When was Korean First Spoken in Southeastern Korea?

J. Marshall Unger, The Ohio State University

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The weakest assumption about the prehistory of Korea and Japan is that proto-Korean was spoken in the southeastern peninsula from time immemorial. The Yayoi migrants departed for Japan from there, and much evidence shows that proto-Japanese was their language. Yet comparison of Korean and Japanese does not indicate such a late separation of proto-languages. Also, the so-called Koguryŏan placenames are better interpreted as showing that a form of Japanese was once spoken on the peninsula I hypothesize that this language was related to Korean but had been separated from it for many centuries, and been relexified by the language(s) accompanying wet-field rice and other features of Mumun culture that diffused from the southern peninsula during the second millennium BCE. If the precursor of Japanese had itself been one of those languages, it is hard to explain Korean-Japanese syntactic parallels, which would have required intensive, long-term contact, for there is no archaeological evidence of a distinctively Sillan culture in the south prior to the 4th century CE.

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People seem to have spoken Korean throughout the peninsula at least since it was unified by Silla in the 7th century, and it is therefore widely believed that a form of Korean must have been spoken in the southeastern corner of the peninsula, where Silla was originally located, from at least Mumun times. I want to question this belief because, of all the many assumptions commonly made by historians and linguists, it is the most poorly supported and the one that most complicates our understanding of the relationship between the Korean and Japanese languages. For the past several years, I have been studying the Korean-Japanese relationship in the light of recent archaeological and anthropological research as well as work in historical linguistics, and believe that, whether or not Korean and Japanese turn out to be genetically related languages, proto-Korean was probably not spoken in the Yŏngnam area when the Yayoi migration to Japan began. Consequently, I doubt that there was a Koguryŏan language distinct from Korean. Although Koguryŏan, Paekchean, and Sillan could have been distinct languages, a better hypothesis at present is that they were all just varieties of Old Korean, which did not penetrate the southeast until sometime after 300 CE.

Let me begin by noting briefly some pertinent NON-LINGUISTIC facts about Sillan culture. First, "[t]he archaeology of the Late Iron Age (0-300 CE) of the Kyŏngju basin is virtually indistinguishable from the rest of the Yŏngnam region (southeastern Korea). This means that on the basis of material culture, we cannot see a major distinction between the areas that

later become Kaya and Silla" (Barnes 2004:16). Second, "it is impossible to speak of a 'Silla' before the manifestation of their physical means of identification.... The fourth century was the time period in which the infrastructure for the Silla state was laid, with Silla material and ethnic identity fully formalized in the fifth century" (Barnes 2004:36). Barnes's assessment is supported, for example, by parallels between shamanistic practices in Silla, which persisted even after the official adoption of Buddhism, and ethnographic information about the kingdoms of Puyŏ and Paekche (Lee 2004:50-54), as well as by a comparison of early historical records showing Silla's precarious position between the competing interests of Yamato and Koguryŏ in the late 4th and early 5th centuries (Allen 2004).

If we look for LINGUISTIC evidence that requires placing Korean in the Yŏngnam area centuries before the kingdom itself appears, we simply don't find any. It may be true that Sillan royalty spoke an early form of Old Korean, but there is no guarantee that a different language was used in the region before the late 4th century. Apart from the Chinese word list *Kyerim yusa* of the Koryŏ period, extended texts in Old Korean comprise just the twenty-five *hyangga*, the oldest of which are the fourteen recorded in the 12th-century *Samguk yusa*. Of those fourteen, thirteen are supposed to have been written by Sillans, but one was attributed to a prince of Paekche, and all were composed in the 7th century or later, most during the 8th. The other eleven hyangga are found in the mid-10th-century biography of the priest Kyunyŏ. As Lee and Ramsey (2000:48-49) state, "Interpretation of these short poems is not an easy task. Mysteries abound, and much remains undeciphered." Moreover, we need to be cautious when drawing inferences about Old Korean from Middle Korean documents of the 15th century. It is possible that, with a better understanding of *kugyŏl* reading and writing practices of the pre-*han'gŭl* period, we may discover that the latter was somewhat aberrant with respect to the main lines of dialect development. Recently discovered evidence of inkless interlinear notations in Late Old and Early Middle Korean manuscripts (Kobayashi 2004) have yet to be adequately analyzed and interpreted linguistically.

