to the Knot recognition problem of Knott. Finally, the computational complexity of the Knot recognition problem is not directly relatable to the computational complexity implicated in linguistic processing; if the latter is to be defended, it needs to be substantively related, not analogically hinted at. In short, the charges levied at C&U and BEA1 by Lobina (2012a) remain basically untouched: Knott is quite simply misapplied and unrelatable to the study of the faculty of language. If we were to be charitable, we would simply point to the rather glaring errors that we have thus far sought to highlight, and leave it at that; but, in the end, it would seem that we are not that charitable after all.

4. A Natural Computational System by any Other Name

Thus far the present paper has essentially been addressing BEA2 in terms of their specific responses to Lobina (2012a, 2012b), arguing a consistent failure of these rejoinders to hit their respective marks. Approached at a more general level, however, we believe this consistency to be, if not willful, then, at the very least, no accident; rather, it is the inevitable outcome of the conceptual muddle that BEA2 seem to have gotten themselves into. It is to this muddle that we now turn, beginning with a question which may well have been bothering those readers inclined to have suffered us so far: why all the lather about knots?

The answer, perhaps unsurprisingly, is that the authors aren't really interested in knots *per se*. On the contrary, knots are considered to be a particularly instructive case-in-point regarding their core "thesis". This thesis we take to consist in the following set of interrelated claims:

> (A) The animal mind is at least partially constructed in terms of a "natural computational system (NCS)" (BEA2: 80). This NCS is a "core engine [...] subserving some (but not necessarily all) of the main cognitive functions" (*ibid*.), and "which may be modeled by an abstract machine or automaton in the sense of the mathematical theory of formal languages and automata" (*ibid*.). Each such NCS represents a "computational phenotype" (p. 83) that "one can associate to certain specific neuroanatomical configurations" (p. 80), and which is "functionally unspecific" (p. 84).
>
> (B) Artefacts, those objects which are the material products of animal minds, instantiate a direct relationship between their formal properties and the cognitive biology of the particular animal mind that happened to produce them; that is, "manufactured objects are transparent with respect to the biological structures underlying the processes necessary to produce them" (BEA2: 79).
>
> (C) Considered as artefacts, a formal analysis of knots and natural languages suggests them to be strikingly similar in terms of their computational properties (BEA2: 102–104).

(D) Given this closeness, it is plausible to suppose that human cognition does, in fact, represent the implementation of an NCS (BEA2: 94).

To the best of our apprehension, we believe the above to be a fair summary of the thesis underlying BEA2.[15] So summarised, the centrality of knots to the authors is immediately apparent. For, taken at face value, the supposed equivalence in computational complexity transforms the somewhat trivial observation that humans happen to produce both language and knots into substantive support for the authors' notion of a "natural computational system" (NCS).[16] That is, the very reason human beings are able to produce *both* knots *and* language is that we have evolved a particular cognitive setup that represents the biological implementation of an NCS of a specific computational phenotype. This is no doubt a novel thesis, one of which BEA2 seem suitably proud: Not only does it apparently open out a new perspective on the nature of cognition, it also opens up a way for researchers to productively mine the archaeological record, thereby bringing new evidence to bear on both the biolinguistics enterprise and the evolution of cognition. Much is, indeed, ado about knotting.

A central problem as we see it, however, is that the only rationale BEA2 actually seem to have for taking the supposed mathematical equivalency of knots and language seriously as substantive evidence for the cognitive equivalency of knots and language is their claim that the mathematical properties of artefacts are also their cognitive properties. Unfortunately, the only reason BEA2 seem to provide for taking *this* claim seriously is their own notion of an NCS, which they *define* outright to be something both cognitive and mathematical; something, that is, which simultaneously underlies "some (but not necessarily all) of the main *cognitive* functions of an animal mind" (BEA2: 80, our emphasis) and which is also a "*computational* phenotype" (BEA2: 83, our emphasis). In other words, BEA2 nowhere offer substantive reasons for linking the two domains; instead, the authors merely equate, and thereafter interpret all evidence accordingly.[17] As such, we believe the edifice of BEA2 to be founded, and foundered, on an enviable equivocation, one which has fooled its authors into thinking they can swap out theory of cognition talk for theory of computation talk as if they were one and the same, never stopping to think either what it might mean or how it might be for the two domains to be related in the very real world of the mind; perhaps not so difficult a thing to do when the world can be predefined to suit one's fancy. Put bluntly, we're afraid they've been jumping too fast. Let's proceed at a slower pace.

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15 This does, of course, assume such a summary to be genuinely possible, something of which we are not entirely convinced given the rather scattered remarks from which we have attempted to reconstruct the thesis and given that the actual thesis itself appears something of a tangled web in which premises also seem to serve as their own substantive conclusions; see, for example, BEA2 (pp. 79–111).

