

Alexander VOVIN and OSADA Toshiki (eds.): *Nihongo keitôron no genzai. Perspectives on the Origins of the Japanese Language*. Kyoto: International Research Center for Japanese Studies 2003 (Nichibunken Japanese Studies Series, No. 31); 572 pp.

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In the ‘Introduction’ to his *Korean-English Dictionary* J. C. Gale fondly recalled “...intimate friends ... with the remembrance of whom you associate the charms and absurdities of the Far East” (1911, p. v). Those of us who (like the present reviewer) are old enough to be survivors of front-line action in the *Nihongo no keitôron* wars of the 1960s, 1970s, and 1980s will find in this hefty new addition to the academic literature concerning the possible genetic connection (or, connections?) of Japanese with other languages in the world, past or present, much that will immediately bring back memories of intimate friends, and much also with which to remind us, in Gale’s felicitous phrase, of the “charms and absurdities” of the academic communities, both East and West, that were similarly involved. It was, as scholarship goes, not the best of times; but for all that it was not the worst of times either: it was a time when one waited for the new issue of *Gengo Seikatsu* or *Gekkan Gengo* or *Dolmen* to arrive, to learn who in Japan had now proposed what foreign language as the latest if highly unlikely genetic “near relative” to Japanese; and there were the letters, many letters, from such indefatigable observers of (and participants in) the scene as Murayama Shichirô (1908–1995) (incidentally, one of the four scholars to whom this volume is dedicated) – all good friends who could always be relied upon to keep one informed about who was now up to what.

Sometime before the end of the last century, all that changed. The *Nihongo no keitôron bûmu*, like so many trendy ‘booms’ in Japanese society and academia, sank into oblivion almost as suddenly as it had arisen. Like the ‘boom’ itself, the underlying causes for its sudden eclipse mostly remain to be identified and analyzed. That work, if and when it is ever undertaken, will properly fall more nearly into the purview of the sociologist than that of the historical linguist; the phenomena at issue had a great deal more to do with the nature and structure of Japanese society than they did with linguistics. Nevertheless, many of us who still remain interested in these questions have often wondered what happened in Japanese academic circles in connection

with these questions after the ‘boom’ collapsed; the present volume goes a considerable distance toward answering our questions. The answer, it turns out, is that actually nothing much of consequence has happened. But that does not diminish the importance of this volume, which ought to be studied carefully by all historical linguists whose interest in the question of a possible genetic connection of Japanese with other languages has, *mirabile dictu*, survived both the ‘boom’ and the bust that so soon thereafter followed.

The book aims to present to the academic community the results of “an international research committee on the current state of affairs in [*sic*] the research on the genetic affiliation of the Japanese language” (p. 421) that met off and on between April 2001 and March 2002 at the Nichibunken in Kyoto. The committee consisted of fourteen regular members, both Japanese and foreign; in addition, seven “guest speakers” were invited to participate in its deliberations. Interestingly, only two of the Japanese members of the committee listed Japanese as their academic field of specialization; and no one at all admitted to being involved in historical linguistics. Indeed, it appears that it was not easy to persuade a number of Japanese academics even to associate themselves with the work of the committee; as Osada laconically remarks, in Japan today most academics feel that any discussion of the possible genetic affiliation of Japanese with other languages can only “lead to dangerous disputation” (pp. 8–9).

Moreover, as is too often the case with academic meetings, not all the participants who did consent to take part in the work of the committee eventually came through on their promises to submit finished papers when the time came to publish the volume; and as luck would have it, what appear to have been some of the most interesting papers presented at the Nichibunken meetings are as a result not included in this publication. For example, it is especially disappointing that a paper by Takahashi Yoshiharu refuting Nishida Tatsuo’s proposal to link Japanese genetically with Tibetan could not be included. The brief notices of this contribution that the editors share with us (OSADA, p. 5; VOVIN, p. 425) suggest that Takahashi argued in his paper that in actual fact Nishida was only joking when he proposed to link Japanese and Tibetan genetically. Many of us who were there when it all happened will find this difficult to believe (see L. LIGETI, ed., *Proceedings of the Csoma de Körös Memorial Symposium ...* [Budapest, 1978], pp. 295–312). But perhaps Takahashi knows things that we do not; if so, all the more pity that he did not publish his paper.

Nevertheless, enough was salvaged from their year-long marathon of committee meetings to make possible the publication of this volume, no

doubt due in large measure to the energy and industry of Vovin, whom the Nichibunken designated as the enterprise's *hanchô* 班長 'corporal'. (It is interesting to find this table-of-organization term from the old Imperial Army still in use in 21st-century Japan, particularly given what now turns out to be this reviewer's premature announcement of its demise [*The Japanese Language*, 1967, p. 234]; his co-editor Osada, a specialist in Munda [p. 421], was merely designated *kanji* 幹事 'company clerk'.) Vovin not only organized and ran the meetings but he also contributed one of the longest papers in the collection (pp. 15–40); his is also the only paper here that unlike many of the others deals throughout directly with the topic that the Nichibunken committee set for itself, *e.g.*, the problem of identifying possible genetic relationship or relationships of Japanese with other languages.

