

STATE OF THE FIELD

COULD THERE BE A KOREAN–JAPANESE LINGUISTIC RELATIONSHIP THEORY? SCIENCE, THE DATA, AND THE ALTERNATIVES

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The ethnolinguistic history of early East Asia depends on the comparative-historical study of the different languages. Scholars have long studied the early interrelationships among the major languages of East Asia, but only rarely according to the theory and methodology of scientific comparative-historical linguistics and linguistic typology, in which theories are expected to conform to the data. Among the many highly contested genetic relationship proposals in the region is the “Korean-Japanese theory”. Despite nearly a century of work by some very prominent scholars, no one has given a convincing demonstration of such a relationship, partly due to the paucity of supporting data, despite the fact that the two languages in question are vibrant and well attested. Now two leading scholars of Japanese and Korean linguistics who are familiar with each other’s work, J. M. Unger and A. Vovin, have almost simultaneously published new books on the topic, one in favor of the theory, one against it. The contributions and flaws of the two books, and their position relative to the development of a scientific tradition of comparative-historical linguistics, are discussed. Special attention is paid to Koguryo, the extinct Japanese-related language once spoken on the Korean Peninsula that is crucial to any discussion of the historical relationship of Japanese and Korean.

INTRODUCTION

The early ethnolinguistic history of East Asia has long been controversial. Leaving aside non-scholarly issues, the reason for most of the trouble is the state of the theory and methodology of the disciplines of comparative-historical linguistics and typological linguistics in connection with the study of East Asian languages. By comparison with Western

Abbreviations: AKog, APK: Archaic Koguryo, Archaic Puyo-Koguryo; CJK: Common Japanese-Koguryoic; KJ: Korean-Japanese; LOC: Late Old Chinese; MKor, MK: Middle Korean; OJpn, OJ: Old Japanese; OKog: Old Koguryo; OKor, OK: Old Korean; PJK: Proto-Japanese-Koguryoic; PJpn, pJ: Proto-Japanese; pKJ: proto-Korean-Japanese; SOV: Subject-Object-Verb. A star (*) marks a form attested in Chinese character transcription.

Asia, South Asia, and Oceania – not to speak of Europe and the Near East, which have enjoyed advanced scholarship since the very beginnings of these disciplines – it is as if a powerful spell has been cast over East Asia and its neighbors, preventing the region from catching up with the rest of the world.

One of the most outstanding manifestations of this phenomenon is the great number of relationship “theories”¹ involving the major East Asian languages. This was headlined in 2001–2002 by two special issues of *Gengo Kenkyū*, the most respected general linguistics journal published in East Asia. The first features articles by the leading Japanese proponents of the “Japanese-Dravidian theory”, the “Japanese-Altaic theory”, and the “Japanese-Austronesian theory”, who expound their views in full. The second issue features articles by leading Japanese specialists in the Dravidian, “Altaic”,² and Austronesian language families, who refute the corresponding articles in the previous issue. While it is extremely odd that the editors did not include an article on the “Korean-Japanese theory”, the single most prominent one,³ in all fairness it must be mentioned that there are many more such “theories”. Nevertheless, it remains puzzling that anyone could take any of these particular ones seriously enough to expend so much effort on them. Among professional linguists in Japan today the dominant view is either agnosticism – that is, “we do not know, or cannot know, anything about the relationship of Japanese to any language spoken elsewhere”⁴ – or the bald assertion (often with non-linguistic motivations) that Japanese is an isolate, unrelated to anything else. These views might seem “safe” to the uninitiated, as they certainly do to their proponents, but because they too ultimately reject science⁵ they are equally as problematic as the above relationship “theories” proposed and attacked in *Gengo Kenkyū*. The situation in Korea is less complex, in that most Korean linguists evidently support one or another version of the “macro-Altaic theory” (see below), but there too alternate theories abound, including a “Korean-Dravidian theory”.⁶

During the early days of comparative-historical philology in the late eighteenth century and well into the nineteenth century, similar “theories” – Indo-European and Semitic, Uralic and “Altaic”, etc. – were propounded quite seriously. But in the nineteenth century, as the basic regularity of phonological change was discovered and its conceptualization

1 They are so tenuous, irregular, and poorly supported by data that they are untestable and therefore not theories at all, from the point of view of science; for the sake of simplicity they are called “theories” here.

2 The “Altaic” theory has been resoundingly disproved from several points of view, most recently by Vovin (2005b).

3 A version of the theory still dominant in Japan and Korea includes the two languages within a much larger family, “Macro-Altaic”. The article by Itabashi (2001) on Japanese and “Altaic” thus includes Korean. However, “Macro-Altaic” is now rejected by the main proponents of KJ outside Japan and Korea. See also the preceding note.

4 The relationship of Japanese to the extinct Koguryo language of Manchuria and Korea is passed over in silence by *Gengo Kenkyū*, perhaps because the name *Koguryō* (= *Koryō*) is the source of the dominant foreign name for Korea (*Koryō*) and the Korean language (Beckwith 2007a), and earlier scholars often confused the two languages. Also, Ryukyuan is incorrectly considered by many to be simply a dialect group or “branch” of Japanese.

5 Beckwith 2009, p. 407 n. 59, pp. 417–18 n. 88; Beckwith forthcoming–b; cf. Searle 1995.

6 A version of it is supported by Unger; see below.

refined, philology developed into the science now called linguistics.⁷ Text philology developed along with it into scientific critical text edition. And in the mid-twentieth century, linguistic typology, which focuses on linguistic universal tendencies in human language, became one of the driving forces behind the revitalization of linguistics as a whole. As a result, unscientific relationship “theories” attempting to relate Indo-European, Semitic, and Uralic (among numerous other languages and language families large and small) have sooner or later been refuted and eventually excluded from professional discourse. Unfortunately, this has not happened in the study of the languages of East Asia. Although the special issues of *Gengo Kenkyū* might represent timid steps in the direction of science in East Asian comparative-historical linguistics, nearly a decade later the field remains dominated by unprofessional ideas. To make matters worse, nearly all scholars of East Asian studies in general have no idea what critical text edition is,⁸ and informed typological work is either naïve or nonexistent.⁹ The fundamental problem is, simply, the lack of the idea of science in these fields of East Asian studies.¹⁰

For the past century East Asianist scholars, especially but not exclusively non-native Japanese and Korean scholars, have been entranced by the “Korean-Japanese” (KJ) theory: the proposal that the Korean and Japanese languages are related divergently or “genetically” from a common ancestor, “proto-Korean-Japanese” (pKJ). Major publications by Martin¹¹ and Whitman¹² have propelled this theory into a position of dominance in the comparative-historical linguistic study of the two languages, despite the staggering problems standing in the way of demonstrating any premodern relationship whatever between them other than an insignificant loan relationship. Indeed, KJ has come to be seen by its proponents as a kind of orthodoxy, much as Sinologists outside China (as well as native Chinese linguists) almost universally believe in “Sino-Tibetan”, a similar speculation that should be in search of a scientific theory.

