DISCUSSION 8

REPLY TO THE COMMENTS

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I am very grateful to the commentators for their responses to my paper. These are thoughtful, interesting, and diverse. I can only hope that the following amounts to a worthy reply.

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More than one commentator takes up my point that there would be no paradox, if the move from monomorphemic to bimorphemic vocabulary had preceded the loss of phonological contrasts. I gave reasons to believe that it could hardly have happened that way, but Wang Feng offers some fascinating statistical material which is quite new to me. (I am ashamed to say that I know little of the research literature being published within China.) According to Wang Feng, the sound-changes which have made Mandarin phonological structure so much simpler than that of Old Chinese were concentrated within two periods of just a few centuries each; and his Figure 1 shows that the propensity of Chinese to use compound words, though it has risen more or less continuously since the earliest written documents, rose particularly fast also over two short periods. These data, Wang Feng says, make it likely "that an increase of multisyllabic words in Chinese preceded phonological simplification".

That conclusion could follow only indirectly, I presume. Many

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new compounds will have been coined to express new concepts; what is relevant to our topic (as Matthew Chen points out) is only those cases where a compound came into use to replace a monosyllable previously used for the same concept. But filtering out such cases from the entire vocabulary would take a large-scale research effort, and I accept that the raw figures for all compounds may well give us a hint at how relevant usage might have evolved. Even so, it is not clear to me that Wang Feng's specific data support his conclusion. According to these data, the periods of phonological simplification were 200 BC to AD 200, and AD 800–1000.[1] Figure 1 places the first jump in polysyllabic vocabulary at 300–200 BC and the second at AD 1200–1300.[2] In the earlier case, the period of vocabulary change does precede that of phonological simplification (or at least, in view of note 2, overlaps with it); but in the second case the vocabulary development apparently falls centuries later than the phonological simplification. So we have at best a score draw, it seems.

Wolfgang Behr argues that the beginnings of the shift to polymorphemic vocabulary are visible in the records very early, in the Spring and Autumn period (roughly 700–500 BC) or even earlier. Clearly, the earlier that shift got under way, the more plausible it becomes that the repeated losses of phonological contrasts did not in practice create much homophony when they occurred. But the examples Behr offers of early polysyllables mostly appear to be very special cases. Onomatopoeic terms are surely irrelevant: an onomatopoeic polysyllable, like English *ding-dong* or *ho-ho*, is hardly a "compound" of separate "morphemes". And obscure proper names are just obscure – do we know that the Shang dynasty dignitaries mentioned by Behr were even Han native speakers? I do not dispute that there may have been some early compound words which fell outside these categories. However (to repeat) the issue is not that Chinese came to use many compounds, but that even concepts previously expressed by single morphemes came in many or most cases to be expressed by compounds, often compounds of synonyms.

Estimates of the incidence of compound words in early Chinese are necessarily based on written sources. Matthew Chen draws attention to the undoubted fact that the written form of a language is not a perfect guide to usage in casual speech at the same period, suggesting that speech in the Classical period may have been much more polymorphemic than one would guess from the Classical texts. It is recognized by all that the Literary Chinese of recent centuries preserved a style that was long obsolete in speech, but I can hardly believe that the writing of the Classical period was a kind of telegraphese abbreviation of a spoken language with a vocabulary more like present-day 白話. Consider the 詩經 ("*Book of Odes*" – see e.g. Sampson 2007): its lines scan, and in some respects it takes obvious care to record fine details of pronunciation. The language had a contrast between full and phonetically-reduced personal pronouns, rather akin to French *moi* v. *me*, for instance *ŋâiʔ and *ŋâ were full and reduced first-person pronoun forms (quoted here as reconstructed by Axel Schuessler 2007): the difference was purely phonetic, with no semantic implications, yet the forms were written differently, 我 and 吾 respectively. So it does not seem as though there could have been significant chunks of the speech stream which were simply omitted in writing. Furthermore, there were no earlier literary monuments; those who wrote down the poems of the 詩經 had nothing to guide their usage other than the speech they heard around them. That speech must have had the largely monomorphemic character we find in the Classical texts.[3]

I do not dismiss out of hand the possibility that an early date for the shift to bimorphemic spoken vocabulary might offer a solution to our enigma, or part of a solution. But we would need better evidence than the commentators offer, before it seemed plausible that the change in vocabulary preceded most of the phonetic neutralizations. And, if that were true, it would only solve one problem at the expense of creating another: why should Chinese have changed its vocabulary in this unusual way?