Over and above what Vovin says about his subject, it is also important to note that at the meetings he read, and here publishes, his contribution "*about Japanese in Japanese*". Osada remarks (p. 10) on the salutary effect that the spectacle of "this Russian-born American discussing Japanese linguistic origins in Japanese" had upon the Japanese members of the Nichibunken committee, prone as many of them were to consider the discussion of such matters a "Japanese monopoly associated with the search for Japanese identity". Additionally, Vovin has contributed an account in English (pp. 421–426), somewhat hidden away in the back of the volume, commenting upon and evaluating in the highest terms possible the many other contributions, all of which he found to be informative and useful, and one of which he even evaluates as "wonderful" (p. 423). As in the Caucus Race in Lewis Carroll's *Alice in Wonderland*, everyone has run, and so everyone must have a prize.

Also somewhat concealed in this back-of-the-book preface is Vovin's brave attempt to sum up the conclusions reached by his committee – surely no easy task given the diversity of the backgrounds of the members of the group and the fact that most of them had virtually no academic background in this question of the genetic status of Japanese. Nevertheless, the corporal is brave enough to venture framing a consensus for his squad: he writes that everyone taking part in the year-long marathon of meetings came to the conclusion that all previous studies and publications that sought to relate Japanese in some way or another to "the other Altaic languages" must be dismissed out of hand, since the "Altaic theory" itself "has too many holes and contradictions with [*sic*] the comparative method" to be tenable; at the same time, however, some members also argued for "a genetic relationship between Japanese and Korean, with a more speculative, but still quite feasible link to Manchu-Tungusic" (p. 426).

One cannot but be grateful to Vovin for the admirable clarity and precision with which he has framed this consensus of views on behalf of his Nichibunken committee. A persistent problem plaguing this field of historical-linguistic scholarship has long been a reluctance to formulate clear, unambiguous statements of the results, real or putative, of our studies. Vovin here, greatly to his credit, has deftly avoided that all too common pitfall. In a word, he and his colleagues in Kyoto now are on record as having found the field in question today to be almost precisely the same as it was *ca.* 1970, following S. E. Martin's 1966 publication arguing for a genetic relationship between Japanese and Korean. Both explicitly and implicitly we are told over and over in this volume that in the past three decades and more nothing has really changed, nothing new has been learned, nothing important has been discovered. This is not an encouraging conclusion, but at least it is clear and unambiguous.

Less clearly set forth here is one important link in the dialectic chain that ties together the argument underlying Vovin's contributions to this volume as well as those few portions by others that actually deal with the topic at hand, namely the enormously influential position taken in the early 1970s by the late Gerhard Doerfer (1920–2003); somewhat surprisingly, he and his demolition of earlier decades of studies in Altaic historical linguistics are not even mentioned anywhere in this volume. Particularly striking, and indeed more than a little puzzling in view of the Nichibunken committee's avowed theme of inquiry, is its passing over Doerfer's influential paper "Ist das Japanische mit den altaischen Sprachen verwandt?" (*ZDMG* 124 [1974] pp. 103–42) in total silence. Nevertheless, and despite the absence from these pages both of Doerfer and of his work on Japanese, his 1974 position on this overall problem is alive and well on almost every page of the book – at least in those contributions that actually discuss the theme of the volume.

Doerfer's 1974 position, much like Vovin's committee-consensus of 2003, had the admirable advantage of directness and clarity, in part due to the fact that in these matters he had followed closely in the footsteps of the British Turkologist Gerard Clauson (1891–1974). Clauson had lost faith in Altaic historical linguistics when he discovered that even though he knew Turkish very well he found that he could not easily read Middle Mongolian ("I tried to read it. I did not begin to understand it. ...so I came to the conclusion that ... the Altaic theory ... was almost certainly wrong", *Turkish and Mongolian Studies* [London, 1962], p. xii). Building upon this epiphany on the one hand and upon the copious lexical citations in his own Göttingen *Habilitationsschrift* (4 vols., 1963 ff.) on the other, Doerfer argued that Japanese could not be genetically related to anything in an original Altaic linguistic unity because

no original Altaic linguistic unity had ever existed. All the apparent “similarities” and “parallels” between Turkic and Mongol on the one hand and “Manchu-Tungus” on the other were due to no more than an elaborate network of lexical borrowing in one direction or another at a fairly recent periods of history. Given the absence of significantly older written records for almost all the languages involved, this scenario of borrowings could neither be demonstrated nor denied on the basis of documentary evidence; but in the absence of the same there was always Clauson’s position to fall back upon, for if these languages were indeed all genetically related, *i.e.*, if they were all later changed forms of an original but now lost earlier proto-language, why then must we spend so much time and effort to learn each of them when we already know one? *Quod erat demonstrandum...*

The seductive simplicity of the Clauson position, especially when fortified by the impressive details of Doerfer’s exhaustive catalogue of putative loanwords in all the languages involved, soon pretty well terminated scholarly interest both in Europe and America in further studying or refining the details of the long-standing hypothesis of the original East Asian linguistic unity that had, for want of a better term, long been known in the literature as Altaic; and that designation, in turn, soon became something that could only be mentioned if enclosed in invidious single or double quotes (as so often in the English-language portions of this volume). Scholarship could now only refer to ‘Altaic’ or “Altaic”, since nothing deserving of the term had ever really existed.

