

Reply to Rao et al. and Lee et al.

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1. Introduction

In the last issue, I presented a piece that called into question some of the techniques reported in two papers in high-profile journals that purported to provide statistical evidence for the linguistic status of some ancient symbol systems (Sproat 2010a).

Not surprisingly the authors of those two papers took issue with a number of my claims, and have requested the opportunity to respond. The two responses, taken together, are rather lengthy and as a result it is not possible, given limitations of space, for me to address each and every one of their criticisms. I will therefore focus on what I consider to be the most important objections.

2. Rao et al.'s response

The essential claim of Rao and his colleagues (henceforth "Rao") is that I have misrepresented the claims of (Rao et al. 2009), and painted an incomplete picture of their work. They also take the opportunity to discuss at some length a wide range of evidence that they feel supports the Indus script hypothesis.

I want to make two things clear at the outset. First, my critique was about the paper that appeared in *Science* in 2009. While I mentioned some of Rao's later work, it was not a critique of that later work. It seemed reasonable to assume that the paper in *Science* was intended to be taken seriously and to stand on its own merits. If so, then I felt – and still feel – that the paper had serious problems. A problematic paper that cannot stand on its own does not become unproblematic even if subsequent research were to show the conclusions to be correct. To give a stark example, if someone should eventually demonstrate rigorously that cottontop tamarins *are* capable of learning "regular" grammars, that would have no bearing on the questions currently surrounding Marc Hauser's 2002 publication in *Cognition* (Johnson 2010).

Second, it was not my purpose in my piece to use the pages of *Computational Linguistics* as a forum for promoting the non-script theory for the Indus. Indeed I spent exactly one short paragraph, where I discussed our work in (Farmer, Sproat, and Witzel 2004) as a way of setting the stage for the current discussion. In contrast, Rao spends a significant portion of his response discussing the Indus symbols, the case for the script theory, the case against the non-script theory, and repeatedly refers to our 2004 paper and claimed problems with our arguments. The desire to keep my discussion of the background short was what prompted me not to expand on my claim that our

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arguments had “been accepted by many archaeologists and linguists”. Rao notes that I do not actually cite “who these ‘many archaeologists and linguists’ are”. Perhaps they do not exist? But they do: Andrew Lawler, a science reporter who in 2004 interviewed a large number of people on both sides of the debate notes that “many others are convinced that Farmer, Witzel, and Sproat have found a way to move away from sterile discussions of decipherment, and they find few flaws in their arguments” (Lawler 2004, page 2029), and quotes the Sanskrit scholar George Thompson and University of Pennsylvania Professor Emeritus of Indian studies Frank Southworth.

2.1 Misunderstandings about Non-Linguistic Symbols

A large part of the discussion in Rao’s response centers, as it should, on the question of the “Type 1” (random) and “Type 2” (rigidly ordered) models, which I argued did not accurately characterize any non-linguistic symbol systems. At the core of this debate are the Old European sign system of the Vinča, which is claimed to be a good instance of Type 1, and the Mesopotamian deity symbols found on *kudurru* stones, which are claimed to be good instances of Type 2.

To support the claims for Vinča, Rao provides a quote from (Winn 1990), which notes that the signs do not seem to have any ordering, and are “characteristically disarranged”. But if one sees the broader context of this quote, it becomes clear that Winn is talking here about a subset of the corpus, which is found on small vessels and spindle whorls. He contrasts the inscriptions discussed in the quote given by Rao with “the more arranged format of tablets and seals” (page 270), and then again (page 276) where he notes that “[d]ifferences in complexity of sign usage is denoted by sign ordering, which occurs on tablets and other objects, such as the plaque from Gradešnica.” On page 263 he refers to the “discovery of tablets with script-like content at Tartaria in 1961”, a clear indicator that the Vinča tablets would seem to involve some form of sign ordering. It is clear then, that Winn is *not* saying that the system is in general unordered, only that a subset of the corpus is. Winn also suggests that the small vessels in particular probably had a ritualistic function (page 276). Furthermore, the text that was ellipited in Rao’s quote, suggests that order was probably unimportant on the spindle whorls “since concepts or mnemonic aids could be inferred or interpreted by an individual cognizant of the culture-specific content of the signs” (page 269), suggesting a situation where the Vinča “reader” could interpret the jumbled signs since they knew the structure of the system. It is unfortunate that most of Winn’s interesting work has been published in obscure locations, and is therefore not something the average reader would run across. Fortunately though, Winn maintains a web page at <http://www.prehistory.it/ftp/winn.htm>, where one can see what the sign texts look like and where Winn also notes (page 8) that “the distinction between signs on pottery and the more organized signs on tablets and other artifacts may signify functional differences or different levels of usage and formality.”