Although at this point one might be inclined to ask whether the jaded agnosticism of the Japanese linguists noted above is not perhaps justified, their view actually implies that science does not work in East Asia, or at least not for East Asian languages. But science works everywhere, including in East Asia, whether or not its validity is recognized by scholars working on the region. Accordingly, “theories” which require as their foundational principle that *some or all of the data must be rejected or ignored* cannot be accepted as valid. Scientific theories *must* account for the data; if a theory does not conform to the data, it is the theory that must be modified, not the data. This principle seems not to be well understood by most scholars working on East Asian ethnolinguistic history.

7 For comparative-historical linguistics in general see Campbell 2006; for Indo-European see Szemerényi 1996.

8 See the Preface of the lone published example of a critical edition of an East Asian text (Thompson 1979).

9 For typological linguistics, including criticism of largely unexamined East Asianist notions about morphophonology (such as “phonemic” tone and register, which are mentioned by both Unger and Vovin) and morphosyntax, see Beckwith 2006b, 2007b, 2008.

10 There is no space here to expound at length on the historical and political roots of this widespread problem. See the references in note 5.

11 Above all, Martin 1966. He later backed away from his early enthusiasm for KJ.

12 Most influentially, Whitman 1990.

In the last half-dozen years, a number of books on the historical linguistics of the languages of ancient Korea and Japan have appeared or are forthcoming. They include, in chronological order, the first monograph on the Koguryo¹³ language;¹⁴ a two-volume study of Western Old Japanese;¹⁵ a new history of the Japanese language;¹⁶ and a revised and updated version, in English, of Lee Ki-moon's history of the Korean language.¹⁷ However, with the exception of the comparative parts of the first of these books, and some related articles, for many years no major new work on the interrelationships or lack thereof among these languages had appeared, or was slated to appear, until a few months ago, when suddenly not one but two new books were published: firstly, by J. Marshall Unger,¹⁸ arguing in favor of the KJ theory, and subsequently by Alexander Vovin,¹⁹ arguing against it. Both scholars are leading specialists in Old Japanese and in the Korean-Japanese theory, and their works thus may be considered to sum up the state of the art in Korean-Japanese comparative studies today.



This article is devoted to examining firstly whether these two new books, and other recent works, succeed in supporting or falsifying the dominant KJ theory, and secondly, to what extent they utilize or depend on scientific comparative-historical linguistics and linguistic typology, and consequently attempt to build theories that conform to the data, at least. That is, can one declare that science has finally begun to replace the alternatives in these fields of East Asian studies?

The first of the new books is Unger's *The Role of Contact in the Origins of the Japanese and Korean Languages*, which argues in favor of a divergent or "genetic" relationship between Korean and Japanese. Having assumed such a relationship, the main problem the author then addresses is how exactly the similarities and dissimilarities between the two languages could have come about.

A brief Introduction summarizes his argument (p. xi): "Remove the borrowings from the etymologies offered in support of proto-Korean-Japanese and one is still left with more lexical material pointing toward common origin than ought to be there by chance, not to mention parallelisms in grammar not seen in nearby languages of similar type." He also mentions a few other putative justifications for his theory (cf. pp. 1, 5) and concludes (p. xiii) "that the hypothesis of a genetic relationship between Japanese and Korean is no worse than the alternative". This unusual argument, which Unger repeats elsewhere in his book, assumes that the convergence theory is *just as weak* as the divergence theory. As will be seen below, this is clearly not the case.

13 I transcribe this name and other major proper names of the region traditionally, without diacritics – e.g., "Koguryo" instead of "Koguryō" – except in verbatim quotations of other scholars.

14 Beckwith (2004a); I generally cite the second edition (Beckwith 2007a) in this article.

15 Vovin 2005–2009.

16 Frellesvig 2010; this had not yet appeared by the time this article went to press.

17 Lee and Ramsey forthcoming.

18 Unger 2009.

19 Vovin 2010.

Chapter 1, “Contact Hypotheses and Their Consequences”, discusses divergence and convergence in general and in connection with the ancient and early medieval languages involved in any comparative-historical study of the Korean and Japanese languages; much of the chapter is highly polemical. He states, “one can never prove that Korean and Japanese – or for that matter, any two languages – are *unrelated* . . .” (p. 5, his emphasis). This statement explicitly contradicts the principles of scientific comparative-historical linguistics, and in fact rejects science in general.

In Chapter 2, “Critical Assessment of the pKJ Reconstruction”, Unger presents his views on “proto-Korean-Japanese” phonology and his main argument (p. 65) for a genetic relationship between Korean and Japanese, based on shared grammatical features. He outlines nine syntactic features shared by Japanese and Korean (and in one case also Chinese, which he rightly notes is “not an SOV language”), claiming (p. 66) that these shared features require an explanation:

If Korean and Japanese are unrelated languages, one or two might be purely fortuitous, but could all nine be? If any of them are not purely coincidental, then they must be explained in terms of contact. On the other hand, if Korean and Japanese are genetically related, then some of these features might be taken as common innovations showing that proto-Korean-Japanese broke off from a larger family, such as Macro-Tungusic. Indeed, the focus particles of Korean and Japanese seem to be prime candidates for being the products of such a shared innovation.

He lists these features (p. 65):

- highly developed systems of honorific verb morphology, including . . . auxiliary verbs of giving and receiving
- the heavy use of focus-marking postpositions of specific function . . .
- distinct attributive and predicative verb and adjective forms . . .
- heavy reliance on abstract nouns for clause nominalization . . .
- predominance of aspect over tense in the interpretation of predicates
- zero pronominalization
- infrequent use of overt plural or class marking
- verb forms indicating degree of certainty or probability
- the use of final particles to mark different main-clause types (questions, emphatic statements, etc.)