Wolfgang Behr suggests that the vocabulary shift was not so unusual in kind. He argues that synonym compounds occur in English too, as a consequence of the mixing of Germanic and French vocabulary, and that a similar mechanism might account for some Chinese synonym-compounds. For English Behr offers two "textbook examples", namely *subject-matter* and *courtyard*. If these examples are really taken from a textbook, I would warmly urge Behr to avoid using that book with his students! He has been grievously misled by Réka Benczes, whose 2014 article he cites. The term *subject-matter* came into use as an Anglicization of a purely Latin phrase, *materia subiecta*, which in turn was a calque of a Greek phrase used by Aristotle, ὑποκειμένη ὕλη. It meant, literally, "material placed under", or as we would say, "placed before (one's eyes)". It is certainly not either a combination of synonyms or a linguistic hybrid. *Courtyard* is an Old French/Germanic hybrid, and could possibly be a compound of synonyms, but I am sceptical. It seems more likely to have been derived as "an open space (*yard*) which is surrounded by a large building (*court*)", as opposed e.g. to a garden surrounded by a hedge or fence. Indeed Benczes eventually (2014: 443–4) appears to accept this derivation, contradicting his own repeated earlier statements that *courtyard* is a synonym compound.

The type of synonym compound which is so characteristic of Mandarin Chinese really is vanishingly rare in English and other European languages familiar to me, and it is difficult to disagree with the usual assumption that these compounds arose in Chinese as a reaction to excessive homophony among individual morphemes.

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A concept that emerges as crucial in this debate is "falsifiability". Falsifiability is not tied up with logical positivism, as Matthew Chen suggests – the latter is an early twentieth century philosophical doctrine with few adherents today. Falsifiability on the other hand is crucial to the enterprise of science. It simply amounts to the idea that a scientific theory is valuable only if it tells us something concrete and testable about the world, rather than merely consisting of empty verbiage. Since scientific theories engage with the world by making generalizations about observable phenomena, a theory has content only if it rules some potential observations out, so that the theory would be refuted if such observations were actually made. The more potential falsifiers a theory has – the "stronger" it is – the more valuable it is, provided it is true. The statement (*A*) that an unsupported solid object will move towards the centre of the Earth has content: it would be falsified by an object remaining stationary, or floating upwards. The statement (*B*) that an unsupported object accelerates towards the centre of the Earth at 32 feet/sec^2 is stronger and better: it will be falsified by any observation that falsifies *A*, but also by an object accelerating downwards at a different rate, or falling at a constant speed without acceleration. A statement (*C*) that "unsupported solid objects move in accordance with their own intrinsic natures" might sound impressive, but as a scientific theory it is empty (taken in isolation, at least), because it rules nothing in particular out: who is to say that a dropped crab would not be conforming to its intrinsic nature if it scuttled sideways in midair?[4]

Linguistics vaunts itself as "the scientific study of language", but falsifiable theories are actually rather thin on the ground in our discipline. As usually understood, though, the functional yield theory of sound-change is one. It takes what André Martinet expressed as a question:

> *toutes choses égales d'ailleurs, une opposition phonologique qui sert à maintenir distincts des centaines de mots parmi les plus fréquents et les plus utiles n'opposera-t-elle pas une résistance plus efficace à l'élimination que celle qui ne rend de service que dans un très petit nombre de cas?* (Martinet 1964: 54)
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> (other things being equal, will a phonological opposition which serves to keep apart hundreds of the commonest and most useful words not resist elimination more effectively than an opposition which serves that purpose only in a very few cases?)