16 Hence, presumably, their bizarre, and somewhat random, statement that "the archaeological record [...] shows a strong correlation between the presence of language and knotting". (BEA2: 94).

17 Hence, no doubt, their bizarre, and somewhat random, statement that "the archaeological record [...] shows a strong correlation between the presence of language and knotting" (BEA2: 94).

4.1. *The Theory of Computation and Theories of Cognition*

Our initial reason for supposing equivocation to be at the heart of BEA2 is simply the sheer amount of space devoted, firstly, to recounting some essentials of the theory of computation, and, secondly, to discussing some of the computational properties of knots and language (pp. 89–104); as if this were somehow enough in itself to refute the concerns of Lobina (2012a, 2012b). Yet, so devoted, BEA2 entirely miss the thrust of the original criticisms, which never doubted the possibility of *formally* equating knots and natural language.[18] Rather, what was queried was the legitimacy of moving from the possibility of formal equivalence to the plausibility of substantive equivalence; that is, of meaningfully moving from the domain of the theory of computation to the domain of cognition.

To see that any such movement cannot consist in simple switches, swapping out talk at the theory of computation level for talk at the theory of cognition level, we need only note the *prima facie* distinct concerns of the two domains when they come to consider real-world objects, whether or not said objects happen to have actually been manufactured. Thus, the theory of cognition deals with the properties of entities as actually instantiated within, and realised by, animal minds; that is, its concern lies with the properties objects have as *cognitive* objects. The theory of computation, on the other hand, deals with the properties of entities considered in the mathematical abstract; that is, its fundamental concern lies with the properties of objects modelled as *formal* objects. As such, the theory of computation is clearly under no obligation to give any thought whatsoever either as to how these formal 'objects' might actually be, or as to how they might actually be related to each other, within the specific confines of an animal mind. In this sense, at least, the theory of computation is somewhat like statistical analysis: You give the chosen statistical technique a particular set of numbers, and it outputs a set of results without a thought for the actual interpretation of these numbers and whether they really measure what the researcher believes them to be measuring.

By way of illustration, let us take two objects chosen at random; knots and human language, say. Handily, both objects turn out to be humanly-produced, thereby presenting themselves as reasonable candidates for explanation within the theory of cognition. Accordingly, it should be possible to assign each certain properties which serve to ground them cognitively; call these, for complete want of imagination, *cog-properties*. Equally handily, both objects turn out to be formally analysable within the framework of the theory of computation (the caveats identified in section 3 notwithstanding). Accordingly, they can each be assigned various formal symbols that enable them to be treated computationally; call these, again for complete want of imagination, *comp-properties*. It so turns out that, assigned certain comp-properties, knots and human language evince a certain similarity with respect to these properties, at least according to BEA2 (pp.

[18] As already noted above, such a possibility was clearly acknowledged through the original referencing of Turing (1954), Turing therein showing that knots can, indeed, be modelled in terms of the theory of computation. In point of fact, we are really quite comfortable with the idea that *any* real world object can be modelled mathematically, though we do think it perhaps sensible to draw the line at Italian cuisine (Hildebrand & Kenedy 2010).

102–104). The question that can now be asked is what this computational equivalence might have to tell us about their cognitive equivalence? Very little, we suggest, and quite likely nothing, for the simple reason that to speak of knots and language computationally is first and foremost to speak of these objects as already having been couched in terms of the formal symbols that are the province of the theory of computation. As such, it is only at this level of description that any formal results are defined and at which any equivalence directly holds. Hence, there is no *necessary* reason to believe that this formal equivalence will hold when we come to consider knots and language within the domain of the theory of cognition, even though both may well be cognitively-derived objects, because there is no *necessary* reason to believe that the comp-properties theory of computation researchers might legitimately choose to assign knots and language bear any relation at all to their cog-properties; that is, exactly those properties which the human mind takes into account with respect to its competence for knots and language. *Prima facie*, being mind-external physical objects, knots and natural language utterances may have any number of properties in common, any number of which the human mind neither notices nor processes, and which are therefore irrelevant in terms of a theory of cognition.

Now, to be fair to BEA2, there are moments where the authors acknowledge this point. So, for instance, they state that "from the fact that Knots are perfectly good models for knots it does not follow that they are good models for *cognitive* representations of knots" (BEA2: 96). Yet, despite the occasional glimmer of recognition, the general thrust of BEA2 rather suggests this point to be one the authors all too easily forget. Indeed, as best we can tell, their core thesis apparently reduces to the claim that you can infer back *from* the computational equivalence of artefacts *to* the cognitive equivalence of these artefacts:

> In a series of papers we have been developing a proposal for a novel methodology to 'read' the archeological record […]. Our proposal is based on the idea that a formal analysis of the material remains left by our ancestors may prove useful in determining the kinds and amount of cognitive resources deployed to produce such objects, in other words, that manufactured objects are transparent with respect to the biological structures underlying the processes necessary to produce them. By performing such an analysis […] one is capable of inferring the computational complexity of the said cognitive tasks and to advance hypotheses concerning the architecture of the mind capable of performing them. (BEA2: 79–80)