Although even for Japanese some of these claims require considerable clarification, for the sake of argument they can be accepted. Yet is the sharing of these morphosyntactic features really so remarkable? Unger mentions that I note “Japanese . . . shares specific phonological, lexical, and typological grammatical features with Tibeto-Burman languages” (Beckwith 2007, pp. 160, 162). Indeed, every single one of the features on Unger’s list is also a feature of Tibetan. Linguists working on that language should thank him for having summarized so many of its outstanding features in a convenient list. But Tibetan and Japanese are unlikely to be related genetically, so Unger’s contention that his list is a *unique*

sign of a genetic relationship between Japanese and Korean, and that *only* a genetic relationship could explain what he believes to be such remarkable, unprecedented similarities in structure, is not supported. In fact, Joseph Greenberg – none of whose fundamental works on typology²⁰ are cited by Unger – long ago showed that such similarities, often including the finest details, are purely typological and are set by a few basic parameters. After this presentation, Unger remarks, absolutely correctly (p. 71): “The foregoing argument is, of course, no substitute for a classical demonstration of genetic relationship.” His remark may be restated more explicitly and precisely, “The foregoing argument for a *typological* relationship is, of course, no substitute for a scientific demonstration of a *genetic* (divergent) relationship.”

In Chapter 3, “Convergence Theories”, Unger criticizes “Beckwith’s theory” and “Vovin’s theory”, arguing stridently and at considerable length against the former. He ignores the extensive, detailed publications showing that his arguments are directly and explicitly contradicted by the data, and in particular, that his claims to have discovered significant errors in my reconstructions of Koguryo or Japanese-Koguryoic forms are incorrect.²¹ Unger now repeats these claims almost verbatim from his 2005 paper, with the addition of new mistakes and misunderstandings. He also suggests (p. 76) that I have attempted to hide a great number of Korean-like words that he believes are found in the data, or something else equally dastardly, and insinuates that because I have based my reconstruction of Koguryo on the Korean Peninsula dialect of Chinese, not on “standard reconstructions of Middle Chinese”, there are “doubts about the objectivity” of my “identification of Koguryōan morphemes.”

It therefore seems necessary to repeat, once again, that the language material from the former Koguryo Kingdom includes much that is simply pure Chinese, and therefore of no use for reconstructing Koguryo or any other non-Chinese language.²² A scholar who actually read the Chinese text of the *Samguk sagi* 三國史記 would immediately discover that in most cases the existing Chinese toponyms remained Chinese after the mid-eighth-century changes; such cases thus tell us nothing about the Koguryo language or other native languages of the peninsula, though sometimes they can tell us something about the phonology of Archaic Northeastern Middle Chinese, the language spoken by the Chinese inhabitants who dominated the northwestern part of the Korean Peninsula under Koguryo rule. Many other toponym transcriptions are unglossed phonetic imitations, as also explained in my book. It is certainly conceivable that one or two examples might be coincidentally similar phonetically, and actually might have been intended as real glosses. Nevertheless, if they do seem to be homonyms it would be poor scholarship to include such examples as Koguryo or other non-Chinese morphemes; at best they can be used to help reveal the phonology of the transcriptional language, Archaic

20 Most famously, Greenberg 1963. Unger does cite one work by Greenberg, on Indo-European and “its Closest Relatives”, but it is well known that Greenberg’s highly unscientific forays into comparative-historical linguistics are to be avoided at all costs. Unger’s bibliography cites a number of general works on the comparative-historical method, but does not include a single reliable, major one.

21 These works (Beckwith 2005a, 2006a, 2007a) all appeared two years or more before Unger’s book was published. Most of his arguments have already been thoroughly examined and criticized therein and will not be further discussed here.

22 Beckwith 2004a, 2007a.

Northeastern Middle Chinese, which is how I have used them. There are, however, a large number of such un glossed phonetic imitations, and in most cases the similarities are trivial at best, so I only discuss those that are significant for the reconstruction of the two languages on which the book focuses, namely Koguryo and Archaic Northeastern Middle Chinese. The idea that the neo-medieval anti-scientific creations considered by him to be “standard reconstructions of Middle Chinese” are to be preferred to linguistic reconstructions based on actual data is unfortunately widespread among East Asianists.²³

Indeed, Unger’s preference identifies one of the most deep-seated of all the problems afflicting the comparative-historical linguistics of ancient and early medieval East Asia: all of the sources on Japanese and Korean are written in Chinese character transcriptions, of several different kinds. Interpretation of the transcription systems is certainly highly controversial in many cases, but neither Unger nor Vovin are evidently aware, firstly, that much work has been done in recent years on early medieval Chinese transcriptions of other known non-Chinese languages in the region, for which we have contemporaneous segmental (alphabetic) transcriptions in some of those very languages, as well as transcriptions of Chinese itself from the seventh century on; secondly, that much has also been done on ancient Chinese transcriptions of known non-Chinese words, despite the unfortunate non-linguistic frameworks or transcription systems applied to the reconstructions by some of the scholars who have worked on them, with the effect of obscuring their results; and thirdly, that an entire book has been published that goes into great detail on the sources and interpretation of the phonological system of the Koguryo language, a relative of Japanese once spoken in Korea and southern Manchuria. Surely some of this ought to be relevant enough to the early history of Japanese and Korean to have been worth perusing.

In Chapter 4 “Japanese Borrowings from Old Korean”, Unger compares and contrasts some loan and non-loan etymologies – the latter being Japanese and Koguryo (his “para-Japanese”) words. In the first section Unger (pp. 107ff.) contrasts words, which he argues are borrowed from Old Korean into Japanese, with Old Koguryo words (called by him “para-Japanese”) that are attested in the *Samguk sagi*, mostly according to my reconstructions.²⁴ This is on the whole a careful account of the words he covers, but they are a small percentage of the total number of words he claims are retained from his proto-Korean-Japanese, most of which Vovin shows are not in fact retentions but loanwords or invalid comparisons.