This was more or less the point of departure for those few Nichibunken committee members whose contributions actually touch upon the theme set for the group’s studies (*e.g.*, MATSUMOTO, p. 43 ff.); but Vovin, their leader, took a somewhat different tack. Ignoring Doerfer’s magisterial mandate, he regarded it necessary once more to refute *ab initio* the possibility of treating the genetic connections of Japanese in terms of an original Altaic linguistic unity by directing critical remarks toward selected passages in S. A. STAROSTIN, *Altajskaja problem i proisxoždenie japonskogo jazyka* (Moscow, 1991) – a major contribution apparently so little known to contemporary Japanese scholarship that his co-worker Osada was compelled to admit that Vovin’s attack upon it was the first that he and the others at the Nichibunken meetings had even heard of Starostin and his work.

This is not surprising. The book in question was published at a difficult time in East European history, and copies were slow to filter out to the rest of the world. Two major reviews in journals of international circulation (*Language* 69:4 [1993] pp. 828–832; *UJbr* N.F. 13 [1944] pp. 68–107) might have

been expected to have brought the book to the attention of Japanese scholarship (as they surely would have done three decades ago); but apparently that did not happen. That was especially unfortunate, since in this contribution, and a full two decades after Doerfer's ruling that "Japanese cannot be related to Altaic because 'Altaic' does not exist," Starostin in 1991, for all the world as if Doerfer had never issued his *ukaz*, methodically went about reinvestigating the lexical comparisons between Japanese and the "other Altaic languages" (including Korean) then already in hand, added a large number of new comparisons of his own, and projected the whole onto an expanded version of the once-classic, but by 1991 widely discredited, Ramstedt-Poppe reconstruction of Proto-Altaic.

When all is said and done, Vovin's 2003 critique of STAROSTIN 1991 ends up pretty close to where Doerfer left us in 1974 when he answered his own question ("Is Japanese related to Altaic?") with a circular dialectic denial ("No, because there never has been any Altaic to which it might have been related"). To be sure, now there is here a slight possible margin for wriggling out of Doerfer's negative circularity of denial ("the possibility of a genetic relationship between Japanese and Korean" is held out); but even if the ostensibly tempting bait of a "more speculative, but still quite feasible link to Manchu-Tungusic" is also held out to us as well, the order-of-the-day that the corporal here posts for us all is still *caveat comparator lectorque*: neither Korean, nor Manchu, nor Tungus has anything to do with "Altaic"; hence nothing is known "for sure" about where any of these came from; and so, even if they may possibly, or eventually, or accidentally, prove to have anything to do with Japanese, we still are now and in future will always be no closer to establishing, in the sense of learning anything about, the "origins of the Japanese language" – unless, of course, we are willing to settle for its being no more than a somehow mangled form of Korean, or Manchu, or Tungus – and in that case, still operating along the lines of Gerard Clauson, then why can't we read those languages even though we can read Japanese?!

In effect, what we now have here is almost precisely parallel to what we would have if, attempting to sort out the genetic connections of the handful of European languages best known to us today, we were to conclude that there is a "possible argument" for a genetic link between Dutch and German, and/or between English and German; but that at the same time beyond that it is impossible to go, since nothing is known about the further historical-linguistic connections of German. And of course, German must not be thought in any manner of form to be related to Sanskrit, because even though one knows German, one cannot make any sense of a Sanskrit text, even in romanized

transcription (the form in which Clauson confronted, and was defeated by, the Middle Mongolian *Secret History*).

Not only does this consensus seem like a fairly slim reward for the labors of twenty or more scholars working at the problem for an entire academic year; it also by any stretch is scarcely a new or novel achievement in Japanese scholarship. Kanazawa Shôzaburô (1872–1967) postulated a “common origin of the Japanese and Korean languages” in a book published in English in Tokyo in 1910; he later supplemented his argument in a Japanese version that appeared in 1929, with many later reprints. Kanazawa’s work was encouraged by the militaristic regime of the 1930s, who believed that his hypothesis of “a common origin” for the two languages, particularly since it did not go the one step further of identifying a still more remote “origin” for both, provided justification for the Japanese annexation of the Korean peninsula; this in turn also explains why Kanazawa’s work originally appeared in an English-language version aimed at the international community. Mercifully, there is no hint of the jingoistic adumbrations of 1910 and the years following in the Nichibunken committee’s 2003 consensus; but apart from that admittedly important detail, it nevertheless leaves us just about where Kanazawa did in 1910. We are told that the two languages have a “common origin”, but we are not told, or even encouraged further to inquire, what that origin may have been.