According to the position we have been outlining in this section, however, this is precisely what researchers are not capable of inferring. After all, given that the relevant 'manufacturers' have long since passed, to do so would be to take the comp-properties of an artefact as strong evidence for their cog-properties *independently of the possibility of any direct cognitive analysis*. But, since the comp-properties of objects need bear no relation to their cog-properties, independently of any direct cognitive analysis there are scant grounds for considering the comp-property equivalence of objects to be much kind of evidence at all. As such, when it comes to the archaeological record, there is simply no transparency to be found at all; and to claim otherwise could only be to assume that one can switch back and forth between the two domains without supplying the specific reason needed to justify each specific switch. To equivocate, in other words.

Perhaps most emblematic of this equivocation, however, for us at least, is the euphoric note on which BEA2 end, the authors presenting an analysis whereby "Knot recognition is reduced to a language recognition problem as required by computational complexity theory" (BEA2: 103). Strikingly, it is following on from the apparent success of this analysis, that they state:

> On the whole, and considering the different kinds of data we have presented here, it seems likely that natural computational systems, knots, and language do not define such a bizarre love triangle after all as pretended by Lobina. (BEA2: 104)

Unfortunately, what seems to have escaped BEA2's attention is the fact that the string language analysis preceding this bullish conclusion is still entirely couched within the terms of formal language theory; that is, it deals only with 'Knots' and 'Language' rather than 'knots' and 'language'.[19] As such, it directly relates only to certain comp-properties of knots and language, having no necessary bearing at all on their cog-properties and, hence, their substantive relationship according to a theory of cognition. The latter still remains something that must always be independently demonstrated to hold at the *cognitive* level. Unless, of course, you happen to have some concept that allows you to arbitrarily inter-define the two domains within the same sentence; a natural computational system, say.

4.2. *The Problem of the Missing Link*

If the previous section's line of reasoning is in any way proceeding along the correct path, then, BEA2 would, in fact, seem to be operating under the mistaken belief that theory of computation equivalencies between artefacts are *prima facie* grounds for considering these artefacts to also be equivalent at the cognitive level, this the result of an unfortunate equivocation between the two domains that we take to be embodied in the authors' own notion of an NCS. As further support for our argument, however, we believe this equivocation can yet be highlighted another way. For, if comp-properties and cog-properties are in principle distinct,[20] it becomes criterial that proper consideration be given to the manner in which they can actually be substantively related. As such, it cannot be enough to show that certain artefacts have certain comp-properties in common and that they also have certain cog-properties in common. Rather, it must also be demonstrated that it is *because* of these cog-properties that the comp-property equivalencies hold.

We believe this to be an especially important point for BEA2 to grasp, at least if their thesis is to actually go through, since it is surely not the case that knots and natural language utterances have whatever cog-properties they do be-

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19 The capitalisation of 'Language' here is simply intended to reference language as defined in theory of computation terms, on the analogy of 'Knots' and 'knots'.

20 Something, recall, that BEA2 would themselves seem to believe: "Of course, from the fact that Knots are perfectly good models for knots it does not follow that they are good models for *cognitive* representations of knots" (BEA2: 96).

cause of the specific comp-properties they do, but that they have whatever comp-properties they do because of the specific cog-properties they do. After all, theory of computation accounts of cognitive phenomena are, ultimately, *models* of such phenomena and, hence, entirely dependent on the actually existing cog-properties for their own reality. Taking the particular case at hand, therefore, what this means is that even if knots and language do turn out to have some comp-properties in common, and even if the artefacts' comp-properties are genuinely relatable to the artefacts' cog-properties, the latter could nevertheless still be entirely distinct at the cognitive level, thereby rendering any computational equivalence completely irrelevant from the perspective of a substantive theory of cognition: They would simply be different things, only coincidentally equivalent at the theory of computation level.

Now, given that BEA2 (p. 80) apparently acknowledge the fact that theory of computation accounts are, indeed, mathematical *models*, this is intuitively something for which they ought to display serious concern. Yet, despite the token acknowledgement, we find little evidence of any genuine concern for this state-of-affairs in the actual paper itself. Hence, once again, the authors' apparent belief that researchers can uncover cognitive facts by a simple perusal of the archaeological record. Hence, also, the switching back and forth between cognitive findings and computational findings, without the authors ever really providing any direct arguments that would serve to justify these highly general switches.[21] And hence, in particular, their seeking to argue for the cognitive equivalence of knots and language on the basis of their supposed computational equivalence (BEA2: 102–104),[22] without actually providing any reason to believe that these equivalencies have the same cognitive base (except, of course, for that handily provided by their own notion of the NCS). Something of a topsy-turvy state-of-affairs, to be sure.