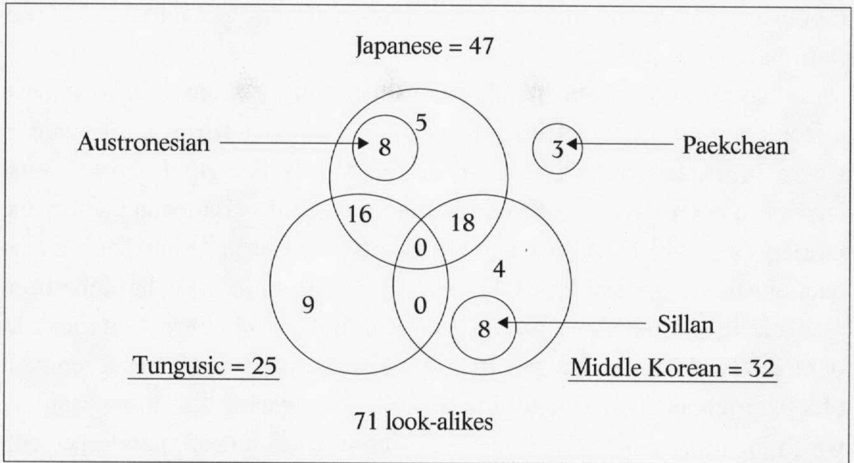
At any rate, whether Old Korean was the original language of the Silla kingdom is a separate matter. Fukui (2003:223-24) is, to my knowledge, the

only linguist who has raised it explicitly, and he offers just one argument to support the identification. (When I spoke with him last March, he told me it was his ONLY argument.) In passages in *Nihon shoki* referring to the peninsula, OJ *kwi* 'fortress' is commonly associated with Paekche; a synonym, OJ *sasi*, seems to be cognate with MK *cas* 'fortress', which is used in reference to places in Silla. The so-called Koguryŏan word for 'fortress', written with the character 忽, was *hol, as Fukui reconstructs it. Since OJ *sasi* resembles neither *hol (which has been compared with OJ *kura* 'storehouse') nor OJ *kwi* (which Yun 1994 argues derives from the same source as *hol), Fukui reasons that MK *cas* must go back to a Sillan word. But this assumes that Koguryŏan and Paekchean were distinct languages from Sillan, which is precisely the point in contention. Indeed, OJ *sasi* may be a borrowing rather than a cognate. This is certainly the impression one gets from the placename *musasi* < OJ *muzasi* < *mu(.)nV.sasi, which obviously contains the morpheme *sasi* and has been written with the *ateji* 武藏 since the Nara period. The characters here imply that OJ *kura* 'storehouse' (the usual gloss on 藏) was thought of as a synonym of *sasi*. Musashi was a frontier area where, according to *Nihon shoki*, immigrants from Paekche as well as Silla were settled at the end of the 7th century, and the name may have been coined around that time by peninsular settlers who knew the corresponding Old Korean word. (The Ainu word *casi* 'fortress' may be a by-product of this innovation, though Vovin (1993) thinks the resemblance in form and meaning is accidental.)

Linguistic evidence thus does not help us determine when the Korean language arrived in the southeastern peninsula, and despite traditional Korean legends implying great antiquity, the preponderance of the non-linguistic evidence suggests that a language like Japanese was spoken in the area at least as until the 3rd century BCE, and perhaps for quite a while afterward. It is even conceivable that the founders of Silla originally spoke a para-Japonic language (to use Juha Janhunen's term) but gave it up as their domain expanded, much as the Franks gradually abandoned Frankish for the somewhat oddly named lingua franca of their Romance-speaking subjects. But even if Korean was always the language of Sillan royalty, which is certainly possible, it does not follow that Korean was the dominant language of

the Yŏngnam area before the 4th century. On the contrary, the Yayoi migration theory of Japanese linguistic origins implies that para-Japonic was the dominant language of the area at an earlier time. One cannot meaningfully discuss the origins of Korean or Japanese without taking the origins of the other into account.