Winn’s description does not support the claim that Vinča sign texts were generally without structure. But suppose, counterfactually, it did. Would Rao et al.’s “Type 1” be a good model of the system? Their Type 1 data is modeled by a random process that generates texts “based on the assumption that each sign has an equal probability of following any other” (Rao et al. 2009, Supplement, page 2), or in other words a system that is random and equiprobable. But where does Winn say that the system is *equiprobable*? The most Rao’s excerpted quote could be taken to mean is that the system is random, but Winn certainly does not say that the signs occur with equal frequency. A much more plausible model of a random ancient sign system would be one where the symbols have

a non-equiprobable distribution: as I argued in my article “symbols represent things, and the things they represent typically do not occur with equal probability”. Further, we know that many things, from non-linguistic systems such as *kudurrus* (see below) to populations of sets of species, have a Zipfian distribution, so a reasonable model of such a system might well be one in which the unigram probability follows a Zipf-Mandelbrot distribution. But to have presented Type 1 in this way would have been problematic: as I discussed in my piece, such random texts easily “fool” the bigram conditional entropy measure that Rao et al. proposed in their *Science* paper. Rao objects to the statement in my piece that at least some of the discussion surrounding their paper depends on the confusion between “random” and “random and equiprobable”, deeming that statement “unwarranted and not worthy of comment”. Yet their model of Vinča as a random equiprobable system depends precisely on this confusion.

At the other extreme are the *kudurru* texts, which are claimed to be good examples of Type 2, a rigidly ordered system. In support, Rao quotes (Slanski 2003), who says “to a certain extent, the divine symbols were deployed upon the Entitlement *narūs* (*kudurrus*) according to the deities’ relative positions in the pantheon. The symbols for the higher gods of the pantheon . . . are generally found upon or toward the top and most prominent part of the monument.” Crucial phrases here are “to a certain extent” and “the symbols for the higher gods . . . are generally found”. No doubt these are partly true; indeed it is very often true that the “sun disk”, “crescent moon” and “star” symbols occur at the beginnings of texts — though ordering within those three varies. But the *kudurru* texts are still far from rigid, as we showed in (Farmer, Sproat, and Witzel 2004) and as my own sample from Seidl shows. Over and above this, the quoted excerpts would only seem to support a hierarchy, not a model “based on the assumption that each sign has a unique successor sign” or even one where based on “variations of this theme where each sign could be followed by, for example, 2 or 3 other signs” (Rao et al. 2009, Supplement, page 3). All the hierarchy implies is that a symbol *Y* that is lower on the hierarchy should come after a symbol *X* that is higher on the hierarchy. But for any given *X* there may be several symbols lower on the hierarchy, which means that *X* could be followed directly by any of these, assuming that no concepts from the intervening elements are “mentioned” in the “text”.