To more clearly demonstrate what we mean by this point, as well as how it specifically relates to BEA2, let us return to the case of language, here considered apart from knots, and taking for granted that the human linguistic competence has an expressive power that is mildly context-sensitive; a comp-property which BEA2 persistently return to (pp. 83, 84, 90, 93, 103). What can we conclude from this? Arguably, two things. The first is that, couched within the terms of the theory of computation, natural language syntax is mildly context-sensitive. The second is that, whatever particular grammatical frameworks linguists are seeking to develop, were we to formalise any of these frameworks and thereby take advantage of the extra precision such formalisms afford, these frameworks should plausibly manifest an expressive power that is mildly context-sensitive.

21 Indeed, near as we can tell, what BEA2 effectively present is a somewhat convoluted argument by analogy, the authors providing two entirely separate evidence bases, one relating solely to the cognitive level and the other solely to the computational level, it being left to the reader to magically join up what BEA2 only ever present as distinct dots.

22 Strictly speaking, BEA2 do not even do this, since they never really argue that knots and language are exactly equivalent, as they surely must if they are to give their notion of an NCS any kind of plausibility. Instead, they seem quite content with the mere belief that "Knots (and knots) are complex objects but no [sic] too complex, perhaps sitting in a region of complexity space similar or not too far away from that of language" (p. 103); which seems nothing more than a roundabout way of admitting knots and language to not, in fact, be equivalent.

The first thing, of course, is little more than a truism (eggs *is* eggs, after all). The second, on the other hand, is potentially quite a useful thing to know, since it enables linguists to further evaluate any proposed grammatical framework according to the comp-properties it evinces, thereby constraining the range of frameworks that can be taken as reasonable candidates for modelling the linguistic competence which human cognition instantiates. So, based on such an evaluatory approach, for example, it would seem to be the case that the comp-properties of Head-Driven Phrase Structure Grammar and Lexical Functional Grammar mark them out as apparently too powerful a framework for adequately describing natural language syntax, whereas those of Combinatory Categorial Grammar, Minimalism, or Tree Adjoining Grammar apparently mark them out as more likely 'just right' (see Stabler 2010, together with the references therein). Accordingly, linguists attempting to ascertain the nature of human grammatical competence might therefore wish to use this state-of-affairs as reason for focusing less on the former two frameworks, and more on the latter three (or, indeed, any other framework that can be shown to be mildly context-sensitive from a theory of computation perspective).[23]

On this account, then, there would indeed seem to be at least one sense in which the comp-properties that are assignable to natural languages can be held to substantively relate to some of the cog-properties that we take to be characteristic of human linguistic competence. This state of affairs, however, only holds in this case because it is of the nature of grammatical formalisms that they bridge comp-properties and cog-properties. That is, whilst the comp-properties follow from the formalism being a formalism, and therefore ripe for treatment in theory of computation terms, the cog-properties follow from the fact that each formalism constitutes a formalisation of a specific grammatical framework, these frameworks specifically motivated in order to account for the criterial features of human linguistic competence. As such, to formalise a particular grammatical framework is perforce to provide a formalisation of the cog-properties of human language and, thereby, to provide a substantive mechanism for linking some of the comp-properties of natural languages to their cog-properties.

Unfortunately for BEA2, however, it is genuinely hard to conceive of any means by which the two sets of properties can be meaningfully bridged other than via the formalisation of a particular grammatical framework. Thus, for example, it is presumably not the case that the mind literally instantiates some infinite store of utterances; rather, what it instantiates is some form of productive competence on the basis of which this particular set of utterances can be generated. Similarly, whilst this competence can plausibly be characterised as generating some infinite set of expressions, what it actually, or "strongly", generates is not some infinite set of symbol *strings*, so much as a set of structured representations over which the appropriate string set can be abstracted; a set of structured representations, furthermore, which BEA2 (p. 89) themselves admit to be beyond the scope of the theory of computation. In other words, viewed from

23 Note, of course, that this is still very much only a might, there really being no *requirement* that linguists must take such properties into account, comp-properties being only one of a number of factors that might make a particular grammatical framework attractive from a linguistic point of view.

the perspective of the theory of cognition, the cognitively-relevant comp-properties of human language would seem to be entirely *derivative*. That is, they have no direct cognitive reality in-and-of-themselves, being essentially by-products of the underlying competence, it being this competence which *is* cognitively real and which is actually responsible for the cog-properties that ground the comp-properties of human language. Accordingly, if we are to genuinely establish the kind of NCS link so critical for BEA2, this can really only be accomplished by first establishing a direct account of the aforementioned competence. And since such an account cannot apparently be provided by the theory of computation (something which, just to reiterate, BEA2 (p. 89) themselves seemingly admit to be the case), our only recourse would seem to be the grammatical frameworks that are the focus of professional linguists because it is these frameworks which are expressly developed in order to directly account for the human grammatical competence and these frameworks which are capable of providing the kind of structural descriptions needed to properly model said competence.