I won't attempt to summarize the archaeological and biological evidence that Yayoi culture was brought to Japan by migrants, but instead state what I think are the three most important linguistic arguments for that hypothesis. First, if Japanese had been spoken in Japan much earlier, we would expect to find greater linguistic diversity there than we actually do (Hudson 1999:92, Whitman 2002:260). For later reference, note also that the high degree of homogeneity of Japanese dialects means that substratal contributions of pre-Yayoi language could only have occurred during the short period before proto-Japanese began to spread out of northern Kyūshū and split into dialects. Second, to the extent that Ainu can be taken as representative of languages spoken in Japan during the Final Jōmon period, even a severe critic of Martin 1966 and Whitman 1985 would probably agree that there are more lexical matches between Japanese and Korean than between Japanese and some putative Jōmon language (cf. Patrie 1982). Interest has recently revived for finding Ainu etymologies for names that seem meaningless in Old Japanese or are written with *ateji*, a search earlier pursued by such scholars as Basil Hall Chamberlain and Murayama Shichirō. Without passing judgment on new work along these lines by avocational researchers like Nagata (2001, 2005) and Ōyama (2002, 2003), it is worth noting that they implicitly agree that Japanese was not itself a Jōmon language. Third and finally, whether one thinks Japanese proper was related to Koguryŏan, to Korean, or to neither, the well-known *Samguk sagi* placename data show that Japanese-like words were once used on the peninsula. Despite clear signs of trade throughout the Jōmon period between the islands and peninsula, there is no evidence of permanent habitation by Jōmon type people on the continent (Nakahashi 2005:122, 202-206; Mori 2005), and it is doubtful trade contacts alone could have introduced words that found their way into local placenames. Para-Japonic evidently spread from—not to—Korea, and such a diffusion is easier to explain as a result of than as a precursor to the



115 Koguryŏan words(including 4 from *Hou Han shu*)

Yayoi migration.

Many scholars, myself included, have assumed that non-Korean morphemes recovered from the *Samguk sagi* chapters headed "Koguryŏ" and "Paekche" are actually fragments of distinct Koguryŏan and Paekchean languages, though Kim Panghan explicitly warned against making that assumption. Beckwith (2004) has gone much further, arguing that those languages were genetically unrelated to Korean but came from the same source as Japanese. I am now, however, inclined to agree with Kim. The distribution of tentative etymologies for the placename morphemes in Itabashi's (2003:189) analysis of essentially the same data discussed by Beckwith supports Kim's view, regardless of what one thinks of the inferences Itabashi draws from it. As the diagram above makes clear, overlapping etymologies are few compared with the number of Japanese, Tungusic, and Korean words that, respectively, resemble so-called Koguryŏan words.

Whitman summarized the situation aptly when he wrote (2002:263), "[T]he Japanese material looks like Japanese, the Korean material looks like Korean, et cetera. Furthermore, none of the decipherable toponyms refer[s] to the historical homeland of Koguryŏ north of the Yālü river, and none of the toponyms associated with Japanese designate localities north of the Taedong river; most are south of the Han river basin. Some 'Japanese'

toponyms (such as the number 'three' [*mil]) are also found in the Silla sections of the *Samguk sagi*."