and it answers "yes", making an assertion which is eminently testable: it says that low-yield oppositions may merge but high-yield oppositions will not. This idea (which I shall refer to for brevity as "the functional yield theory") is so widely believed that many textbooks treat it as an uncontroversial truth, and work by Abby Kaplan and others cited in my paper appears to subject it to quite severe empirical tests which it passes with flying colours. The paradox arises because the history of Chinese phonology has often falsified it.

Some of the commentators resolve this paradox by, in effect, replacing the prediction (*P*) "High yield oppositions are not merged" with (*Q*) "High yield oppositions are not merged, or if they are then the language takes other steps to cure the resulting ambiguity problems". Chinese falsifies *P* but does not falsify *Q*: replacing single roots with compounds cured the ambiguity problems which would otherwise have arisen. The trouble is, *Q* is far weaker than *P*, so that the functional yield theory becomes rather empty and uninteresting. I do not believe that most linguists who have argued for the significance of functional yield have understood the theory as containing an implicit "or if it does" clause. Abby Kaplan clearly believes, with good reason, that the evidence she has discussed supports the theory in its stronger, *P* form.

Like some other commentators, Daniel Silverman envisages a process in which sound changes and vocabulary changes went hand in hand ("co-evolved"), rather than either set of changes preceding the other set as a whole. That seems plausible; one may very well believe that things happened that way. But the point is that one cannot reasonably believe it, and also believe *P*. If there was a process of co-evolution, then before it got under way *P* implied predictions which that process was destined to falsify. The fact that vocabulary as well as phonology evolved makes the scenario compatible with the *Q* form of the theory, and Silverman insists that the theory does have content even in that form. But, ironically, the very passage where he expresses that insistence implies that it is unjustified. Silverman writes about "The incontestable fact that we will *never* find … a language … in which communicative success has become genuinely eroded as a consequence of phonetically-based semantic ambiguity". Silverman's "incontestable" implies that we know *a priori* that (*R*) no society will ever let its language change in ways that make it unusable. If we already know *R* as a general truth, then *Q* adds nothing (it just identifies one particular kind of change which would make a language unusable), so *Q* would be thoroughly empty.

Actually I am not sure that *Q* is quite as bad as Silverman implies. I agree that we can hardly envisage societies which become inarticulate because their sole language has decayed too far, but many societies are multilingual, and I suppose in such a society one could imagine one of its languages becoming unusable through too many phonological mergers, with members of the society abandoning it for another language. *Q* predicts that this would never happen, so it is not entirely empty. But *Q* is certainly far weaker than the idea commonly advocated by linguists, and for which Abby Kaplan and others have recently adduced impressive new evidence. The "enigma" to which I should like to find a solution is the relationship between the Chinese language and *P*, not some feeble modification of *P*.

(Of course, if one is interested in the Chinese language but not much interested in general linguistic theorizing, then the enigma will not feel very troublesome. It may be that that is some commentators' position. But I am interested in both.)

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A subtler issue about falsifiability is raised by Abby Kaplan, who points out that the functional yield theory is probabilistic rather than absolute. What should count as "falsifying" a probabilistic theory is a standard problem for scientific method; one counterexample refutes an absolute rule, but there can always be individual exceptions to a statistical tendency.