Assuming the above-argued state of affairs to be in any way accurate, therefore, it would seem to be just these grammatical frameworks which constitute the requisite locus of description necessary for substantively bridging the comp- and cog-properties which are instantiated by the human competence with respect to natural language; call this level of description, for ever-persistent want of imagination, the *grammatical* level. As such, it seems clear that there is no real sense in which BEA2 can plausibly seek to legitimate the cog-property equivalence of knots and language on the basis of theory of computation results, even if such results may actually turn out to have a genuine cognitive basis. Rather, for the authors to actually make good on their claim regarding the NCS equivalence of knots and language, what they must be doing is demonstrating that this claim holds at the all important grammatical level. Unfortunately, this sort of argument is quite clearly a very different one from that which BEA2 seem interested in providing.

Indeed, to see this, one need only consider some of the grammatical frameworks on which various linguists are currently working; those of Culicover & Jackendoff (2005), Sag *et al.* (2003), and Steedman (2000), to name but three. For what even a moment's such consideration amply demonstrates is that these frameworks are directly motivated by the need to account for such criterial and highly specific features of natural language syntax as agreement, binding, constituency, dependencies, displacement, grammatical functions, scope, the selectional properties of lexical items, etc; not to mention the rather obvious requirement that linguistic expressions must somehow be such as to be both semantically interpretable and phonologically expressible. Now, since it is these properties which the various frameworks are expressly designed to capture, on our account it can only be these properties which result in the cognitively-relevant comp-properties of human linguistic competence. Hence, in order to even begin arguing for the existence of any kind of substantive NCS link between natural language and another human artefact such as knots, what BEA2 would actually have to argue is either that knots demonstrate similar criterial features to those of natural language syntax or that any of the highly specific (and, preferably, mildly context-sensitive) grammatical frameworks which linguists posit in order to account for these linguistic features can also be plausibly thought appropriate for handling the criterial features of the human competence with respect to knots (whatever these criterial features might actually turn out to be). So, taking the case of Combinatory Categorial Grammar as a particular case in point, BEA2 would have to show either that it makes sense to model knot-competence using the combinatorial framework postulated by, for example, Steedman (2000), or that the linguistic features which motivate this account have any directly equivalence with respect to those features which serve to cognitively ground said knot-competence.

Perhaps needless to say, we remain rather sceptical about even the principled possibility of such a demonstration. After all, it seems hard to conceive of any meaningful way in which human knot-competence could legitimate a treatment in terms of bluebirds, starlings, and thrushes. Regardless of the outright difficulty of such an approach, however, what is important for our purposes is simply that BEA2 nowhere attempt to mount any kind of argument at what we have called the "grammatical" level. Rather, the only comp-property accounts of knots and natural language they do provide are precisely those framed in either time/space terms (pp. 91–101) or language recognition terms (pp. 102–104), pure theory of computation accounts which make no reference at all to the "grammatical" relationships which might exist between knots and language and which would provide a genuine bridge between the cognitive and the computational. In fact, BEA2 are quite open about their beliefs here, explicitly following a claim about the complexity of natural language, for example, with the statement that "this inference is entirely independent from any consideration concerning parsing, *choice of grammatical formalism*, or any other architectural or formal consideration about performance models" (p. 93, our emphasis). What they seem to be arguing, in other words, is that you don't need any kind of grammatical framework to explore the computational properties of natural language.

Viewed purely from the perspective of the theory of computation, of course, this is quite possibly right (though even here this is perhaps still a rather limiting position to take). Viewed from the perspective of the theory of cognition, at least as we see it, however, this statement is highly misleading, and emblematic of the equivocation we take to underlie BEA2. For what the statement ignores is the critical fact that, to demonstrate any kind of substantive link between the computational and cognitive properties of human artefacts, this demonstration can only be unpacked at the grammatical level. For it is grammatical frameworks that serve to directly model human linguistic competence, and ultimately these frameworks which serve to ground any cognitively-relevant computational properties that human artefacts might have. Otherwise, all you are left with are some cog-properties and some comp-properties of one type of artefact and some cog-properties and some comp-properties of another type of artefact, with no means for substantively relating these properties. Which is perhaps just another way of noting that all BEA2 actually offer are hypothesised comp-property relationships which, as presented, are purely coincidental when approached in terms of the substantive reality of human cognition. Unless, of course, you happen to have some notion that enables you to arbitrarily inter-define two domains within a single concept, allowing you to move from the computational to the cognitive as if it were the former that grounded the latter; a natural computational system, say.