I hasten to point out that this assessment of the *Samguk sagi* evidence undercuts not only the claims of Beckwith 2004 but also some I made in Unger 2001 and Unger 2003. I argued that words like OJ *mi-* 'three' must have been borrowed from Koguryŏan in light of OJ *sakikusa*, a plant name written 三枝, which, as Whitman (1985) observed, seems to preserve a cognate of MK *še(k) ~ šey(h)* < OK *seki (?~ *saki) 'three'. As has long been recognized, one can reconstruct Koguryŏan *mil or *mit 'three' on the basis of doublet writings in *Samguk sagi*. If, however, this form is just a remnant of a para-Japonic language (not necessarily Koguryŏan), this interpretation, which in any case has been criticized by both Serafim (2003) and Beckwith for unrelated reasons, must be abandoned. One must say instead that OJ *mi-* is a native morpheme and that the *saki* 'three' of *sakikusa* is not cognate but rather a borrowing from Korean, even if Korean and Japanese diverged from a common source. (Otherwise, Japanese would unnaturally have had a pair of exact native synonyms for the cardinal number 'three'.)

This underscores the point I made earlier about the interdependence of hypotheses regarding the the origins of Korean and Japanese. Whatever the reasons for the lexical similarities between Korean and Japanese may be, pure chance is the least likely. If the languages diverged from a common source, then our slow progress in sharpening the phonemic correspondences in the proto-Korean-Japanese reconstruction, difficulty in reducing the unmatched residues in both languages, and inability to find cognates among words associated with wet-field cultivation and metallurgy (Rozycki 2003:454-55, Blench 2005:44) all imply that the two languages separated much earlier than the Yayoi migration. On the other hand, if Korean and Japanese were not genetically related, then Korean cannot have been the sole language of the Yŏngnam area from time immemorial, because the archaeological evidence points to that area as the place from which the Yayoi migrants embarked. Indeed, if Japanese and Koguryŏan had a special relationship not shared by Sillan or Korean, as Beckwith thinks, then what Hudson (1999:97) calls a "geographical inversion" problem arises. The capitals of Koguryŏ and Silla are in the wrong ends of the peninsula with respect

to the most likely starting point of the Yayoi migration.

Beckwith addresses this problem by arguing that Korean speakers of pre-Silla Chinhan were enveloped by Koguryŏan-Japanese speakers who lived in Liáoxī sometime before 400 BCE. He is not sure whether "the ancestors of the Yemaek state of eastern Korea ... moved by land to Liádōng and Korea with Wiman Chosŏn" of legendary fame, but he states that "at about the same time ... some did move by sea to the southern tip of the Korean Peninsula and to northern Kyūshū; these were the Yayoi or Wa, the ancestors of the Japanese." "Others," he continues, "eventually moved overland into southern Manchuria to found the Puyŏ kingdom. Still later, the Koguryŏ and other Puyŏ-Koguryŏic peoples also moved by land into Liáodōng, southern Manchuria, and Korea. The latest to move were a group of Wa, who migrated by sea to the Ryūkyūs at around that time, and the Puyŏ-Paekche, who conquered the Mahan area of Korea (the western south-central region, focused on the area of modern Sŏul) in the mid-fourth century" (Beckwith 2004:36-37).

Though some parts of this scenario are supported by Chinese historical notices, I find it hard to accept it as a whole for several reasons. Beckwith (2004, 29-32) makes much of the similarities among the progenitor myths of Koguryŏ, Puyŏ, and Paekche, which he thinks have echoes in Japanese myths, but fails to acknowledge that the trope of being born from an egg is applied not only to Tongmyŏng (or Chumong) of Koguryŏ but also to Hyŏkkŏse of the Pak clan, T 'arhae of the Sŏk clan, and Alchi of the Kim clan of Silla (Lee 2004:57). The Sillan versions mention a character named Ho-gong, who is said to come from Wa, and the myth of T 'arhae makes even more detailed references to Japan (Lee 2004:58-59), which is odd if Koguryŏan-Japanese and Sillan-Korean culture were fundamentally unrelated. I also think Beckwith's reliance on Chinese ethnographic identifications of Korean Yemaek with later Koguryŏ is unwise. As Byington explains (2003:55-58), the Ye or Huī (穢) "do not seem to have been known in China until after Yān's occupation of Liáodōng around 282 B.C.[E.]" while the Maek or Mò (貊 or 貉) were "located to the west or southwest of Yān" (55-57), i.e. in western Hèběi. Neither group was in Liáoxī at the time Beckwith places them there, and neither was connected with the other or