It is puzzling to me that, given these considerations and my own description of my close examination of the *kudurru* corpus, Rao still suggests that “we expect the entropy of the *kudurru* sequences to be lower than linguistic systems and perhaps slightly above the minimum entropy (Min Ent) range in Figure 1(b).” Surely one would like to examine the data and see whether such a statement is likely to be supported. In any case there seems to be little point in arguing about this. I have the data for the *kudurrus* that I transcribed from Seidl and I will be happy to make them available to anyone who wants them. As the reader will have seen in Lee et al.’s reply, and as I discuss in Section 3, Lee et al. took a different attitude to the deity symbols than Rao has. Rather than argue from ‘first principles’ about what the corpus might look like, they actually went to the trouble of replicating my experiment and developed their own corpus from Seidl’s work (Rob Lee, p.c.): they concluded that by their measures *kudurrus* would also count as (logographic) writing (an issue I get back to below) — suggesting a much less rigid ordering that Rao et al seem to believe.

Rao contrasts the hierarchy that is certainly partly true of *kudurru* texts with the situation in linguistic systems, which “have no such hierarchy imposed on characters or words”. Interestingly, while this is generally true, there are pockets of such hierarchies that do occur in language. A good example is from Egyptian (Allen 2000), where there is a hierarchy that determines the order in which terms denoting human or divine entities

are written. Thus terms for gods will occur in writing *before* terms for humans or other things, even if the divine terms would be spoken *after* the other terms. This occurs a lot in royal names, so that for example *Tutankhamen*, whose name means “living image of Amen”, is written in cartouches with the god Amen’s name first.

In my piece, I made the point that relevant non-linguistic symbol systems abound, a point that Rao seems to be calling into question. As a minor point, they object to my use of Boy Scout merit badges and highway signs as relevant to the discussion, inviting the reader to examine these and see “whether such a comparison bears merit.” Unfortunately this is due to a misinterpretation of what I meant by these comparisons, and it is perfectly possible that I did not explain my point well enough. I was not discussing the forms of the individual signs, but rather the fact that for merit badges and highway signs the symbols can be combined into “texts” that are invariably linearly arranged. Boy Scout badges are worn on sashes, where they are neatly arranged in rows. Informational highway signs, such as indicators for gas stations, accommodations and food, frequently occur together and when they do they are arranged linearly. Of course the individual signs are not linearly composed any more than the Indus symbols are linearly composed or, in general, the basic symbols in any script.

A more serious objection surrounds my claim that to have done the job properly, (Rao et al. 2009) would have to have compared a wider set of linguistic and non-linguistic systems, but I also suggested this would depend upon having a good set of corpora for real non-linguistic systems, something which I claimed nobody has done the legwork of compiling. Rao claims that this ignores the work of (Vidale 2007), who included a description of 10 ancient South Asian non-linguistic systems in his rebuttal to (Farmer, Sproat, and Witzel 2004). This is misleading at best. If the reader will care to reread what I said in my article, it is clear that what I meant was that electronic corpora do not exist on which one could carry out the kinds of statistical tests that Rao and colleagues wish to perform. I believe that remains true. Vidale does indeed discuss ten systems, discusses their distribution and use, and lists numbers of signs in each. But by ‘legwork’, I was not referring to *discussion* of such systems, but rather the development of *corpora* of such systems whereby one could actually perform serious statistical analyses and compare among a range of linguistic and non-linguistic systems. As far as I am aware, there is no such set of corpora. As Rao highlights, Vidale notes the “systematic, large-scale redundancy” of symbol distributions of some of the systems he examines. Perhaps, but as we have seen in the discussion of Vinča symbols and *kudurru* above, translating such broad statements into precise statistical characterizations of the kind necessary for Rao’s methods is tricky at best.