That said, it seems to me that Abby Kaplan underestimates the difficulty which Chinese poses for the functional yield theory. Her title "A highly improbable data point" suggests that Chinese constitutes just one exception, but in reality, over three millennia there have been many separate Chinese sound changes each of which created large numbers of new homophones – far more than have typically been created by individual sound-changes in Indo-European languages. I have not attempted to put a figure on the homophones created when final stops dropped, so that e.g. 立 *lip "to stand", 栗 *lit "chestnut", 力 *lik "strength", and 利 *lì (or earlier *lih or *lis) "profit" fell together as Mandarin *lì*, but it must surely have been of a similar order to the number in the *j q x* case which I did try to quantify.[5] Even if the functional yield theory only says that changes creating many homophones *tend* to be avoided, why has one particular language violated that tendency not once but repeatedly?[6]

We cannot escape the paradox by saying "Oh well, there are thousands of different languages – among so many, we can expect quite a few to be moderately exceptional to a given statistical tendency, and at least one to be an extreme exception; Chinese just happens to be that one". There do exist several thousand different languages in the world, but for many of them our knowledge of their past history is quite limited. If we knew no more about the historical background of Mandarin than we do in the cases of African or native American languages that were first reduced to writing a hundred years or so ago, I doubt whether we could identify Mandarin as exceptional with respect to functional yield theory. Few languages have histories which can be reconstructed for a period of millennia (and I believe most of those are Indo-European, and therefore not independent of one another throughout their history). We have to say "Of the handful of languages whose histories are well enough known to permit an extensive check of the predictions of functional yield theory, one of them has violated those predictions over and over again". Granted that the predictions are only statistical, if this incidence of departures from the statistical tendency is not enough to falsify the theory, what would it take to do that?[7]

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Some comments seem to create as many puzzles as they claim to solve. Discussing reasons why Chinese might have developed synonym-compounds (if these were not motivated by rescuing the language from ambiguity), Wang Feng suggests that these compounds may have served to form general terms from pairs of more specific terms. For instance, according to the 禮記, the two roots of 朋友 *péngyǒu* "friend", as independent words, meant something like a person one is linked to by external circumstances, or by shared values, respectively. I do not know how reliable the 禮記 etymology is (we know that early Chinese etymologizing was sometimes highly imaginative – probably no serious scholar today believes that 麒麟 *qílín* "unicorn" derived from hypothetical roots *qí* "male unicorn" and *lín* "female unicorn", for instance); but suppose that the 禮記 is right about cases like 朋友. Would that not just leave us wondering why this particular language should have needed to replace so much of its vocabulary with less-specific terminology? We do not find European languages which not only coin numerous words analogous to *sibling*, as a generalization from *brother* and *sister*, but then go on to abandon the older and more specific words and use only the new coinages.

Again, Mieko Ogura argues (reasonably enough) that the need to keep spoken words distinct for successful communication is only one of the pressures which jointly determine the incidence of homophony. Hearers need words to be distinct, but that requires effort by the speaker; speakers want to economize effort, which tends to eliminate phonetic distinctions. Perhaps so, but then why should the Chinese language have increasingly privileged the interests of speakers over those of hearers, giving the former more weight than they are accorded by any European language? (That would be the implication of her explanation for the occurrence of homophony.) A European familiar with polite Chinese behaviour-patterns might have expected the opposite. I well remember how startled I was, as an 18-year-old undergraduate, when my stumbling attempts at translation would cause my Chinese 老師 to launch into elaborate circumlocutions in order to avoid the blunt "No, Sampson, you're wrong" which I would have uttered without a second thought if our roles had been reversed. Not much privileging of speaker's over hearer's interests there.

Wolfgang Behr offers an alternative to Wang Feng's explanation for why Chinese should have replaced so many monomorphemic words with compounds (if this was not caused by the pressure of homophony), remarking that other East and South East Asian languages have a "disyllabic template" which may have influenced Chinese (see the beginning of Behr's section 4). This is an interesting point, which goes beyond my knowledge; I am not sure whether, for those other languages, "disyllabic" also means "bimorphemic", or simply means that their individual morphemes typically consist phonetically of two syllables (as I believe is true of Malay). If a resolution of the enigma might lie in this direction, we would need the hypothesis to be spelled out more clearly and fully before we could assess its plausibility. One immediate objection relates to the fact that the Chinese vocabulary shift took place within the historical period. Given the imbalance of power and civilization by that time between the Han and their "barbarian" neighbours, is it really plausible that the Chinese language could have been so radically reshaped through contacts with the neighbouring languages? Would influences not have been more likely to operate mainly in the reverse direction?