Unger usefully observes that the change of Proto-Japanese (PJpn) *r > Ø “was evidently conditioned by the preceding vowel” (p. 113). His example involves Old Japanese (OJpn) *ki (< *kwi) ‘tree’ – with regard to which the Old Koguryo (OKog) cognate *kir ‘tree’ is overlooked by him. His observation is presented within the framework of the KJ theory, so its significance for the linguistics of the region might be missed. It helps to explain the phonology of Puyo-Paekche *ki ‘fort, city (城)’ (without -r), cognate to OKog *kuær (with -r) ‘id.’ from Archaic Puyo-Koguryo (APK) *kuru ‘id.’ The Paekche word, borrowed into Japanese, is attested as OJpn *ki (*kwi) ‘fort’. This similarly explains the “-r-loss” seen in some other Japanese reflexes of Koguryo words, as well as in Old Korean loanwords in

23 Citing Beckwith 2004a; see now Beckwith 2007a.

24 See Beckwith 2007a.

Japanese, and further clarifies that the word *mir ‘three’ attested from the southeastern Korean Peninsula area²⁵ must be an Old Koguryo word, not a Puyo-Paekche or Proto-Japanese-Ryukyuan word.

Unger also reverses an earlier argument of his in which he proposes that the Japanese-related words preserved in Korean Peninsula toponyms were Korean words borrowed by Japanese in the Kofun period.

He says (p. 107), “I was mistaken . . . because I assumed that Koguryōan, Paekchean, and Sillan were distinct languages and that Korean necessarily descended from Sillan. But if Koguryōan, Paekchean, and Sillan were simply three early dialects of Old Korean – that is, if the *Samguk sagi* place-name evidence merely shows the existence of a para-Japanese language antecedent to Korean,” it is possible, he argues, to identify the non-Japanese-like synonyms he studied as “Old Korean loans into Early Old Japanese”. Unfortunately for this assertion, his idea that all three major languages of the Korean Peninsula in the period before United Silla (from 667 AD on) were actually Korean flies in the face of the data, as shown below.

Chapter 5, “Syncretism in Japanese Mythology”, which is devoted to an exposition of mainly Japanese folkloristic material, is unrelated to the subject matter of the rest of the book. In his brief discussion of mythology that is related to the book (pp. 87–88), Unger complains that I make “much of the similarities among the progenitor myths of Koguryō, Puyō, and Paekche,” which actually belong to a “narrative type” common to them and to the Southern Tungusic peoples. In other words, they are not particularly similar or distinctive. Immediately after this assertion, he follows Mark Byington’s claim (according to Unger) that the Koguryo and Paekche elites “sought political legitimacy by adopting imitations of Puyō creation myths to explain their own ancestries.” So the myths (which incidentally are not “creation” myths but national origin myths) are both non-distinctive and at the same time so distinctively similar that they seem to be imitations of each other. What about the data? The sources say absolutely nothing about why the myths are so similar, but they do very clearly state that the Koguryo, Puyo, and Puyo-Paekche peoples all spoke more or less the same language before and during the migrations into the Korean Peninsula, and also shared much the same culture; it is thus hardly surprising that they shared the same national foundation myth – the three versions of which are practically identical in the sources – indeed, that is to be expected. Claims of imitation or illegitimacy go far beyond the historical sources on these peoples and are pure speculation. This shared myth – a variant of the national foundation myth that is one of the characteristic features of the Central Eurasian Culture Complex²⁶ – is *radically unlike* any of the Korean or other stories Unger mentions.

25 Unger’s criticism (pp. 34–35, n. 41) of my interpretation of the transcriptions of this word is certainly justified. However, it seems to be the only such example noted in his book – the other cases that he claims are errors have mostly already been clarified (if they are his own errors), or corrected (Beckwith 2005, 2006), and subsequently incorporated into the second edition of my book (Beckwith 2007a) – and his own astute comments on the Korean internal reconstruction of the Korean “semantic” reading of the transcription character indicates there is still a problem with this particular item, calling for more study. In connection with the word for “three”, note further the correct remark of Vovin (p. 181) that even when numerals are borrowed “they are also borrowed as a set.”

26 See Beckwith 2009.

Chapter 6, “The Korean Role in the Rise of Kofun Culture”, discusses the transmission of Korean Peninsular political-military culture (part of the Central Eurasian Culture Complex) to Japan.²⁷ In his discussion he remarks (p. 151 n. 5) that the Japanese toponym Sasanami 樂波 was originally a phonetic transcription of a “para-Japanese” immigrant name “similar in sound to Takawoka (changed in writing to 高丘); and indeed LOC [Late Old Chinese] *lakpai 樂波 would be close to pJ [Proto-Japanese] *tak(a)bə(ka).” This is absolutely unacceptable phonologically for these languages.

Chapter 7, “Languages in Contact with Early Japanese”, rejects the “southern-route view” of transmission of wet-field rice cultivation to the southern Korean Peninsula and northern Kyushu from the Ryukyus and further south in the Yayoi period, and proposes what Unger calls a “middle-route theory” which “bears a superficial resemblance to Beckwith’s idea” but which (radically unlike my view) actually focuses on theories that one or another distinctly *southern* language – Austronesian, Austroasiatic, Dravidian, etc. – was responsible, according to his version of the “Nusantao” South China Sea trade region theory.²⁸

Despite other scholars’ criticism of his views, Unger rightly discusses their works and cites them. The book’s bibliography of works cited includes my book on Koguryo as well as three other articles by me, including one published in the *Journal of Inner and East Asian Studies* wherein I criticize Unger and Vovin’s articles, both of which are published in the same issue of that journal.²⁹ Unger also includes Vovin’s book (discussed below), though it was then still “forthcoming”; he refers to it many times in the text, often remarking explicitly on Vovin’s criticism of his views. His bibliography also includes twelve other publications by Vovin. See below on Vovin’s bibliography.

The book concludes with several indices of language forms – Middle Korean, Modern Korean, Old Japanese, Modern and Middle Japanese, and English glosses – but none for Koguryo, Paekche, or the other languages mentioned. No reason is given for their omission, or the lack of even a basic general index of topics, names cited, etc.