Rao also makes a point of noting Vidale’s observation that the number of distinct signs in the systems he examines is 44, “a far cry from the 400 or so signs in the Indus script.” To that statistic one could add *kudurru* symbols, which number about 60 in Seidl’s (1989) catalog. In fact, Vidale challenges Farmer, Witzel and me to “identify another South Asian system datable to the 3rd millennium B.C. of non-linguistic signs amounting to 400 basic signs or more” (page 344). Non-linguistic systems with hundreds of symbols certainly exist: heraldry (where the sign set is open-ended; see any text on heraldry such as (Slater 2002)) would be one such example, though of course that does not meet the time and space restrictions required by Vidale. But then, what is the basis of those restrictions? A symbol system has as many symbols as it needs. Chinese writing has thousands of characters because it has chosen to represent linguistic units at the level of the morpheme; the Greek alphabet is much smaller due to its decision to represent phonemic segments. If the Indus symbols are non-linguistic, then the vocabulary size of over 400 symbols would merely reflect that it represented a

rich underlying set of concepts. Proponents of the script thesis always like to discuss the advanced state of Indus civilization, arguing that such an advanced civilization could easily have invented writing, and would be hard pressed to do without it. The Indus certainly was one of the most advanced civilizations of the third millennium: would it therefore not be reasonable to suppose that they could have invented a rich non-linguistic symbol system, even if their less advanced neighbors could not?

I end the discussion of Type 1 and Type 2 systems by noting that Rao characterizes the objection to the use of artificial datasets as “a red herring” that “does not change the result that the Indus script is entropically similar to linguistic scripts”. But within the first two paragraphs of the *Science* paper, Rao et al state: “We compared the statistical structure of sequences of signs in the Indus script with those from a representative group of linguistic and nonlinguistic systems. Two major types of nonlinguistic systems are those that do not exhibit much sequential structure (type 1 systems) and those that follow rigid sequential order (type 2 systems).” These (artificial) systems are clearly central to their argument yet as I have argued, they are poor models of any relevant non-linguistic system. Objections hardly seem to constitute a red herring.

Rao takes strong issue with the fact that whereas I discuss Figure 1A from their *Science* paper, nowhere do I discuss their Figure 1B, nor do I do more than mention their later work. On the latter point, let me reiterate: my article was about the paper that appeared in *Science*, not any later work. I was not, for example, making any claims about the block entropy calculations in (Rao 2010): I simply have not analyzed those. But as I said, I assume that at the point the *Science* paper appeared, the methods proposed were intended to be taken seriously on their own, and the evidence presented was intended to be taken to support those methods. It is therefore perfectly reasonable for me to have failed to discuss later work. What about Figure 1B? It is true that I did not present this Figure, which shows high entropy for DNA and proteins, includes Sanskrit among the linguistic examples, and shows that Fortran has a *somewhat* lower entropy than the natural language samples they include. My question about DNA and proteins would be: Why are these relevant? While they are possibly “the two ‘most ancient’ non-linguistic systems”, the topic of discussion surely is symbol systems that were products of the human mind.

Fortran is of course a different matter, but here the question that has always puzzled me is why Fortran was not included in Figure 1A: why did Rao and colleagues not show the entropy growth curve for Fortran in 1A, as Rao (Rao 2010) did and as they also show in their figure 1(b) in their response? I suspect I know: using their original bigram conditional entropy measure, Figure 1A, Fortran would have shown a growth curve that overlapped significantly with the linguistic systems, compromising the visual impact of the plot. But I had no direct evidence for this, so rather than speculate in my piece (as I have here), I preferred to omit discussion. Unlike Rao, I did not view their Figure 1B as including crucial data that I was somehow hiding.

While we are on this topic though, the block entropy in their new Figure 1(b) still leaves one wondering: Fortran is lower than their linguistic examples, but not *much* lower. The same is true of the Figure 1B in their *Science* paper. How would one expect the plot to look if they had included a larger range of languages and text types? I return to this point below.

In Figure 1(b) in their response, Rao includes music (a Beethoven piece) which like DNA and protein, is very close to the maximum entropy curve. They later criticize me for ignoring “the fact that the results already include nonlinguistic systems: DNA and protein sequences . . . as well as man-made sequences (Fortran code and music in Figure 1(b)).” DNA, protein and Fortran we have just discussed. It seems odd to accuse me

of ignoring the case of music, since that data did not appear at all in the *Science* paper; indeed it did not appear in (Rao 2010), where he first introduced the block entropy calculations. It would have required unusual prescience on my part to discuss data that, until now, have not appeared in print.