Still, the Nichibunken committee’s consensus is, for all its problems, not to be taken lightly. At the very least it goes a good way toward clarifying an otherwise puzzling lexical variation that begins with the volume’s title, and persists throughout the captions, both Japanese and English, provided for the various contributions that it prints. Just as we cite it above, the book has two titles, one in Japanese, the other in English; but they are far from both saying the same thing. The Japanese title promises information about *Nihongo keitôron no genzai* ‘the present [status, situation?] of studies (*ron*) concerning the genetic relationship of Japanese’. Though to be sure somewhat elliptical, which is only to be expected in a book title, this locution calls for no special comment except possibly for its use of *ron* 論. This morpheme, frequently misunderstood (and mistranslated) by students of Japanese as carrying implications of ‘discussion’ or ‘dispute’, is more nearly equivalent to NHD *-ologie*, NE. *-ology*; here it is employed in the same way as, e.g., in such terms as *Shihonron*, the standard translation of *Das Kapital*; cf. also its parallel function in the ubiquitous *Nihonjinron*, not ‘discussion (or, dispute) about the Japanese’ but ‘the essence (or lore, or science) [of what constitutes] a Japanese’.

But more important to the sense of the whole are *keitô* 系統 ‘genetic relationship’ and *genzai* ‘the present’. Both are conspicuously absent from the parallel English title, where instead of ‘genetic relationship’ we find ‘origins’ and instead of ‘the present’ we find ‘perspectives’. This is no simple attempt at ‘elegant variation’ between an original and its translation, particularly in the case of the replacement of ‘genetic relationship’ by ‘origins.’ Behind this there lies a decades-long history of covert disputation within Japanese linguistic scholarship, by and large the result of a culture-clash growing out of the academic community’s increasingly detailed familiarity with the principles and practices of neogrammarian historical postulates, particularly as those were developed and refined in German scholarship on the Indo-European language family. These principles and practices were essential to the establishment of ‘genetic relationships’ among languages, always with the implication of deeper relationships to earlier, mostly now-lost, proto-languages or linguistic unities. As knowledge of these ideas reached Japan, the question was soon asked, what might the genetic relationships, the *keitô*, of Japanese prove to be?

Initially both Japanese scholarship and Japanese society were fairly comfortable with this quest, and with the terminology that it employed. But with the growth within that society of the belief in a supposedly ‘distinctive’ or ‘particular’ character of Japanese culture, much of Japanese academia felt increasingly uncomfortable with the term *keitô*. After all, *keitô* was something that explained the relationship of English to German, or on another level even that of the Romance languages and Germanic and Slavic *etc.*, *etc.* with Indic. Perhaps, certain voices began to suggest, *keitô* was something that the languages of ‘foreigners’ had but that Japanese did not; others went so far as to suggest an inverse parallel with the phenomenon of *kotodama* ‘the spirit of language’ which, it was generally admitted, Japanese had but the languages of the foreigners did not. But even if that were true, what could be said of the remote history of the Japanese language? The eventual answer was that Japanese did not have *keitô*, in the sense of a genetic link with other languages that could be demonstrated according to neogrammarian standards; *keitô* was something that could be identified only in the languages of Europeans and other non-Japanese. In its place, the Japanese language could only have had *kigen* ‘origin(s)’, a term that carried no necessary implications of relationship to any other ethnic or socio-linguistic entity.

In the volume under review we find both these terms, in Japanese and English alike, employed almost in free variation with one another. The restriction of *keitô* ‘genetic relationship’ to non-Japanese contexts and its

replacement for domestic use by *kigen* 起源 ‘origin’ that two decades ago was quite carefully observed is here more than a little blurred; also adding to the general terminological confusion that now clusters about both these terms is the influence of the English plural ‘origins’, which of course directly corresponds to nothing in the Japanese. Nevertheless, the earlier semantic separation of the two terms is without question still visible in the bilingual title of the volume, where the Japanese has *keitō*, but the English has ‘origins’. It is not difficult to imagine that at the outset, when Vovin began marshalling his squad at the Nichibunken, he intended that his troops would study *keitō*; but when it was all over, they were able to do little more than advance ‘perspectives’ on ‘origins’; hence the English title of the book.

At any rate, since the committee’s consensus is reported as having allowed the possibility of some variety of historical connection between Japanese and Korean (though carefully avoiding the question of what can be said about Korean itself, a language consistently identified with Altaic [or as here, ‘Altaic’] for more than a century), the volume’s contributions that particularly deal with Korean deserve special attention.