The main contribution of Unger’s book is the fact that he openly presents and discusses some of the major issues that need to be addressed by any theory about the early ethnolinguistic history of Northeast Asia. He presents a complex argument in favor of his variant of the Korean-Japanese theory and against his named and cited opponents. His stated task is to explain how the two languages could be related if they do not share almost any genetically inherited lexical material or flexional morphology – as he openly admits (pp. 1, 37, 59–60), anticipating the arguments of Vovin in his then-still-forthcoming book against most of the etymologies of Samuel Martin and John Whitman. Unger argues that attested Korean and Japanese retain the morphosyntactic structure and a few remnants of the inherited morphophonology of pKJ, which he claims was spoken over three thousand years ago; the loss of the putative inherited material is, he argues, the result of extensive contact with unrelated languages. He adduces much archaeological, mythological, and linguistic material to support his approach. Although a shared morphophonological system is

27 *Ibid.*

28 Solheim 2000.

29 Beckwith 2005a, Unger 2005, Vovin 2005a.

the *sine qua non* of a demonstrated genetic relationship, Unger nevertheless argues it is possible to go beyond the limits of scientific comparative-historical linguistics in circumstances involving intensive, prolonged contact and great time depth (pp. xi, xii–xiii, 1, 20, 62), in order to prove his theory “anyway”: that is, despite the lack – by his own admission – of much (if any) actual morphophonological data.

The book’s main defects are, firstly, misuse or ignorance of what data he does use; and secondly, failure to stick to scientific linguistic methods.

As for the first problem, Unger notes that according to Fukui:³¹

OJ *kwi* ‘fortress’ is commonly associated with Paekche whereas its synonym, OJ *sasi*, which seems to be cognate with MK **cas* ‘fortress’, is often used in reference to places in Silla . . . The so-called Koguryōan word for ‘fortress’, written with the character 忽, was **hol*, as Fukui reconstructs it;³² since OJ *sasi* resembles neither **hol* (cf. OJ *kura* ‘storehouse’) nor *kwi* (which, as argued in Yun 1994, probably derives from the same source as **hol*), he [Fukui] concludes that Sillan was Old Korean. But this does not prove that Sillan was the only variety of Old Korean since the **hol/kura/kwi* word need not be taken as anything more than para-Japanese.

Unger then argues that “*sasi* and *kura* . . . were translation equivalents” in Old Japanese. But this entirely misses the point of Fukui’s (correct) analysis of the data. The toponyms with 忽 (also written 骨) and those with other words for ‘fortress, walled city (城)’ are in complementary distribution geographically: in the pre-expansion territory of Silla there are *no* examples of 忽 / 骨 (OKog **kuær*, from attested APK **kuru*), as shown long ago by Toh Su-hee³³ on the basis of the *Samguk sagi* and by Yun Haeng-sun on the basis of the Japanese sources. Similarly, toponyms with the cognate Puyo-Paekche word **kwi* (derived regularly from attested APK **kuru*) are found not in Koguryo territory or Silla territory but only in former Paekche territory and in Japanese sources about Paekche.³⁴

As for the second problem, most of Unger’s views – which he does present honestly and openly (especially in the Introduction; see the quotations above) – on the comparative-historical method, though often noted by Unger himself as being “unconventional” or the like (for example pp. 36–37), are not merely unorthodox; they violate

31 Fukui (2003), cited in Unger (2009, p. 23).

32 Further on, Unger (p. 108) gives my reconstruction, 忽 OKog **kuær* ‘fortress, walled city (城)’ (Beckwith 2007a), though he does not cite the source. Note that **hol* is *not* a “reconstruction” of the eighth-century reading of this character; it is purely and simply the modern Sino-Korean reading.

33 Toh 1987–1994.

34 Yun 1994. Unger cites Toh’s books in his bibliography, but seems not to have looked at them, since he claims (p. 80) that “our knowledge of virtually all ‘Koguryōan’ morphemes depends on names of places spread widely across the peninsula.” But with only a handful of exceptions the Koguryo morphemes are found *only* in the area of the former Koguryo kingdom, as Toh’s maps very clearly show (though sometimes one must sift out purely Chinese or unglossed and therefore unknown words); some related words are also found (as expected) in former Paekche territory, as discussed below. This is all presented in great detail in Beckwith 2007a, pp. 236–49.

the basic principles of the comparative-historical method³⁵ and the mass of literature on Indo-European, Uralic, Semitic, Bantu, Mayan, and many other accepted genetic relationships.³⁶ It is thus not surprising that Unger (pp. 98ff.) accepts “glottochronology”; but he even takes Ōno Susumu’s theory of a Japanese-Dravidian genetic relationship seriously, arguing (pp. 173–74), “Material evidence amply points to Dravidian languages playing” the role of a “lingua franca of some kind” among the Nusantao maritime traders. One of his two examples, taken from Ōno, is Tamil *tampal* ‘waterlogged paddy’, which he compares with Japanese (not Old Japanese) “*ta(nbo)*” ‘rice paddy’ (from Old Japanese *ta* ‘rice paddy’). Such comparisons may not appear to Unger to be “quite as far-fetched as they seem”, but they are to Dravidianists – the idea of a connection between Tamil and Japanese has been rejected by Dravidian experts, including a Japanese Dravidianist³⁷ – yet it hardly takes an expert to reveal the obviously unscientific nature of every aspect of Ōno’s proposal. Unger also says (p. 17), “The principal difficulty with the genetic hypothesis” theory of a Korean-Japanese relationship “is that the interval between the Yayoi migrations and the time when written records indicate that Korean and Japanese were distinct languages is only a few centuries long.” If that were true, it would be difficult to demonstrate a genetic relationship even among the Indo-European languages. Unger’s theory is founded on such misconceptions and theoretical and methodological errors.



The second of the new books is Vovin’s *Koreo-Japonica: A Re-evaluation of a Common Genetic Origin*, which focuses on showing that there is *no* genetic KJ relationship, and that the similarities between the Korean and Japanese languages are to be explained as due primarily to convergence – above all, to a Korean loan influence on Japanese during the Kofun and Asuka periods.