Finally, while we are on the topic of types of symbol systems, I note that Rao makes a point of enumerating the list of properties that they deem relevant for considering the linguistic status of the Indus symbols. This is of course related to the issue of how to interpret the claims of their work, which we take up in Section 2.2. Rather than enumerate the properties they describe, I will simply present the script-like properties of another system, namely the Mesopotamian deity symbols, a system we know was not linguistic. I also made some of these points, but more briefly, in my piece: (1) Deity symbols are frequently linearly written and in the cases where linearity is less clear, the symbols are written around the top of a stone in an apparent concentric circle pattern (see examples in (Seidl 1989)). One sees such non-linear arrangements with scripts too: the Etruscan Magliano disk, and many rune stones have text wrapped around the border of the stone. (2) There is clear evidence for the directionality of deity symbols: to the extent that “more important” gods were depicted first, these occur at the left/top of the text. (3) Deity symbols are often ligatured together: one symbol may be joined with another. (4) The deity symbols obey a Zipfian distribution. (5) Deity symbols clearly have language-like properties in that certain symbols display positional preference (symbols for the more important gods coming earlier in the text), and certain glyphs have an affinity for each other — for example some glyphs such as the “horned crown” seem to like to be joined together with the “symbol base”. (6) Deity symbols were used largely on standing stones, but were also used in other contexts such as the “necklace” depicted in the Aššurnaširpal II bas relief shown in my piece. (7) Deity symbols are pictographic, like many real scripts — Egyptian, Luwian, Mayan. This would seem like a fairly compelling list of script-like properties: contrary to what Rao suggests, it is certainly possible to find very script-like non-linguistic symbol systems.

2.2 Misunderstandings about Claims

Rao, in the response to my article, as well as elsewhere, makes the point that nowhere do they claim that the conditional entropy measure(s) are sufficient by themselves to demonstrate that the Indus symbols were linguistic. Indeed they do not. The last sentence of the *Science* article reads: “Given the prior evidence for syntactic structure in the Indus script, our results increase the probability that the script represents language, complementing other arguments that have been made explicitly or implicitly in favor of the linguistic hypothesis.”

It is worth stressing at the outset that in my piece I do not claim that Rao and colleagues make the claim that conditional entropy settles the matter — though it is surely true that this was the way many people interpreted their results. The question is, how is one supposed to interpret their claim that their results “increase the probability that the script represents language”? I can think of three possible interpretations. One is as a loose general statement to the effect that there is already lots of evidence for the script thesis, and this additional piece of evidence adds to that body.

The next two interpretations are more formal. A second interpretation is that they are assuming some sort of “deductive” model whereby for a set of features F_i , one is computing the probability of the linguistic hypothesis (H_L), versus the non-linguistic hypothesis (H_{NL}), given these features. Implicitly then, we are combining the features into a classifier that computes $P(H_L|F_i)$ versus $P(H_{NL}|F_i)$ for each feature, and pro-

duces an overall prediction. It is worth noting that Lee’s paper does indeed use the statistical measures Lee and colleagues develop in this deductive sense.

The third interpretation is the “inductive” one that Rao presents in his response, which he claims is the “correct” way to interpret their work. In that interpretation, a set of feature values are observed, and the task is to induce which of two underlying models — H_L or H_{NL} — is the one likely to have generated the observation. The reader will surely recognize that the second and third interpretations correspond pretty directly to discriminative versus generative models. Both of these approaches are used in a variety of classification tasks, and both are perfectly legitimate ways to view the problem. The choice of the generative model as the correct interpretation of their work, and its formalization as they have given it in their response to my piece, is the first time this particular interpretation has been explicitly stated. At the very least, if they had wanted people to understand this point, it would have been good to make it clear at the outset by stating the model in the *Science* paper. One can hardly fault critics for misunderstanding which of two equally viable interpretations was the one intended.

