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Roy Andrew Miller: Review of Harry Halén, *Biliktü Bakshi, the knowledgeable teacher: G. J. Ramstedt's career as a scholar*. (Mémoires de la Société Finno-Ougrienne 229.) Helsinki: The Finno-Ugrian Society, 1998. viii+371 pages.

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The "Preface" to this new study of Gustav John Ramstedt (22 October 1873 - 25 November 1950) promises "to describe in detail the development of Ramstedt's scholarly career ... and also his discoveries and their significance in his own time and later" rather than merely giving "a narration of adventures or a colourful description of exotic lands", on the entirely reasonable grounds that Ramstedt "himself [had] already in his unparalleled way described his experiences among the peoples of Asia in his [own] memoirs ... originally published ... in 1944" (p. vii). Regrettably, this promise is not kept. Most of the book consists of detailed accounts of Ramstedt's travels, hardships, academic and diplomatic careers, and private and public life; but overt references to "his discoveries and their significance" are too few and in the main disappointingly trivial both in content and analysis.

This does not mean that the book is not worth reading. Indeed, in many ways it is difficult to put down once one begins its story. Ramstedt's almost incredible persistence and determination in the face of enormous physical hardships on his Central Asia expeditions; restarting his research anew after the tragic loss of his books and notes; surviving in his academic career despite the stirring events of modern Finnish history; continuing his work and "discovering Korean" while serving for ten years as a mostly unappreciated diplomat in Japan—all this and more is fascinating to read. But it is also precisely that "narration of adventures" that Halén promises us at the outset not to give us. It is genuinely interesting to read of Ramstedt's involvement with the Temperance Movement (which sometimes paid him almost as much as his university salary, p. 200); of his interest in Esperanto; and of his involvement, even if only tangential, with several fairly far-out Japanese champions of the Turanian Movement (pp. 262-267). It is even interesting to learn that Vilmos Pröhle, down on his luck in Tokyo, borrowed 100 yen from Ramstedt and "cleared off without paying it back" (p. 267). But none of these accounts, which take up most of the book, really tell us anything about Ramstedt's scholarship and its significance.

Even more unfortunately, on the rare occasions when Ramstedt's scholarly accomplishments do manage to make their way into these pages, the treatment afforded them is mostly superficial and frequently also inaccurate. In particular, the following specific cases may be cited, though this list does not pretend to be exhaustive:

1. The hypothesis now generally known in the literature as "rhotacism / lambdacism", proposing historical priority for Turkic *z*, *š* and secondary developments of these phonemes into *r*, *l* in Chuvash or in other cognates showing *r*, *l* correspondences, is at least twice mentioned (pp. 112, 282) in connection with

Ramstedt's work on Turkic and Mongolian. Both these notices are anachronistic and factually misleading as well, since Ramstedt's eventual historical interpretation of the relevant data was precisely the reverse of what this neologism implies. Another notice of this issue (p. 80) further confuses the matter by incorrectly citing "the correspondence *l ~ š* in Turkic and Mongolian", where of course these two language-names must be reversed. There is no hint here of the vast literature that still surrounds this issue; all that is said of Ramstedt's position on this is that it has "been criticised as unproved (e.g. B. A. Serebrennikov, 1960), and several new explanations have been put forward" (pp. 112-113). But "B. A. Serebrennikov, 1960" is never further identified, nor its views even summarized, anywhere in this book.¹ The way in which Ramstedt's increasingly rigorous application of neogrammarian principles to a historical explanation of the Trk. *š, z* :: Mo. *l, r* correspondences eventually provided the basis for Poppe's magisterial formulation of four contrasting "Lateral- und Zitterlaute" (1960: §§ 59-63) in the original language is one of the epic episodes in the history of Altaic studies; here it remains untold, and even worse, obscured by deviously anachronistic terminology and bewilderingly incomplete citations. Similarly anachronistic is the claim that, as early as 1904, "[the] main goal of [Ramstedt's] Mongolian studies from the very beginning was to settle the Altaic question" (p. 110), as well as the statement that Ramstedt's 1912 *Verbstamm-bildungslehre*, inaccurately described as "a treatise on verb roots", was "meant to