A brief Introduction mentions some of the early work on the KJ theory and some of the more recent work in the KJ tradition begun by Samuel Martin, including some by Vovin himself, who until very recently was an outspoken proponent of the KJ theory. However, Vovin ignores all of the publications against the KJ theory by scholars other than himself, with the exception of the comment, “it seems that the only person who held strong reservations [about KJ] was Hattori [Shirō]”. Vovin also gives some of his theoretical views, defines what he means by “Proto-Korean”, “Proto-Japonic”,³⁸ and “Proto-Japanese”, and

35 See, e.g., Campbell 2004.

36 It is necessary also to note Unger’s preference (and in other publications, Vovin’s) for naïve terminology such as “para-Japanese” and the like, adopted from non-professional works. Unger explains (p. 23 n. 27) that the “para-” prefix designates “a variety of any language named for place X but spoken elsewhere”. According to this peculiar idea, the English spoken in, e.g., Ireland, North America, Australia, and New Zealand would be called “para-English”, “para-English”, “para-English”, and “para-English”, respectively. Such terms embody basic misunderstandings about comparative-historical linguistics.

37 The refutation of Ōno’s theory by Kodama (2002) is cited in my book (Beckwith 2007a, p. 260), but not in Unger’s bibliography. Instead, he cites Ōno’s books on the Dravidian theory.

38 Vovin (p. 5) defines “Japonic” as a language family “consisting of two languages: Japanese and Ryukyuan, with a sharp boundary between the two to the north of the Amami island group in the Ryukyuan archipelago.” This ignores his own different, wider usage at several points in the text.

devotes almost a page of his text (pp. 6–7) to “fundamental typological differences” between Korean and Japanese, namely, five lines each on “Ergativity” (“Korean is historically ergative,” unlike Japanese) and “Passive” (“the morphological passive in Korean is quite young”), and a short paragraph on “Ablaut” in which he claims, “There is no reconstructible ablaut for Proto-Japanese.” In view of the fact that the argument in Unger’s book is based heavily on typological similarities, Vovin’s extremely sketchy coverage of typology is disappointing. Moreover, since Vovin’s book focuses overwhelmingly on historical phonology, it is unfortunate that he does not even mention the deeply systemic structural differences in phonology between Korean and Japanese, which languages some linguists have rightly noted are fundamentally, even “startlingly”, different.³⁹

Chapter 1 covers “Proto-Korean and Proto-Japonic Reconstructions and Their Role in the Comparison of the Two Languages”, including the subsections “Recent Advances in Proto-Korean Reconstruction” and “Recent Advances in Proto-Japonic Reconstruction”. Vovin is unquestionably one of the leading linguists in Old Japanese studies, and a major scholar in early Korean studies as well, but quite a few of the views presented in this chapter are controversial within these fields. Discussion of other scholars’ views would have strengthened his case.

In Chapter 2, “Morphological Comparisons”, Vovin carefully analyzes the paradigmatic morphology of the two languages and rejects most of the comparisons that have been proposed by earlier scholars, arguing instead that in some cases Japanese borrowed forms from Korean in the pre-Old Japanese and early Old Japanese period. His many quotations of Middle Korean (and a few Old Korean) and Old Japanese sentences, fully glossed, are models of scholarly presentation (though Vovin’s careless typographical formatting of them makes them hard to read). They are an important contribution to this chapter, which is perhaps the most valuable part of the book.

The bulk of the text consists of Chapter 3, “Lexical Comparisons”. It contains two parts: “Doublets in Western Old Japanese” (pp. 92–94), which despite its brevity is of fundamental importance, and “Whitman’s Lexical Comparisons” (pp. 94–240). In support of his view that the KJ theory must be rejected, Vovin presents a highly detailed linguistic examination of the lexicon (148 pages for “lexical comparisons” versus 46 pages for “morphological comparisons”). Basing himself on his own reconstruction of Japanese-Ryukyuan (for him evidently a nested chronological-areal set comprising Old Japanese, Proto-Japanese [based mainly on Old Western Japanese and the Azuma dialect of Old Eastern Japanese], and “Proto-Japonic” [his term for Proto-Japanese-Ryukyuan, the ancestor of both Proto-Japanese and Proto-Ryukyuan]) and on early forms of Korean, he painstakingly examines and compares the KJ comparisons of John Whitman, which are accepted by most adherents of the KJ theory. After his examination he concludes – correctly – that the tiny number (only six!) of apparently shared, inherited lexical morphemes is insufficient to demonstrate that the Korean and Japanese languages are related genetically. In fact, of Vovin’s six “reliable Koreo-Japanese cognates”⁴⁰ three are excludable because similar forms are also found in neighboring languages: ‘fire’ (MKor *púl* [phonetically *púr*—CIB],

39 Unger (p. 62), quoting Serafim 2003; cf. Beckwith 2008.

40 I give all Korean and Japanese forms in this sentence verbatim from Vovin, in his notation, but with phonetic equivalents where helpful.

OJpn *pi* ~ **pö*): cf. Indo-European (Tokharian A *por*);⁴¹ ‘hold/take’ (MKor *tùl* [phonetically *tür*—CIB], OJpn *tör*): cf. Koguryo (CJK **taw-* ‘to take’);⁴² and ‘field’ (MKor *mwón* ‘paddy’, OJpn *nô* ‘field’): cf. Koguryo (CJK **nu[nj]* ‘land, earth, field, moor’), and similar Chinese forms.⁴³ These could be shared culture words, old loanwords, or something else, but in any event they are non-distinctive and thus not usable for establishing or disproving language relationship theories.⁴⁴ Of the remaining three, one is the word for ‘crane’, which resembles other bird names that are onomatopoeic in origin; and it is hardly likely that a word for ‘melon’ could have been inherited from a pKJ language spoken in Manchuria, as Vovin rightly remarks (p. 238). That leaves a single valid comparison, ‘to fill [with water]’, MKor *:tam-*, OJpn *tamë-/tamar-* (cf. p. 116). But a language relationship cannot be constructed on the basis of a single morpheme; if not a coincidence, the apparent KJ etymon for ‘to fill’ must be a loanword in at least one language.

Vovin’s argument therefore is convincing, and can be added to other unmentioned arguments made against the KJ theory.⁴⁵ It must be remarked, however, that Vovin is frequently rather dogmatic, abrupt, or arbitrary in his discussion of etymologies (cf. the comments by Unger, pp. 36–37); he often rules against them on the basis of the non-attestation of a relevant form in Eastern Old Japanese, which is extremely skimpily attested,⁴⁶ or in the southernmost languages of the Ryukyus (which are not attested until early modern times and were presumably more exposed to Austronesian or other languages, so they were subject to lexical loss by convergence), or because of a discrepancy in register. The last of these is a particularly weak argument, as register is a suprasegmental feature of uncertain prehistory and still has many unexplained dialectal and other variations.⁴⁷ A more consistent, rigorous presentation of the precise phonological congruities or lack thereof, which he does indeed present in some detail in many instances, would be more convincing. Nevertheless, Vovin’s etymological arguments – helped immeasurably by the stunning unbelievability of many of the KJ proponents’ comparisons to begin with – are on the whole correct.