4.3. As Sure as Eggs is X

And were a second strain of reasoning not enough, BEA2 further oblige us to consider a third means by which the equivocation at work in the paper can be highlighted. Thus, it is a central point of their paper that computational systems can be distinguished in terms of their 'computational power', the latter understood as the memory resources "a computational device has at its disposal" (BEA: 82). In fact, their NCS is described as being composed of a "very conservative core engine" (*ibid.*), of which we are told very little indeed, and the all-important working memory device (*ibid.*). As such, according to BEA2, structural differences among computational systems follow "from the constraints on the working memory space the device has as its disposal to perform the computation" (p. 81). This is true enough for the means BEA2 have chosen to model their computational system — namely, those automata that formal language theory studies, and which constitute instances of a Turing Machine — but it is striking that the structural differences they seem so intent on outlining have so little to do with the underlying mechanism (or any of the operations) with which a computational system is usually identified. One could, after all, draw a distinction between, say, Kleene's (1943) partial recursive functions and untyped versions of the lambda calculus, or between any of the latter two and a Turing Machine, or between any of the latter three and Post's (1943) production systems; and doing so is to focus on the *intensional* differences among these systems.

BEA2 are sure to remonstrate that all these systems are extensionally equivalent — that is, they can all generate the same input-output pairs — and so their internal differences are not that important. That is a fact about their comp-properties, but as stated earlier, it is not the infinite set of input-output pairs that should preoccupy the cognitive scientist, but the intensional properties of a computational system — its cog-properties. After all, to determine that this or that automaton can recognise this or that formal language is to *specify* this or that formal language, and this can just as well be done with a string-rewriting system, thereby downplaying (actually, eliminating) any role memory resources may have.

So why are BEA2 so keen on the memory resources a computational system has access to? This is in fact hard to determine, but Lobina (2012b) did point out that both C&U and BEA1 made reference to Uriagereka (2008: Chap. 7), wherein it was defended that the Chomsky Hierarchy, qua a ranking of production systems, had so far only modelled the weak generativity (string generation) of grammars. It was further supposed by that author that 're-interpreting' the Hierarchy in terms of automata provided an account of strong generativity (structure generation), the only construct of some relevance for linguists. The connection between automata (including their memory resources) and strong generativity is of course a false one, something which Uriagereka (2008) seems to not have fully grasped given his apparent confusion of the memory resources of an automaton with psychological models of memory (and, by extension, with the structural properties of the representations so manipulated). Nevertheless, though BEA2 (p. 89) explicitly state themselves to be fully aware of the falsity of such connection, is it really the case that they are themselves so free of Uriagereka's (2008) equivocation?

BEA2 (p. 83) assure us that their NCS is neither a psychological model of memory nor a performance model; rather, it is an abstract characterisation of a model of computation, a formulation they consider to be similar to that of the faculty of language in the narrow sense of Hauser *et al*. (2002; cited therein).[24] However, when it comes to listing the evidence for their NCS — and this evidence is of two kinds, either clinical (viz. to do with cognitive disorders) or neurological, all to be found in pp. 85–86 — BEA2 seem oblivious to the fact that the data they provide are the result, as we have stressed above, of cognitive resources that include both the underlying computational system *and* whatever systems partake in performance, including, naturally, real-time memory.

The equivocation between formal and psychological models of memory is clearest when BEA2 consider the neural substrate of their NCS, as they reference the respective models of Lieberman & Ullman (pp. 85–86; cited therein), both of which quite explicitly outline a psychological, rather than a formal, model of memory. According to BEA2, Lieberman proposes a sequencer (perhaps the conservative core engine BEA2 advertise?) and a *working* memory in order to account for our ability to *process* symbolic elements (p. 85), while Ullman hypothesises about the location of *procedural* memory (p. 86). Apparently, both accounts are "ultimately conceived as to subserve the *learning* and *execution* of diverse tasks" (p. 86, our emphasis).

Can BEA2, therefore, really believe that their 'abstract' working memory construct is analogous to the working or procedural memory hypothesised in most of cognitive psychology? Are they really unaware that the cognitive data they selectively reference are informative of cognitive resources that must include, surely, a psychological model of memory in addition to whatever computational system underlies whatever cognitive skill? That by employing automata theory one is merely specifying formal languages? That any supposed distinction among computational systems in terms of memory access is no more than a result of the chosen formalism, and therefore not a genuine distinction? That, in any case, automata can only model weak generative power and therefore are irrelevant for the study of cognition? If BEA2 are indeed privy to all this, how is it they have managed to cleave so persistently to their cognitive tale? Is it because they have found themselves able to postulate the state of affairs to be thus? In terms of an NCS, say?