Itabashi Yoshizō (pp. 131–185), who elsewhere (p. 5) describes his field of expertise as *Nihongo keiseiron* 形成論 ‘the formation of Japanese’ (yet another new circumlocution, perhaps intended to replace both *keitō* and *kigen*?), devotes one of the longest sections of the volume to a study of Old Korean lexical elements recorded as place-names in the geographical chapters of the *Samkuk saki* of 1145. Unfortunately he does not appear to be familiar with several recent publications by various hands dealing with these records, where he might have found the answers to most of the problems that he confronts; even more curious is the fact that he does not cite his Korean materials directly from any of the easily available text-editions of his source, but relies instead upon an unpublished “mimeographed paper from Indiana University” available to no one else. He (or his *samizdat* source?) clings to the traditional but philologically unsupportable reading of *l* or *r* for the Chinese phonogram 尸 *shī* (p. 142 ff.), perpetuating an early error of the pioneer Japanese Koreanologist Ogura Shinpei (1882–1944) that strangely enough has become rigidly enforced historical doctrine in modern Korean academic circles. He should have consulted, *inter alia*, the paper by Werner Sasse (1982) here cited by Vovin at p. 36 but apparently not studied during the committee’s year-long consultations; there he would have discovered evidence for concluding that, as might be expected, this phonogram was used to transcribe a Korean ㅅ. (The correct interpretation of this phonogram is immediately relevant to, and phonologically contradicts, many of Itabashi’s etymologies,

e.g., his ‘plough’, p. 142 (12), and ‘wild’, p. 154 (57), cf. UAJbr 51 [1979] pp. 14, pp. 16–17.) Wm. Rozycki (pp. 451–562) attempts to study a small selection of the same materials treated by Itabashi; like Itabashi he too is insufficiently familiar with the existing literature on his topic. Even more surprising, he appears not to be familiar even with Itabashi’s work, which he must have encountered at the Nichibunken meetings. Like Itabashi, he too does not go directly to his text-sources; he even goes one step further in reliance upon unavailable Bloomington *samizdat* sources for a number of highly unlikely Middle Chinese reconstructions that serve as the basis for his scrutiny of the Chinese phonogram orthography of the *Samkuk saki*. Similarly passing in silence over relevant existing publications is Fukui Rei (pp. 221–235), the other substantial Korean contribution to the volume. His English abstract (p. 235) promises “a new theory on the reading” of this same critical Chin. *shī* phonogram that stumps Itabashi; but his Japanese text (pp. 228–229) does not deliver on that promise. He too might have been expected to have learned about the literature on this question as set forth in Sasse’s publication in the course of the Nichibunken sessions; but apparently no one told him about it either. If they had, he would have realized that in the Korean imperfect adnominal verb forms he studies, as elsewhere in the relevant corpus, this *shī* phonogram writes Kor. *š*, a phonological entity precisely parallel to that of related forms in Turkic (see K. H. MENGES, UAJbr 51 [1979] p. 32 note 124), moreover one that additionally supplies new evidence for the essential correctness of O. Pritsak’s long-standing but mostly ignored suggestion (UAJbr 35 [1964] pp. 337–349) to the effect that Trk. *š* < \**š*<sup>2</sup> frequently may be traced to original clusters of *l* + C in other languages (in Fukui’s materials, the cluster is written as the MKor. *-lq* sequences that he documents but cannot further explain).

Other papers sometimes make casual mention of Korean materials, but to little purpose; e.g., Matsumoto Katsumi, who urges (p. 110 note 94) that nine Korean-Japanese lexical pairs, of the order of Jap. *tera* ‘Buddhist temple’ and Kor. *čel* ‘id.’, are all cultural loans and should not be discussed on the level of genetic relationship; but in the case of each of his nine examples, including the ‘Buddhist temple’ one, this is only to beat a dead horse: no one has seriously proposed any of these word-pairs as evidence for genetic inheritance for decades; similarly, Kazama Shinjirō (p. 253), who alleges that the modern employment of Kor. *tul* ‘a group, all together’ as a plural marker is unknown to other “Altaic languages” including Japanese; but this overlooks the close phonological and semantic links between the earlier attested forms of this morpheme, MKor. *.tol.h-*, and OJap. *tati* ‘suffix marking respect

for groups of deities and humans of similarly high rank'; the MKor. *-l.h-* here corresponds to the final *-t-* of OJap. *tati* and both revert to Proto-Altaic *\*r<sup>2</sup>* (again validating Pritsak's hypothesis), in a complex historical-phonological scenario that effectively refutes Kazama's allegation (English, p. 340) that "no one has been able to establish a lexical cognates [*sic*] among the core vocabularies of these languages based on the strict laws of phonetic change"; further, his citation and glossing of Korean evidence for this morpheme would have benefited by consulting S. E. MARTIN, *A Reference Grammar of Korean* [1992], p. 819. Like so much important literature in the field, Martin's work is cited once in the volume under review (p. 34), but appears never to have been consulted by the members of the committee.

In a word, little if anything that is said about Korean in this volume adds to our knowledge either of the history of that language or of its possible genetic relationship to Japanese. If what we can learn here about the Korean side of the overall question studied by the Nichibunken committee actually is a "perspective" on the *genzai* 'present state' of these studies in the Japanese academic community, then the present – as often in history – appears to be virtually indistinguishable from the past.