with the Huìmò, that is Yemaek, of Korea (ibid.). Chinese historians used old names to refer to new peoples on their expanding frontiers, as shown by changes in the denotation of such terms as Dōngyí 東夷, Sùshèn 肅愼, and Hàn 韓. The Dōngyí of Zhōu times lived in Shāndōng and points farther south, but in Hàn times in Manchuria and Korea (Byington 2003:57-58). The Sùshèn of Hàn times were the Yìlóu 挹婁 of Manchuria, who lived far to the east of the original group (Byington 2003:61-62), and the Hàn of the Warring States period lived in what is now Hénán, though the same name was, of course, later used for southern Korea. Even the Japanese got into the act, glossing the characters for Sùshèn *misipase* (perhaps related to *emisi* 'northern rustics') to name a strange group of visitors to Sado Island, who could just as easily have been Gilyak as Tungus.

There are also many linguistic problems with Beckwith's argument, a full discussion of which would take us far beyond the topic at hand. To mention just one item related to the previous discussion, if Ye and Maek were recycled names for two distinct groups of earlier people, then Beckwith's reconstructed Chinese form *kormak 穢貊 (2004:50 n2) was probably not an attempt at transcribing the actual ethnonym used by people living in Yemaek territory. Hence, Beckwith's tempting comparison of OJ *koma* 'Koguryŏ' with 穢貊 may be mistaken. Furthermore, Japanese words thought to be ancient borrowings from Late Old or Early Middle Chinese retain Chinese final obstruents (e.g. *niku* 'flesh' , *pude* 'writing brush'), whereas *koma,* if from *kormak or *kotmak, does not. At any rate, OJ *koma* meant 'Koguryŏ' , not 'Yemaek' . It is possible that the meaning changed after Koguryŏ gained control over Yemaek territory, but Beckwith thinks the connection was due to ethnic links between Yemaek and Koguryŏ that Japanese knew firsthand. One could try to save Beckwith' s etymology by arguing that 百済 was a simplified writing of 貊済; since百済 in one *Nihon shoki* passage is associated with OJ *kumanari,* which can be analyzed as K *kom* 'bear' (cf. J *kuma* id.) and K *nay* < *nari 'river' , we can perhaps equate 貊 with 'bear' . In fact, the capital of Paekche from 475 to 538 was Ungjin 熊津 'bear port' , and was moved in 538 to Sabi 泗沘 on the the Kūm (?< *kuma) river. This justifies taking 貊 or 穢貊 as a Chinese gloss on a word meaning 'bear' ; if it were the totem animal of both the Yemaek and

Paekche people, it might have been important also to their ethnic cousins, the Koguryŏans. But all this is impossible under Beckwith's theory, which denies any genetic connection between Korean and the "Koguryŏic-Japanese" languages, for the only reason to link OJ *nari* in *kumanari* with 'river' is K *nay* < *nari 'river'. Under what circumstances could Korean have loaned its word for 'river' to surrounding "Koguryŏic-Japanese" languages at the same time it borrowed its word for 'bear' from them? Furthermore, the name 百済 or 伯済 was in use for the southwest quadrant of the peninsula long before the kingdom of Paekche was established (Kirkland 1983). Although the resemblance between OJ *kudara* 'Paekche' and *kumanari* has been often remarked upon, there are more than a dozen different theories about the origin of the name *kudara*, some of the best of which dispense with 'bear' morphemes entirely (Anselmo 1974:23-48).