4.4. *Coda*

Equivocation, then; and persistently so. According to our analysis, therefore, it is

24 BEA2 also draw a connection between their NCS and whatever computational system Fitch & Hauser (2004; also cited therein) were in fact studying. We won't engage this issue here, but it seems to us that these similarities are more than a little exaggerated.

ultimately to this equivocation which can be traced BEA2's thoroughgoing (and otherwise perplexing) failure to engage with the nub of Lobina's (2012a) original criticisms. Indeed, taking the accuracy of our analysis at face value, this failure would hardly seem surprising, the very possibility of genuine and substantive engagement essentially ruled out *ab initio*, the entire edifice of BEA2 resting on a flawed conceptual foundation which has enabled its authors to arbitrarily confound distinct conceptual domains and present a grab-bag of disparate information as if it constituted a substantive evidence base. As such, BEA2 (pp. 84, 89) would seem critically mistaken when they assert their thesis to be one that is empirically testable. On the contrary, there is really nothing there with the kind of conceptual coherence necessary to even begin getting a proper purchase on the world.

Having paused for summary and breath, we find ourselves in something of a quandary, feeling strongly that there is yet more to be said by way of response to BEA2. Thus, we could take further issue with the specific pieces of evidence provided by our contending authors. So, for example, we could discuss the fact, pointed out to us by Mark Steedman (p.c.), that their example 3 (p. 103) is actually in the linear context-free rewriting class rather than in the triple copy class, and that there is scant reason to believe it bears any relation to their example 4 (p. 103). Or we could discuss the Herzfeld & Lestel (2005) study that BEA2 reference (p. 97); a study which, contrary to BEA2's interpretation, actually offers up evidence of knot-tying in apes, raising the inevitable question as to why, if apes *can* knot and *if* knotting has the expressive power of natural languages, apes have not so far been found to have a capacity for natural language syntax. Or we could discuss the apparent contradiction in using knot-learning evidence by way of support for their thesis (p. 97), when they themselves attempt to immunise this thesis from criticism by explicitly stating that it involves no "focusing on learning capabilities" (p. 83).

Thus, we could take issue with BEA2's (p. 88) mentioning of a "thesis" that was supposedly attributed to them by Lobina (2012b), namely that there is a "processing competence" which composes meaningful expressions, and which is additionally connected to "rich, contentful, language-like thoughts" (p.88). This is a thesis which they are apparently able to doubt on account of some supposed problems with adaptationist explanations of the theory of evolution. So, we could note that we are not sure what the term "processing competence" is supposed to refer to, but that we are certainly sure that Lobina (2012b) didn't ascribe any such thesis to them (unsurprisingly, they don't offer a page reference). We could also note that, in the greater scheme of things (there is life beyond C&U and BEA1, after all), Lobina (2012b) was seeking to discuss the relationship between language and thought, and it was therein argued that a rich conceptual representational system must be postulated if the acquisition of language is to be at all possible, a belief the present authors actually hold themselves (pp. 87–88). And we could note that however this might pan out for a theory of evolution is something we neither know nor care much about, but that we are definitely amused that BEA2 feel able to reference Fodor & Piattelli-Palmarini (2010; cited therein) as an authoritative critique of adaptationism.

Thus, we could also take time to more fully discuss the further implications of our reasoning for BEA2's claims regarding the particular NCS that human cognition supposedly instantiates. So, for instance, we could point out that if, as we have essentially argued in section 4.2, language has whatever expressive power (or comp-properties) it does because of the syntactic structures (or cog-properties) it does, then these (let us assume) mildly context-sensitive properties are ultimately the result of the human linguistic competence taken *as a whole*, a competence which is as much an artefact of the linguistic representations that are operated on as it is of the underlying system that does the operating. Hence, it really makes little sense to speak of the human NCS, in the sense of a "core engine" (BEA2: 80), as being itself mildly context-sensitive (p. 83), since it is not this engine, considered in isolation, which gives language its overall expressive power.[25] As such, should human cognition genuinely turn out to instantiate some domain-general computational system, which is presumably what BEA2 mean by their "functionally unspecific" device (BEA2: 84), then it is more than likely that this computational system will not behave uniformly with respect to the various cognitive domains over which it operates. After all, these domains will essentially comprise different systems and data sets, which will in turn require the computational system to carry out distinct computations according to each particular domain's own particular requirements, computations which should thereby result in distinct types of cognitive artefact, each with their particular expressive power. As such, it is perfectly conceivable that the (let us assume) mildly context-sensitive expressive power of human language will be of no import whatsoever for the study of other cognitive domains. Indeed, in such a situation, it would be quite literally meaningless to speak of human cognition as instantiating a natural computational system characterised by a particular expressive power, because such a system could not be meaningfully said to have any such particular power of its own.

And so on, and so forth. Suffice it to say, there is a lot more that we would like to have said.[26] Nevertheless, being aware that a proper treatment of these points and issues would require more space than is likely reasonable in terms of the present paper, and being unwilling to try the patience of the reader any further, such a treatment is no doubt best left for a more appropriate context; the addendum to a certain forthcoming book, perhaps…

5. An Old-World Apology, or Thereby Hangs a Tale

Almost all being said and done, there remains one final point that we feel does need addressing; namely the *ad hominem*, levelled against at least one of us, of "formal bullying" (BEA2: 104).[27] This is a strong claim, one that at least both of us

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25 To put this another way, it is language, not some domain-general NCS, that gives language the particular expressive power it seems to have.