Finally, two English-language contributions from invited guests, not members of the research squad, are printed near the end of the volume, and deserve special attention.

The first is by Stefan Georg of the University of Bonn, who offers a wide-ranging refutation of the Altaic hypothesis (pp. 429–449) that, though silent on the subject of the Japanese language and its history, appears to be designed to warn away from consideration of Altaic connections any scholars who might, in the absence of such salutary cautions, still be tempted to cast their lot in with the likes of Ramstedt and Poppe. When one considers how frequently over the past three or four decades the Altaic hypothesis has been "conclusively refuted", it becomes a little difficult to understand why, as here, it is still apparently necessary to do it all over again, and again, and again – or do those opposed to Altaic perhaps have in mind a dialectic gambit akin to the old Chinese torture known as "Death by a Thousand Slices"?

At any rate, Georg focuses his refutation on an important segment of the canonical Altaic reconstruction hypothesis, *viz.* its postulation of the existence, somewhere and sometime in the Altaic linguistic unity, of a set of four liquid or 'vibrant' phonemes, usually symbolized in the literature as *\*l*, *\*l<sup>2</sup>*, *\*r*, *\*r<sup>2</sup>*. In the original formulation of the Ramstedt-Poppe reconstruction, Proto-Altaic *\*r<sup>2</sup>* was inherited as *r* throughout the later languages, except for Turkic,

where it appears as *z*, with the further important exception that in Chuvash it ungratefully survived as *r*. Georg offers instead the now-current reverse scenario hypothesis, which sees Turkic *z* as original, and postulates the shift  $z > r$  to account for all the non-Turkic, as well as for the Chuvash, forms. With this, by implication, not only does the hypothesis of two original varieties of *r* in the proto-language fall, but so also does that of two original varieties of *l*; and along with that, the whole hypothesis of an original Altaic language collapses, which is the main point of Georg's contribution. In evidence he cites a single *Wanderwort*, Chu. *pir* 'cloth'; this he argues ultimately reverts to Grk. βύσσοζ, "which found its way into Arabic *bazz*, from where it came to the Bolghars of the Volga, where it, and this is of course important, underwent the sound law  $z > r$ " (p. 426) – and in the process, he might have added, deftly swept away the Altaic hypothesis lock, stock, and barrel.

So far so good; nevertheless, there are problems. Not the least troublesome of these is the question of just who these "Bolghars of the Volga" were, where they came from, and especially how many of them there must have been in order to have carried out all the linguistic changes attributed to them. As E. Helimski has pointed out, when we opt to work along these lines we soon confront an early Asian linguistic landscape so crowded by "ubiquitous Bulgars ... [that] neither in Mongolia nor in Siberia [would there have been] practically any place for those Turks who are supposed to [have] coexist[ed] with them" (*JSFOu* 83 [1991] p. 263). Troublesome also is the question of the Chuvash form *pir* upon which Georg's refutation of Altaic is anchored; he does not cite (though surely he knows of) Chu. *pūs* 'calico', which with its vowel *-i-* and its final *-s* would be an excellent candidate for a virtually unaltered phonetic imitation of whatever may have been the original form(s) behind all these words in all these languages, whether Arabic or Hellenic Greek or anything in between; it would also represent a vitually accurate phonetic imitation of any of a number of Mongol forms that without question originated in this same *Wanderwort* scenario, viz, MMo. *böz*, WMo. *bös*, Mongr. *bos*, etc., 'cotton textile' (N. N. POPPE, *Introduction to Mongolian Comparative Studies* [Helsinki, 1955], pp. 49, 122). Finally (and "last but not least") there has long been available in the literature an involved etymological excursus that would relate, to all these *Wanderwort* forms, not only a number of Tungus-Manchu forms but also OJap. *Fusuma* 'a kind of bedding; a coverlet' (in: R. J. PEARSON, ed., *Windows on Japanese Past: Studies in Archeology & Prehistory* [Ann Arbor, 1986], pp. 101–120). Probably all this evidence, and at the very least all these other forms, including but not limited to Chu. *pūs*, must be taken into account before Georg's sweeping refutation

of the Altaic hypothesis, resting here as it does upon the evidence of a single phoneme in a single form with a vowel that seems strangely altered (why, one must ask, does it have *-i-* for original *\*-u-*?), may be said to stand scrutiny.