Vovin sums up his view (p. 239),

The loanwords that constitute the major portion of the Koreo-Japonic hypothesis are mostly attested in Central Japanese. Most of them represent comparatively late loans from Old Korean into Central Japanese and were probably borrowed between the late fourth and the late seventh centuries AD. A few of them are possibly much older, since they are present in all branches of

41 Adams 1999, pp. 392–93.

42 Beckwith 2007a, p. 137.

43 Beckwith 2007a, pp. 132–34.

44 See Beckwith 2008 on non-distinctiveness. For Old Chinese loanwords in Japanese see Kiyose and Beckwith 2008 and Beckwith 2007a; for those in Korean see Beckwith forthcoming-a.

45 e.g., Beckwith 2007a, pp. 164–83.

46 Unger (p. 95) notes, “The amount of text we have for all of Eastern Old Japanese is meager, and most of it is poetry. That a certain word happens *not* to appear in the corpus tells us little...; such an absence could be purely fortuitous.”

47 See the glossary sections of Martin 1987 for examples.

Japonic [i.e., Japanese-Ryukyuan – CIB] . . . Japonic was strongly influenced by Korean. . . This Koreanization was strongest in Western Old Japanese, where we can see not only a large amount of lexical borrowing that resulted in doublets,⁴⁸ but also significant borrowing of morphological markers from Korean into Western Old Japanese.

Vovin’s book constitutes a major, sustained attack on the Korean–Japanese genetic relationship proposal, and essentially succeeds in its goal of demolishing that theory.

In his bibliography, Vovin cites three works by Unger, of which two are papers published in 1990, and the third is the second edition (1993) of Unger’s doctoral dissertation. No work by me is cited (except for the Acknowledgments page, my name is not mentioned in the book). Although Vovin cites twenty-two works by himself, he thus omits my book, as well as two articles that discuss some of his work;⁴⁹ of the latter two, one is published in the same journal issue as Vovin’s article, in which he does cite my book. The points made in my articles, and most of the content of my book, are ignored by Vovin. See the comments on Unger’s bibliography above. Like Unger’s book, Vovin’s has no general index, but only an index of words cited.



Although the new books by Unger and Vovin thus differ with regard to the specific issue of the KJ theory, on one issue – the most crucial single issue in the early ethnolinguistic history of Korea and Japan, at least from the point of view of the data – they nevertheless share essentially the same view. This is the question of the non-Chinese toponyms from the former Koguryo Kingdom, which are nearly all in a language related to Japanese.⁵⁰ Unger presents his views about this issue fairly clearly, asserting (pp. 81–82) that “the ‘Old Koguryō’ place names are actually in a language different from Koguryōan and similar to Early Old Japanese . . . [, that] Koguryōan was just a variety of Old Korean[,] and that the place-names merely preserve the vestiges of a dying para-Japanese.” Vovin has scattered remarks on the issue throughout his book, and at the very end (p. 239) agrees with Unger that a language closely related to Japanese was spoken in the central Korean Peninsula *before* the immigration of the Puyo-Koguryo peoples, who spoke *Korean* and imposed their language on the entire peninsula. This shared view is based partly on ignoring or misinterpreting *the only data* we have, and partly on sheer speculation. But the scientific method requires logical, methodologically rigorous analysis of hypotheses *based on the data*. Theories that are confirmed by the data are accepted as working theories or paradigms; if they are not confirmed or are clearly falsified by the data, they are rejected; they cannot be retained “anyway”.

48 He gives a table (Chart 28) of such doublets on p. 93. They constitute a strong argument in favor of his theory of a Korean influence on “Central Japanese” or “Western Old Japanese” during the Kofun-Asuka period (pp. 239–40).

49 Beckwith 2007a, 2005a, 2006a.

50 i.e., they form the majority of the corpus of Old Koguryo morphological material, q.v. Beckwith 2007a.

Unger (pp. 150–51 n. 5) approvingly refers the reader to “Vovin’s (2007)⁵¹ interpretation of Chinese remarks on certain Chinhan words as attempts at transcribing para-Japanese.” But what about the data? Vovin does indeed argue that the language of Chinhan was a “Japonic” language, yet the Chinese text Vovin quotes, the *Sanguozhi* 三國志, actually claims that the language of the people of Chinhan was *Chinese* in origin, and cites in support of its claim a small number of ordinary *Chinese* words used in Chinhan in place of other, synonymous, ordinary *Chinese* words. This is crystal clear, and fits the outlook of the text in general. Vovin rejects what the text says, taking the words out of context and attempting to create “Japonic” etymologies for them. But the text’s descriptions of Chinhan – for example, the way the people greeted one another – can only be understood correctly by comparing them to the same text’s deliberate, closely parallel descriptions of the other Korean Peninsula peoples’ corresponding customs. The text is absolutely clear and there are no “Japonic” words involved. A brief examination of Vovin’s one seemingly strong example will have to suffice. He claims that the first person pronoun *a [阿], which is mentioned in the *Sanguozhi* as an “eastern” Chinese word, “does not have any Chinese etymology”, whereas it is attested in Old Japanese as *a*, so it must be a Japanese-Ryukyuan word. But it actually *is* an “eastern” (Wu “dialect”) Chinese form, still used even today – cf. Shanghainese *a-la* 阿拉 ‘we’ (vs. *i-la* 伊拉 ‘they’) – and thus surely just another example of the Chinese dialect spoken by the local Chinese of Chinhan, who undoubtedly are the ones who supplied the visiting chroniclers with their own origin stories as well. It should not be necessary to point out that a word consisting of a single segment, and a pronoun as well, is hardly an argument in the East Asian context. It is certainly possible – indeed I have argued it is *probable* – that the Koreanic language of the Chinhan (later Silla) region was strongly *influenced* by the Yayoi speakers of Proto-Japanese, but there is no evidence that the Chinhan language was genetically “Japonic”. Vovin’s article thus does not provide any support for his or Unger’s speculations.