26 So, for example, we haven't even bothered to mention BEA2's (p. 98) below-the-belt charge of behaviourism, a mischaracterisation that again only serves to highlight the general inadequacy of their own conceptual framework (which apparently misequates 'trial-and-error' learning with 'stimulus-response' learning, something which is likely the result of their inability to distinguish between claims regarding mental architecture and claims regarding performance).

27 An accusation we take to have been levelled with a certain amount of irony, originating in a

feels to be essentially unwarranted; for the simple reason that it is, essentially, unwarranted.

True, the spirited form of Lobina (2012a, 2012b) may not have suited all tastes; but, read thoughtfully, there really is nothing in the actual substance of these papers tantamount to "bullying". In particular, neither Lobina constituted some churlish refusal to engage with the matter at hand. Rather, sustained criticism was put forth in an attempt to substantively address the specific points and general claims of C&U and BEA1. Surely, to so criticise is not to bully. Nor, surely, is it bullying to point out lack of understanding if either (a) in general, there does indeed seem to be such lack of understanding, or (b) as specifically written, C&U and BEA1 can reasonably be argued to demonstrate such lack of understanding. This is what Lobina (2012a, 2012b) undertook to argue, and nothing in BEA2 suggests either that the original criticisms were ill-founded or that the situation has been substantively improved; at least not to us. Hence, the present paper.

And, just to be absolutely clear, no opposition to the *principle* of the endeavours represented by BEA2 *et al*. is to be found anywhere herein.[28] Indeed, in this sense, we are entirely in agreement with the spirit of the authors' attempts to advance new methodologies, methodologies that would enable fresh evidence to be uncovered and productively pursued; such undertakings are commendable. We simply disagree with the substance of their specific proposal, in its present form, and fail to see how querying this proposal is in anyway unproductive. Unless, of course, there is some sense to be had in cleaving to something that cannot claim to do what it sets out to do.

Here it is worth pointing out the assumption on the part of BEA2, and it is purely an *assumption*, that their framework advances our understanding; or at least has the potential to advance our understanding (p. 104). What Lobina (2012a, 2012b) was at moderate pains to point out, however, and what we have sought to reargue here, is that it is far from clear that the proposed framework actually does advance our understanding, being apparently based on false analogies, tenuous evidence, and dubious interpretation. Most fundamentally, there is clearly no virtue, in-and-of-itself, to a priori define one distinct conceptual domain in terms of another, whether or not said definition strikes some mysterious chord, and whether or not a particular set of authors are able to dress up their argument in superficially impressive formalities. After all, the history of linguistics is littered with such dead ends, false prophesies that muddy more than clarify and entangle more than merge. These prophecies are hardly surprising, there being no doubt an immense aesthetic appeal to find that language originated in the croaking of frogs or the thunder of Jove (Brisset 2001, Vico 1744/1948); and, well, you know, it sort of kind of looks like it does, you know, assuming, of course, that you are able to look at it in the 'right' way. Unfortunately, what stands to 'right' reason does not always stand to reality.

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paper composed by five established academics for the specific purpose of critiquing not more than one of us. After all, for someone to be able to bully, one would have thought that they would first need to be in some actual position of power…

28 Nor, of course, is there to be found anywhere herein any general opposition to, or dismissal of, the theory of computation as taken on its own terms.

And so it was, for example, that the statutes of the Linguistic Society of Paris included the well-known 1866 moratorium on evolutionary talk, explicitly recognising the highly speculative nature of that particular enterprise as it stood at the time.

If there is one underlying motivation with respect to the present paper and its two forebears, therefore, it is perhaps that the 1866 moratorium was issued with good sense, and that certain linguistic work with a biolinguistic/evolutionary flavour ought to take the spirit of that moratorium very much to heart, explicitly recognising the highly speculative nature of the enterprise as it currently stands. Not that we wish to dismiss outright any such work or demand the literal issuing of any such moratorium. After all, the familiar history of early twentieth century research into language and cognition demonstrates the pitfalls that easily arise through such *a priori* diktats. Rather, we make the simple point, easily forgotten in all the speculative excitement, that if linguists are to genuinely establish and cash out an apt biological/evolutionary framework for understanding human language, it will be important to proceed both thoughtfully and *critically*; not least because it is not especially clear what or where the relevant evidence base will turn out to be, or even what such a framework might itself actually mean given the highly interdisciplinary requirements of the task. As such, we take the three critiqued papers to represent a telling cautionary tale. For, if it is true, as has been remarked, that "[t]here is no end to plausible storytelling" (Lewontin 1998: 129), then surely it is even more true that we first have a plausible story to tell.

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