But no matter: how are my criticisms, and those of others Rao cites, relevant to what they do claim, under the various interpretations? Do my criticisms, as Rao would claim, miss the point? Am I building strawmen and destroying them? I do not think so. Under the “deductive” interpretation my critiques would appear to be highly relevant. I argue, for example, that a memoryless random process with a non-equiprobable unigram distribution is classified as “linguistic” by the bigram conditional entropy feature from the *Science* paper.¹ This implies that there are distributions that are misclassified by the feature. How many such misclassified systems are there among plausible non-linguistic symbol systems? We do not know, because Rao et al. did not do a fair comparison of a wide range of linguistic and non-linguistic systems, and demonstrate statistically how well the classifier works. So we do not know whether in fact this feature “increases the probability” of the Indus symbols being a linguistic script. Needless to say, this problem with the deductive interpretation also implies the identical problem with the looser first interpretation I outlined above.

For the “inductive” generative interpretation, the argument is a bit trickier since we need to consider the probability of observing a feature, given one of the two hypotheses. For some features, such as the feature of linear arrangements of symbols, it seems plausible that one has a higher expectation of seeing linearly arranged symbols on the hypothesis that something is a true script, compared with the hypothesis of a non-linguistic system. For bigram conditional entropy, again we really do not know. Again, not enough examples of human-created non-linguistic systems were presented in the *Science* paper to be able to estimate the probabilities. And there is another issue: none of Rao and colleagues’ work provides a statistical measure by which one can assert that something is inside the “linguistic” range: rather, they present plots and invite the reader to eyeball them, hardly a rigorous demonstration. As I noted above, Fortran does not appear to have a conditional entropy that is *much* lower than the linguistic examples. What would one expect the “linguistic range” to look like if a larger number of languages were included and, equally importantly, a variety of genres ranging from prose, to poetry, to genealogies, to graffiti, to shopping lists? Again, we do not know, so it is rather hard to evaluate the claim that for this feature $P(F_i|H_L) > P(F_i|H_{NL})$.

¹ This was the “simple experiment involving randomly generated texts” that I was referring to in my article, a phrase I was misinterpreted to be using to characterize their work.

The bottom line is that if a proposal is to be taken seriously as a formal model, it needs to meet certain criteria. When one proposes a new feature, one must demonstrate that this feature adds information, and increases the discriminative power of one's model. I do not believe Rao et al.'s paper in *Science* does this. And as we have already noted, given several plausible interpretations of what one is trying to demonstrate, it is usually best if one makes it clear at the outset what one means.

There is more that can be said. As we noted, Rao lists a number of features according to which the Indus symbols look linguistic. But one can surely add to their list: suppose one considered features like "is only used to write very short texts" or "occurs in a culture where there are no archaeological markers of manuscript production" (Farmer, Sproat, and Witzel 2004)? Then the script thesis would not fare so well. As for Rao's claim that the prior probability $P(H_L)$ — the *a priori* probability of the linguistic hypothesis being true — is "higher than chance", and the archaeological support for that claim, I will have to pass over that point for lack of space. The interested reader may look at (Farmer, Sproat, and Witzel 2004), and the responses of our critics such as (Vidale 2007), and decide for themselves who makes the more convincing case.

2.3 Computational Linguistics, Decipherment, and the Wider World

Rao concludes by citing the impressive achievements of computational linguistics as a field, and some of the important contributions to decipherment that work in the field has afforded. They question my hypothesis that the editors of prominent science journals may not "even know that there are people who spend their lives doing statistical and computational analyses of text", and claim that "[s]uch a statement fails to acknowledge both the impressive achievements of the field of computational linguistics in recent years and the wide coverage of these accomplishments in the popular press."

I am of course aware of the impressive achievements of my field, and I even alluded to them in the conclusion of my piece. I was trying to hypothesize why, apparently, computational linguists were not asked to review the *Science* piece by Rao (or the Lee et al. piece in *Proceedings of the Royal Society*). It is entirely possible my hypothesis here is wrong and that the editors of, say, *Science* are well aware of computational linguistics as a field. That just makes this apparent oversight all the more disturbing.