The second is by Juha Jahunen of Helsinki University (pp. 477–490), whose contribution takes the unusual form of “a possible framework for the understanding of Japanese linguistic prehistory ... in the form of issues (numbered 1–35), consisting of theoretical presumptions, empirical facts, preliminary conclusions, working hypotheses, and unsolved problems”, in the hope that “future research” will “solve the problems and verify to what extent the proposed working hypothesis are correct”; to that end “no actual language material is quoted, and references are kept down to a minimum” (p. 477 with note 1). His numbered “issues” might more accurately be called “propositions” or, in the original scholastic sense of the term, “theses” (cf. Dr. Martin Luther’s Theses on the Church Door); they range from “1. Japanese is a normal language” through such intriguing propositions as “9. Japanese has no known living relatives” and “10. Japanese belongs to the Altaic type” to “28. Pre-Proto-Japonic was a Dongyi [*i.e.*, 東夷 ‘Eastern Barbarian’] language”. Ten members of the Nichibunken committee to whom this paper was read were asked to vote ‘yes’, ‘no’, or ‘don’t know’ on each topic (interestingly, one replied ‘don’t know’ to whether or not Japanese is a normal language). Their replies are summarized (pp. 488–489), but indicate little more than that “we still know very little about the linguistic prehistory of Japanese” (p. 489).

It would be interesting to employ Jahunen’s Thirty-five Theses in future group study and discussion of these issues, ideally bringing them to the attention of more than the stalwart group of ten who attended their original presentation in Kyoto 23 September 2001. But before that is done, the text as here printed must first be carefully checked for errors. Among its many misprints one is particularly ominous – the consistent replacement of the author’s original “areal” by “a real”, so that in “12” for “Japanese is areally linked with Korean” we read instead “Japanese is really linked with Korean” not once but twice; so also in “25” where for “it must have been areally contiguous with some other language” we read instead “it must have been really contiguous...”. Some of the many printing errors in these pages, such as Itabashi’s “Kargren” for “Karlgrēn” (p. 147) and the frequently careless transcriptions of Japanese names and words (*e.g.*, pp. 247, 466), will be self-correcting for most readers; but that is hardly the case with these multiple occurrences of “really” for “areally”.

I am grateful to Professor Jahunen for calling these misprints, which I would not otherwise have detected, to my attention in a personal communication under date of 9 February 2004; but in that same communication he also raised another issue far more serious even than these unfortunate misprints. Commenting upon the text of his “13. Japonic was once spoken in Korea” as now printed he wrote: “This is an editorial distortion of my originally different formulation.” From this I conclude that the text of Jahunen’s contribution as printed is not to be trusted as accurately presenting his views, which in turn throws into question its ultimate utility for the very kind of future research and investigation it was designed to promote. The question must also be asked about who tampered with his views in this fashion before the publication was sent to the press, and equally important, why this was done; also, were other papers similarly altered to reflect views other than their authors’ ?

On the face of the matter, one might suspect that such unauthorized changes in authors’ texts in some way or another lie behind the otherwise impressive anti-Altaic *Leitmotiv* that all the papers printed here echo time and time again. Such wholesale scholarly agreement upon any issue, particularly upon one as rife with controversy as the Altaic hypothesis, is rare. Yet this volume records no single instance of anyone involved in the year-long meetings of the committee ever having had a good word to say for Altaic, much less for the pioneering work of Ramstedt and Poppe to whom we owe it all. Or can it be that this impressive unanimity of opinion is, in part at least, the result of judicious editing and pruning of manuscripts without their authors’ approval?

Of course, this is not to suggest that the misrepresentation of his views of which Jahunen complains was along such lines. Whatever happened to his text, it must have involved still other controversial issues, since elsewhere Jahunen has frequently gone on record as being of the opinion that the entire Altaic hypothesis is no more than an “illusion of genetic relationship” growing out of “a methodological error” that, simply because it is so gravely in error, has tempted certain misguided persons to “extend the Altaic hypothesis to Korean and Japanese” (“Prolegomena to a Comparative Analysis of Mongolic and Tungusic”, [1996] p. 209, in: G. STARY, ed., *Proceedings of the 38th PIAC...*, 1995). Given this and other well-known examples of the unmistakably clear-cut manner in which he has always expressed his scholarly positions in his publications, one is even more at a loss to understand this overt example of academic censorship of his views in the course of the Nichibunken publication process. But at the very least, it casts a shadow over many of the statements that the volume attributes to other members of the committee, not to mention, as in this instance, their invited guests.

Kanazawa's 1910 English-language publication of his hypothesis of a "common origin for Japanese and Korean" attracted considerable scholarly attention in its day. As late as 1939, L. H. Gray (1875–1955) of Columbia University found room to mention it in his *Foundations of Language* (New York, 1939), a basic introduction to linguistic science with major emphasis upon historical linguistics, long used in many American universities until, *ca.*, 1970, courses in those subjects were removed from most curricula. Gray naturally took no sides in the issues raised by Kanazawa, remarking only that his book "discussed ... the question of the connexion [*sic*] of the two languages [*i.e.*, Japanese and Korean] not wholly conclusively" (p. 373). By and large, with this Nichibunken publication, not much has changed since Kanazawa in 1910 and Gray in 1939, not only in connection with the relationship (or perhaps the "common origin" ?) of Japanese and Korean, but also as regards the ultimate "origin" (or, "genetic connection(s)" ?) of Japanese.