Returning to the issue of Old Koguryo, Vovin (pp. 24–25) asserts that “Central Korean sits on top of a Japonic substratum, namely, it is located in the same area as the so-called Koguryo place names recorded in the *Samkwuk saki*,” and he discusses a few examples. Unfortunately, his apparent failure to read my book on Koguryo carefully enough to grasp my analysis of the data and reconstruction of the transcription system has resulted in numerous mistaken judgments about forms from that language and from the other languages recorded in the *Samguk sagi*. For example, the reconstruction of Old Koguryo (OKog) **tar* [達] ‘mountain; high’ is not based on “Northwestern Chinese” per se (though it agrees with it); nor is it based on the modern “Sino-Korean reading *tal*” (though in this case too it agrees with it); and it is most certainly not “likely to be just **tat*” (Vovin, pp. 114–15). The value of the final consonant is firmly established on the basis of both internal evidence (multiple differing Chinese-character transcriptions of Koguryo words) and external evidence (early Chinese transcriptions of other languages,⁵² as well as Chinese loanwords and transcriptions of Chinese words in non-Chinese languages).

51 The bibliography of this article also includes a citation of my book (Beckwith 2004a).

52 Beckwith 2005b.

The fundamental problem both Unger and Vovin continue to overlook is that, with one or two exceptions due to the Puyo-Koguryo peoples having overrun most of the region during their initial expansion, there are *no Japanese-related toponyms from the southeastern part of the Korean Peninsula*; they are found, instead, from the northerly area of Liaotung and southern Manchuria south into central Korea and, partly, in the Paekche area. Correspondingly, *in the central region there are no Korean toponyms* – again, with a few probable exceptions⁵³ due to the spread of Korean in the period between the Silla conquest and the recording of the old toponyms. And *in the northerly region there are no Korean toponyms at all*. The Puyo-Koguryo morphemes (Unger’s “para-Japanese”) are found in the area of the former Puyo-Koguryo kingdoms of Koguryo, Yemaek (early conquered by Koguryo), and to some extent Paekche, agreeing with the known history of those kingdoms. By contrast, *the Korean toponyms are found only in the southern Korean Peninsula, especially the southeastern part (Silla)* – precisely the region Unger and Vovin claim was Proto-Japanese speaking – and to some extent also in Paekche. These simple facts falsify Unger and Vovin’s assertions concerning the identity of the Japanese-related language recorded in sources on the Korean Peninsula region.

The solution to the problems presented by both Unger’s and Vovin’s untenable scenarios for the ethnolinguistic history of early Korea and Japan is simple and conforms to the linguistic, archaeological, and historical data. The Yayoi people, who are acknowledged to be the Proto-Japanese, settled in southern Korea and northern Kyushu, where they introduced their advanced culture characterized by intensive, widespread wet-field rice agriculture. While they had a powerful impact on the less advanced Jōmon peoples in Japan, and replaced them and their languages with little mixing, in the southern Korean Peninsula the autochthonous native people were relatively more advanced; the Yayoi had a strong influence on them, but did not replace them or their language, which there is no reason to think was not Korean. This accounts for the presence throughout Japanese-Ryukyuan of some morphological elements also found in Korean – but rather than borrowings in Proto-Japanese-Ryukyuan from Proto-Korean, as Vovin argues, they are undoubtedly Proto-Japanese-Ryukyuan elements borrowed by Proto-Korean (a possibility that Vovin suggests alternatively on p. 239). Later, the Puyo-Koguryo peoples, who spoke Common Puyo-Koguryo, a language related to Japanese, entered the Korean Peninsula from the north and used their language for many toponyms in the territory ruled by them, which did *not* include Silla. Still later, the conquest of Puyo-Koguryo territory by the Korean-speaking Silla people introduced some Puyo-Koguryo words into Korean (as was proposed long ago by Lee Ki-moon and is now generally accepted). Subsequently, immigrants to Japan from the Korean Peninsula in the Kofun-Asuka period introduced numerous loanwords into pre-Old Japanese and early Old Japanese. About a century after the Silla conquest, the non-Chinese toponyms of the former Koguryo, Paekche, and Silla kingdoms were all changed into Chinese names by order of the Silla king, and the record was made that underlies the account of toponyms in the *Samguk sagi*.⁵⁴

53 See Chapters 3 and 8 of Beckwith 2007a.

54 Though not as richly covered as Koguryo, the two languages of Paekche brilliantly identified by Kōno Rokurō (1987) are also attested in the *Samguk sagi* – namely Puyo-Paekche (a Puyo-Koguryo language), and Han-Paekche (a Han 韓, or Korean, language); cf. Beckwith 2007a. The same source includes some glossed

CONCLUSION

Scientific work must take into account all of the data, and all major studies based on the data. In order to make any progress in the study of the ancient to early medieval languages of the Korean Peninsula, however, reconstructions must be based on a realistic *linguistic* reconstruction of the Chinese transcriptional language, the foundations of which have been laid through study of the richest corpus of material, the Old Koguryo toponyms in the *Samguk sagi*.⁵⁵ Supposed “reconstructions” based on Sinologists’ continuing fascination with medieval Chinese non-linguistic approaches to the problem of Old and Middle Chinese phonology do not work for the languages of the Korean Peninsula, as the *Samguk sagi* transcriptions reveal, nor do they work even for Chinese.⁵⁶ It is actually necessary to use modern science. Until specialists in East Asian linguistics come to grips with that fact, the field will continue to be dominated by speculation and worse. I hope that scholars will reject empty polemics, overcome the pernicious traditions that have long hampered the development of so many sub-fields of East Asian studies, and finally impose the rule of science – at least in philology, comparative-historical linguistics, and linguistic typology.

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Silla toponyms. Unfortunately, scholars have not yet studied the sources philologically and eliminated the false readings, ghostwords, and other unusable material in these parts of the corpus.

- 55 Beckwith 2007a. Unger (pp. 48–49) rightly criticizes some recent attempts at redoing the reconstruction of the transcriptional language used for Old Japanese for having overinterpreted the often highly ambiguous data, noting, “a simpler explanation is that Chinese transcriptions ought not [to] be interpreted in this way”.
- 56 Beckwith 2004b.

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