Archaeological evidence also fails to support Beckwith's association of Yemaek as precursors of Koguryŏ. As Sarah Nelson has written,

> A common Korean interpretation of the time period after about 1000 BCE is that a nomadic group, sometimes specified as the Yemaek ... entered the peninsula from the north, bringing new pottery styles and rice agriculture with the stone tool technology to carry it out. In this interpretation the Yemaek also buried their dead in stone cists and erected enormous dolmens to mark their chiefs.... This is a simplistic argument based loosely on historical sources .... The archaeological data cannot so easily be fitted into a Yemaek pigeonhole, and as Manchurian archaeology becomes better known, the mosaic becomes more complex, not simpler. Many of the sites of Liáoníng, Jílín, and Hēilóngjiāng provinces appear to be those of settled farmers .... The archaeological materials, with the exception of pottery types, do not lend themselves easily to regional treatment.... A relatively consistent assemblage of artifacts in the megalithic period of Korea suggest a similar way of life for ordinary villagers throughout the peninsula.... In this society, bronze artifacts and gemstone beads appear to function as status markers for the elite. Necklaces of tubular beads were often with the dead. Occasionally comma-shaped beads ... which became an important symbol in the Silla kingdom, as well as in Yamato Japan, appear in burials,

> usually in conjunction with tubular beads, perhaps signifying some specific role or status. (Nelson 1993:111-13)

Let me now leave Beckwith's theory to build on Nelson's last comment and outline an alternative. Mori Kōichi has pointed out that tubular beads and *magatama* made of hard jade were produced almost exclusively in the Noto peninsula of northern Japan during most of the Jōmon period and traded by sea over a large area. Contact is also shown by the appearance in Late-Final Jōmon period Japan of upper incisor tooth ablation, which originated two millennia earlier in the southern Shāndōng-northern Jiāngsū area (Han & Nakahashi 1996). This contact was not necessarily a matter of Jōmon people undertaking long voyages, though they may have; more likely, it involved long trade chains with several short-distance links. Wilhelm Solheim has described such a trade network in the East China Sea, which he calls Nusantao. Although the kind of evidence Solheim presents for Nusantao is sometimes cited by critics of the hypothesis that Austronesian spread rapidly from Formosa starting about 6000 years ago (e.g. Oppenheimer & Richards 2001), the idea of a Nusantao trade network is in no way incompatible with that hypothesis, and should be researched independently of the question of Austronesian origins, without making an hasty guesses as to the language or languages used within the network. The fact that, by the 3rd century BCE, sea trade extended from China as far west as Poompuhar in the Paṇḍya kingdom of southern India indicates that network, even if composed of short links, covered vast distances. The seafaring Dông Son people of northern Vietnam, whose advanced bronze work spread throughout Southeast Asia during the 1st millennium BCE and who were physically similar to the Yayoi migrants (Matsumura & Hudson 2005), no doubt played a role in it. Although research in Ryūkyū archaeology does not support a direct southern route for the entry of wet-field rice techniques into Japan (Takamiya 2001), it is possible that network travel between coastal Jiāngsū and southern Korea preceded the later Yayoi migrants from there to northern Kyūshū (Solheim 2000:4-5).

This version of Solheim's hypothesis bears a superficial resemblance to Beckwith's idea that his Liáoxī group originated in a Tibeto-Burman lan-

guage area of ancient southern China, but differs significantly on matters of geography, chronology, and language. It is also more in keeping with Nelson's emphasis on multiple inputs to the wet-field rice culture of Mumun Korea. Others have noted that the Yayoi migration must have involved multiple settlements, if only because evidence of warfare in Japan is found only from the Middle Yayoi onward. Although areas north of Shāndōng may have played a role in the Nusantao network, it is noteworthy that the closest match for Yayoi bones found on the Chinese coast so far (Nakahashi 2005) are from southern Jiāngsū. Recent research on the spread of wet-field rice also points to the Yángzī delta as a center of diffusion (Fuminori 1998). Links to the kingdoms of Wú and Yuè may perhaps also be inferred from the inclusion of two or three dozen mirrors in Japanese burials starting with the Mikumo Minami Kōji Tomb near Fukuoka (1st c. BCE). This practice was thought to be unique to Japan until the 1983 discovery of 38 mirrors in the tomb of the second king of Nán Yuè in Guǎngzhōu (Mori 1993:88, 91; 2005:146). There may also be parallels between folk practices and myths in the old Yuè area of China and Yayoi Japan (Kudō 1999, Suwa 2005).