I am also well aware of a whole range of work on applying computational techniques to ancient scripts. My point, clearly, was not to claim that such work in general is a 'misuse' of the methods of computational linguistics, as Rao seems to be implying I do. I was rather focussing on two specific papers, the one published in *Science*, and the one in the *Proceedings of the Royal Society*, to which I now turn.

3. Lee et al.'s response

Lee et al.'s response (henceforth "Lee") is much shorter than Rao et al.'s, and boils down essentially to two points. The first point is that one can use randomizations of a given corpus, compare the bigram conditional entropies of those randomized corpora to the conditional entropies of the original, and see if the true conditional entropy is uniformly smaller than any of the values computed from randomized corpora. For 80% of script examples, and for the Pictish symbols, Lee found that this was the case. For sets that were generated by random processes, the distribution of "true" entropy vis-à-vis randomized entropy was more uniform. This method seems to make sense as a way to spot memoryless non-equiprobable processes masquerading as structured systems, such as the example of successive tosses of seven six-sided dice I discussed in my piece.

But this was not part of the methodology presented in the published paper by Lee, which as I showed *is* fooled by such processes. Furthermore, it would still fail to spot non-linguistic systems that have structure, such as Mesopotamian deity symbols.

This leads directly into the second point, since Lee acknowledges my result that texts from *kudurrus* look linguistic, noting that the values that I calculate “for the *kudurrus* dataset places them in a similar level of communication” to Pictish symbols. For them this is an acceptable result, and comes down to a “difference in viewpoint over terminology as to the definition of what constitutes ‘writing’,” noting that I use a “stricter definition of writing than some other researchers, such as Powell (2009)”. They quote Powell as stating that “writing is a system of markings with a conventional reference that communicates information”. There is no doubt that some students of writing systems have indeed adopted a broader definition of writing than what I assume. But these are by no means majority views. The problem with the broader view expressed by the quoted sentence from Powell is that it would seem to classify as writing, any meaningful conventionalized symbol system. Mathematical equations, dance notation, music and (pace Lee et al.) European heraldry, would all count as writing from that standpoint. After all, all of these are systems of markings, with conventional reference, that communicate information. It is hard to believe that Powell himself really wants this broad a definition: the subtitle of his book is “Theory and History of the Technology of Civilization”. While non-linguistic notation systems such as mathematics have been central in the history of the world, I submit that what makes writing work as the “technology of civilization” is precisely that it allows us to record language; see (Sproat 2010b). In any event if that is the breadth that Lee wants, then certainly Pictish symbols (and Mesopotamian deity symbols) are writing, since they are conventional systems of marks that (presumably) communicated information. But the broader definition is also highly misleading. When lay people think of writing they will undoubtedly think of the script and standard orthography they learned in school, which allows them to write anything from letters, to shopping lists, to poetry, to journal articles. Calling Pictish symbols “writing” easily conjures up in the mind of the reader the notion that such symbols could have been used by the Picts for writing treatises on iron smelting techniques. No doubt Lee did not intend this interpretation, but once one calls something “writing” or “language” this seems to be a natural consequence. In any case the broad definition of writing Lee wants to assume reduces the claim that Pictish symbols encode “language” to near vacuity: nobody ever doubted that the symbols conveyed information of *some* kind.

4. Conclusion

It has been a useful exercise to read Rao et al.’s and Lee et al.’s responses, since this has forced me to think even deeper about the issues than I had already done. I believe my basic criticisms are still valid.

I end by noting that Rao et al., in particular, might feel grateful that they were given an opportunity to respond in this forum. My colleagues and I were not so lucky: when we wrote a letter to *Science* outlining our objections to the original paper, the magazine refused to publish our letter, citing “space limitations”. Fortunately *Computational Linguistics* is still open for the exchange of critical discussion.

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