I am not clear whether Wolfgang Behr's point about "disyllabic templates" is essentially the same as Feng Shengli's point (2), about "prosodic structure", because I do not understand the latter very well. (Feng Shengli cites a source "Feng 1997" which unfortunately is not listed in the bibliography of the MS shown to me.) Feng Shengli begins by alluding to the uncontroversial fact that, as I mentioned above, Old Chinese (unlike modern Mandarin) had pairs of full and phonetically-reduced pronoun forms, and he wants to infer from this that the language may have changed its prosodic structure in a way that somehow forced a shift to disyllabic vocabulary. A first comment here would be that a form *ŋajʔ for 我 does not in fact appear to contain more than one "mora", as that term is normally used in linguistics.[8] But, leaving that aside, why would a change to the metrical value of words require them to be compounded? European languages contain words that are metrically quite diverse (compare e.g. English *the*, *lip*, *before*, *squeegee*, *elephant*), yet this does not appear to create any pressure to modify them in order to achieve metrical uniformity.

Both Feng Shengli's and Wolfgang Behr's contributions give the impression that, if a high enough proportion of English words were compounds, or polysyllables, or both, these scholars would expect speakers to begin subconsciously feeling that "crudely simple words like *bread* or *friend* just won't hack it any longer – we need to replace them with more 'respectable' coinages, say *loafbread* and *friendpal*". In the context of a European language this seems inconceivable. So, if something like that did occur in Chinese, why was that language so much more subject than European languages to a requirement for uniform vocabulary structure? Even if the appeal to prosody might help to explain the Chinese vocabulary shift, which I question, again it would solve one problem only at the cost of creating another.

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Matthew Chen suggests that Mandarin morphemes are not really as ambiguous as all that, quoting figures showing that the phonological system provides about as many distinct syllables as there are morphemes which between them account for about 90 per cent (of running text, I take it). Of course present-day Mandarin has the bimorphemic vocabulary as well as the simple syllable structure, so the fact that word-processing software is rather successful at translating *pinyin*, even *pinyin* without tone marks, into Chinese script is not too surprising.[9] But, without the bimorphemic vocabulary, I am not sure that 90 per cent of unambiguous words would make a very successful communication system. It is the less-frequent words which carry most information. If one took a piece of English prose and blanked out the ten per cent of least common words, I wonder how much sense a reader could make of the result.

If the phoneme mergers had not created real ambiguity problems, then someone well versed in Classical Chinese should be able to listen to an unfamiliar passage of it read aloud and understand what he is hearing, without sight of the written text.[10] I do not believe anyone can do that.

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Many things said by the commentators about the Chinese language are very reasonable and just. But none of them, so far as I can see, remove the incompatibility with the otherwise well-confirmed functional yield theory. Indeed, the points I have made here were in many cases already included in my original paper, but evidently they needed to be spelled out more clearly. I hope I have now succeeded in that.

I agree with Abby Kaplan when she concludes that it would be premature to dismiss the functional yield theory because of one language, but that equally it would be unwise to ignore the weight of counterevidence which the Chinese language provides. We are seemingly all missing some point that would resolve the contradiction. I still wonder whether the distinctive nature of Chinese script might be relevant, despite the reasons I gave for saying that it could not be. Could it be that the mass of unlettered people used such a tiny vocabulary, and spoke in such context-dependent ways, that the merged oppositions did not, in their speech, carry high functional yields in practice? It seems quite impossible, and yet …

The enigma remains. But I thank the commentators most warmly for the stimulation arising from our shared attempts to seek a solution.

NOTES

1. The earlier of these periods is too early to relate to the specific sound changes mentioned in my paper, other than simplification of initial consonant clusters.