Notice that, under the Nusantao middle-route hypothesis for the source of the Mumun and Yayoi cultures, Korean and Japanese cannot just be two continuations of a single language of the western and southern shores of ancient Korea that had been greatly influenced by Nusantao speech. Just as with the classical divergence hypothesis, the comparative evidence is too weak to justify taking the Yayoi migration itself as the cause of the extended period of physical separation, without which distinct Korean and Japanese could not have developed. If Korean and Japanese were genetically related, lexification by the Nusantao traders could only have affected pre-Japanese of the southern peninsula while it was still out of touch with pre-Korean. On the other hand, if Korean and Japanese were not genetically related, then the language of the traders must itself have been pre-Japanese; contact and interaction with Korean could only have occurred after the Yayoi migrations. In either case, Korean had to be a relatively late arrival in the Yŏngnam area.

It might seem that bringing a third language into the picture would favor explaining Korean-Japanese linguistic similarities purely in terms of

borrowings, but a genetic relationship is still conceivable. As just remarked, Nusantao speech could have been served as a lexifier of pre-Japanese before it resumed contact with Korean. (Indeed, for reasons I will not go into here, I suspect this is the more likely alternative.) If the language of the Yayoi migrants were a low mesolect or basilect of the resulting creole, it would be easy to explain not only the smallish residue of Korean-Japanese lexical cognates but also the large number of syntactic parallels. Note also that hypothesizing a relexification of pre-Japanese before it displaced pre-Yayoi languages is much more realistic than seeing it as a variety of Korean that picked up many pre-Yayoi words after crossing to Japan (e.g. Maher 2004). Not only was the opportunity for substratal Jōmon impact on Japanese limited (as noted above), but also, if Korean and Japanese were genetically related, Korean could not have resumed contact with Japanese until long after the creolization that preceded the Yayoi migration.

Nevertheless, even if the divergence hypothesis holds, the borrowing of words like OJ sakikusa needs to be explained. On this point, Beckwith makes the valuable suggestion that Kofun culture was brought to the islands, not by northern horseriders (Ledyard 1975) or invading Paekcheans (Hong 1994), but rather by Japonic speakers who had "learned how to fight continental style, using horses, armor, the latest weapons, and so forth, in order to survive" (Beckwith 2004:23). Unlike Beckwith, I do not see these as Japanese veterans toughened by unsuccessful campaigns against Koguryŏ, but rather as para-Japonic speakers, born on the peninsula, who survived the conflicts that created the states of Paekche and Silla as well as Koguryŏ, learned the ways of their conquerors through hard experience, and absconded to the islands, where they used their skills and knowledge to gain security, prosper, and ultimately gain power. Note especially that this interpretation is much more in keeping with the chronology of Korean-Japanese interactions described by Ledyard, even if one rejects his identification of Emperor Ōjin and his successors as Puyŏ leaders.

To repeat one last time, the early histories of the Japanese and Korean languages are intertwined. Para-Japonic was either a creolized form of a related language long separated from Korean or an unrelated language with roots in the Jiāngnán area of China. In either case, we should not imagine

that Sillan was spoken in Yŏngnam from ancient times, that it was the exclusive source of Old Korean, or that the kingdom of Silla wiped out the languages as well as the states of Paekche and Koguryŏ. It is not impossible that Paekchean and Koguryŏan were distinct languages from Sillan, but considering how quickly the peninsula became monoglossic, I see no reason to think they were in the absence of a credible case relating one or more of them to some non-peninsular language. For that purpose, Japanese is not a candidate since a form of it was no doubt spoken on the peninsula. Hence, for the time being, I prefer to think of Koguryŏan, Paekchean, and Sillan as three early varieties of Korean that displaced residual Chinese and para-Japonic speech in a north-to-south advance, reaching the southeastern corner of the peninsula in the turbulent 4th century. Whether Korean, so understood, had some affiliation with Tungusic, Mongolic, or other SOV languages is unclear, but thinking of Korean as a successful intruder rather than as a survivor in situ from neolithic times should make it easier to investigate that question and to understand how Japanese got to its present range.

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