But as it happens, and just when one might not expect it, there is nevertheless a hopeful sign of possible future scientific progress concealed in these pages. Osada (pp. 10, 12) reports in his introduction to the volume that he has heard of the publication of a book that he calls *An Altaic etymological dictionary* by Sergei Starostin, Anna Dybo, and Oleg Mudrak, in Leiden in 2002, but regrets that it appeared too late to be studied by the Nichibunken committee. He has the title wrong (it is *Etymological Dictionary of the Altaic Languages*), and it was published in 2003, not in 2002. But otherwise Osada is entirely correct; it is truly unfortunate that no one involved either in the Nichibunken meetings or with the publication of its findings could consult this work, since in effect the new etymological dictionary of Starostin *et al.* now renders moot virtually everything discussed in this volume.

This new dictionary treats all the Altaic languages (without feeling it necessary to demean that designation with single or double quotes); it studies in detail hundreds of etymologies, some old, some new, in many languages, and *inter alia* documents and studies a large number of Japanese lexical items, proposing for many of them genetic connections with Tungus, Mongol, Turkic, and Korean cognates, in other words, with Altaic, plain and simple.

As only to be expected of a major work of the huge scope of this new etymological dictionary (a total of 2,096 pages in 3 vols.), many details involved in equally many lexical-etymological entries will obviously call for critical scrutiny by specialists in this language or that, as well as by comparativists interested in the study (and not necessarily in the demolition) of the Altaic hypothesis. Nevertheless, with the appearance of this enormously important publication all previous work on the genetic relationship of Japanese

must now be reinvestigated and reevaluated. It is a great pity that this publication appeared too late to be studied by the Nichibunken committee members. But given the evidence in this volume of how they passed in silence over equally enormous quantities of previously published contributions to the historical linguistic literature that were easily available to them, one must ask if, in actual fact, it would have made much difference even if the new dictionary of Starostin *et al.* had been at hand. Or would the mere fact that the forbidden term ‘Altaic’ appears in its title have meant that it would immediately have been ruled out of court?

Vovin’s paper, by all odds the major contribution to this volume, asks the question *Kore kara doko e?* (p.15), “Where do we go from here?” (p. 29). Perhaps the question for all of us who, despite all odds, remain interested in the question of the genetic relationship of Japanese, is not so much “where?” as “how?”. Vovin (p. 29 note 3) maintains that all previously published work on the question is flawed by its inaccurate and unreliable data, particularly with respect to historical morphology, and gallantly offers samples of his own work in this area as an example of the path to be followed in future.

To be sure, there are many errors both of data and of their interpretation in the published literature, not to mention those that probably lurk in the generally unavailable Bloomington *samizdat* sources that the committee relied upon so heavily. More careful attention to details of forms and meanings in all our considerations of Japanese historical linguistic questions is clearly in order; and we will all welcome more and careful publications by Vovin and his Nichibunken squad in the months and years to come. But that having been said, it must be pointed out that not all the examples that he offers of his own morphological data and analysis, which he alleges to be superior in accuracy and detail to anything available up to this point in time, are reassuring.

In discussing a ‘negative morpheme’ that he reconstructs as *\*-e*, and for which he claims to have identified cognates in Old Turkic, Middle Korean, and Old Japanese, Vovin cites as his primary authority p. 474 in the *Morphologija* volume (Moscow, 1988) of the well-known series of studies in the historical grammar of the Turkic languages edited by E. R. Tenišev. Actually, what he cites is a chapter in that volume by N. Z. Gadžieva dealing with converbs (*dejepričastie* ‘gerunds’ [*sic*]), who nowhere even mentions constructions of negation or negative morphemes; from her account of the converbs he then, for his Old Turkic evidence, takes over one of her citations from the Kül Tegin stele (but miscopies line 46 as line 36), which text however contains no negative formants at all, which is also the case for the Korean evidence that he adds (poem 36 of the Yi dynastic panegyric) as well

as for his Old Japanese text citation (*Man'yôshû* 804). His Turkic and Korean text fragments illustrate routine Altaic converb usage; his Old Japanese example has nothing at all relevant to offer; and nothing in any of the texts cited, no more than in his primary source the 1988 *Morfologija* edited by Tenišev, has the least connection at any level with the ‘negative morpheme’ \*-e that he claims to be documenting, illustrating, and discussing. Further, he makes no mention of earlier publications that actually do identify and discuss in detail the inheritances in the various later language of an Altaic negative verb \*-e (sic! not \*-e), e.g., K. H. MENGES, *Abhandlungen für die Kunde des Morgenlandes*, Bd. xii,2 (Wiesbaden, 1975), pp. 5, 96–110; *CAJ* 29 (1985) 1–2: pp. 35–84; or were these, and the considerable literature they cite, all found to be too “inaccurate and untrustworthy” to be taken into account? Perhaps so; but at least – and for all their faults – they really cite negative forms both from Japanese and from Korean, and from the “other languages” as well, which is more than Vovin manages to do. If this sort of thing is to be “where we go from here” in these studies, ours will be a bumpy road all the way.