2. Wang Feng quotes dates both as years "Before Present" and in terms of Chinese dynasties, but the two do not quite match. Qin is conventionally taken as having begun in 256 BC, and (if we reckon with the fact that we are now in AD 2015) that year is much closer to 2300 BP than to Wang Feng's "2200 BP". To keep things simple, bearing in mind that sound-changes do not occur at precise times, I have quoted Wang Feng's "Before Present" dates translated into BC and AD figures rounded to the nearest hundred.
3. At one point Matthew Chen questions whether we can be sure that adoption of polymorphemic vocabulary in speech preceded the corresponding development in writing ("that remains to be demonstrated"). This puzzles me. Apart from the fact that it runs directly counter to his suggestion discussed in the paragraph above, I have always understood that the replacement of predominantly monomorphemic 文言 by largely bimorphemic vernacular 白話 as a written language was a deliberate social change responding to the 1919 "May Fourth Movement". When I first studied this episode, it fell well within the memory of large numbers of living people, so I can hardly believe that the conventional historical account was radically mistaken. I suppose at some much earlier date, when the incidence of compounding was lower than today in both speech and writing, it is possible that for a while writing ran ahead of speech in this respect – if so, how would that affect our topic?
4. This brief exposition is an oversimplification; see e.g. Lakatos (1970). But the complications that arise in real-life science do not mean that it is all right for theories to be unfalsifiable.
5. Whether or not the vocalism of all four words quoted above was identical before final stop deletion is debatable, which is one reason why it is hard to estimate how many homophones that rule in itself was responsible for.
6. I do not understand Matthew Chen's remark that *all* sound changes are instances of neutralization. For instance, Biblical Hebrew underwent a rule by which stops /p t k b d g/ became fricatives [f θ x v ð ɣ] after vowels, and these fricatives did not otherwise occur. It seems a very ordinary example of sound change; where is the neutralization?
7. Abby Kaplan's penultimate paragraph tries to address this problem via a hypothetical scenario in which one early and unexplained sound-change

in itself created so much ambiguity as to trigger extensive compounding, which in turn reduced the yields of remaining phonological oppositions to the point where further mergers could occur without violating the theory. This has the merit of limiting the Chinese violation of the theory to a single event rather than repeated events, though the single event would have had to be a massive violation. Apart from empirical issues (the scenario seems to suggest that more recent phonological mergers should have been on a relatively small scale, which I am not sure is true), I wonder whether it is scientifically rational to reject an account involving many separate theory-violations but to accept one involving a single mega-violation? I cannot think of an analogy among longer-established disciplines.

8. Feng Shengli undermines his own point by quoting a reconstructed Old Chinese pronunciation for 吾 which does not look like a phonetically-reduced variant of his pronunciation for 我, since the former begins with an *ŋr- cluster while the latter has a simple *ŋ-. Rather than following Axel Schuessler as I did above, Feng Shengli attributes his reconstructions to "Baxter 1992", not listed in his bibliography but presumably *A Handbook of Old Chinese Phonology*; however, Baxter's reconstructed form for 吾 in that book was *ŋa (see his p. 208).

9. On the subject of tones, I am at a loss to understand Wolfgang Behr's complaint (his section 2) that I overlooked the fact that Chinese tones derived from earlier syllable-final laryngeals or sibilants (and, he could have added, from a voicing contrast among initial consonants). This is well known, but how does it affect the issue? If a contrast of one phonetic type mutates into a contrast of another type, the incidence of homophony is not changed. Undoubtedly Chinese in its long history will have undergone many sound-changes that did not affect the quantity of homophones it contained, and some of the changes which turned it into a tone language may have been among these, but Behr will surely not dispute that Chinese also underwent many other sound-changes which greatly increased the number of pairs of homophonous morphemes.

10. Of course I mean here "read aloud in modern pronunciation". I had not thought it necessary to spell that out explicitly when I drafted my article, but in view of Feng Shengli's point (1) it seems that I ought to have done.

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