¹ Perhaps this is a reference to a paper by B. A. Serebrennikov, "O nekotoryx spornyx voprosax sravitel'no-istoričeskoj fonetiki tjurkskix jazykov", cited in Ščerbak (1970: 84, note 229) as "correctly having pointed out that the hypothesis of G. Ramstedt [on the historical explanation of the "rhotacism / lambdacism" correspondences] was erected without taking into account the special development of Chuvash ..."; but this too tells us very little of substance and this time the citation lacks both place of publication and date. It is possible that Halén intended instead here to cite Ščerbak's 1970 monograph, now frequently invoked as the *locus classicus* for the anti-Ramstedt-Poppe formulation that would take Turkic *š, z* as primary and historically original, and from these derive Chuvash *l, r* as a result of phonemic split conditioned by original vowel length; the heart of this hypothesis is set forth in Ščerbak (1970: 84-88). Unfortunately, Ščerbak has "controlled" his data by neglecting to mention critical counter-examples that would contradict his formulation; this is particularly fatal for his scheme that would account for the *-l* in Chuvash *čul* 'stone' as conditioned by the original long vowel in Turkic *tāš* 'id', and that further cites in support of Chuvash *puš* 'head' with the *-š* going with the original short vowel in Turkic *baš* 'id.'. What he neglects to mention is the counter-example presented by Chuvash *tu* 'outside; the exterior', going with Turkic *taš* 'id.'. This striking contrasting pair was discussed long ago in Ligeti (1938); cf. now also Clauson (1972: 557a, 556b), and is in itself enough to show that Ščerbak's rule is actually no rule at all. At any rate, on its way to press Halén's book appears to have lost the list of "References Cited" that would explain this and other enigmatic citations (cf. note 12 *infra*), an accident that puts one sadly in mind of the equally curious fate that befell the manuscript of Ramstedt's own introduction to his *Studies*, described by Aalto (1953: [3]).

complement the Turco-Mongolian historical phonology” (p. 168).² And the account of the *rationale* behind Ramstedt’s Kalmuck dictionary (p. 189) is also both anachronistic and factually misleading. In none of these instances does the book do anything resembling justice to “Ramstedt’s scholarly career ... his discoveries and their significance.”

2. The author’s promise to “investigate in detail ... [the] significance [of Ramstedt’s achievements] in his own time and later” is broken at every turn, not only in the case of the reconstruction of the “Lateral- und Zitterlaute” as above. Ramstedt’s speculations concerning *qan*, *qayan* that occupied him off and on for half a century are here somewhat obscured by obvious typographical problems (p. 45); but more serious is the lack of any evaluation of the same in the light of the wealth of later literature.³ So also for his early notice of possible Altaic connections for Akk. *pilaqu* ‘dagger’ (p. 172) where, in an exception that only proves the rule, a single citation of the later literature is almost more misleading than none would have been.⁴ And, in view both of the central role that Korean played in Ramstedt’s latter

² The claim of the text as cited is anachronistic: in 1912 there existed no “Turco-Mongolian historical phonology” for the *Verbstammbildungslehre* to “complement”. But over and above this simple chronological *lapsus*, it also obscures an important fact about Ramstedt’s seminal role in the early evolution of Altaic comparative linguistics. In his 1912 monograph Ramstedt documented for the first time significant surviving vestiges of the original Altaic system of secondary verbal stem formation long before most of the details of Altaic comparative phonology had been established. An important parallel in the history of scholarship may be identified between this sequence of events in the development of Altaic linguistics and that of the investigation of the Indo-European languages, where F. Bopp’s 1816 treatise *Über das Konjugationssystem der Sanskritsprache* appeared long before the description of any Indo-European sound laws, indeed before the very concept of the sound law as such had become clear. Also misleading is Halén’s term “Turco-Mongolian”; it was with good reason that in the title of his 1912 treatise and elsewhere in his early work Ramstedt habitually used the order “mongolisch-türkisch”.

³ Unresolved is the question of whether the evident confusion of *n* and *ŋ* in the passage cited is Ramstedt’s or Halén’s. For the later literature, none of which is cited, useful introductions are to be found in Rosén (1980) and de Rachewiltz (1983). These two independent studies complement each other, with the Rosén paper especially valuable for the evidence it cites showing that forms related to both *qan* and *qayan* “existed in Korea already during the first centuries A.D.” (p. 155), a point missed by de Rachewiltz. Rosén also has valuable and detailed critical evaluations of Ramstedt’s 1939 etymological attempts to treat a number of Turkic and Mongolian titles; here (p. 45) we are told only that in that investigation “[he] chose a slightly different line”.

⁴ Cf. now the discussion and literature cited, both *pro* and *con*, concerning a possible Semitic origin for Skt. *paraśú*, Gk. *πελεκυς* ‘pole axe’ in Gamkrelidze & Ivanov (1995: 620-621). The problems of forms and their meanings reviewed there at the very least begin to cast serious doubt upon the straightforward equation of Akk. *pilaqu* ‘axe’ with Mo. *aluqa*, MMo. *haluqa*, Ma. *folho*, Ev. *halka*, etc., ‘hammer’ (as, e.g., in Cincius et al. (1977: 2.313); cf. also Doerfer (1985: 22)).

work, and of the resurgence of interest in and information concerning E. D. Polivanov (1891-1938) following his posthumous “rehabilitation” in the former USSR, the single notice of Polivanov and Korean (pp. 260-261) is totally inadequate.⁵

3. No attempt is made anywhere in this book seriously to evaluate the significance of Ramstedt’s etymological work on Korean. At least we are spared a reiteration of the routine denigration of these studies that now seems to be *de rigueur* in Helsinki.⁶ But totally to ignore the historical-linguistic contributions of this work and instead to assert that its only significance is to be identified in the way in which it “laid a solid basis for the Korean people to regain their self-esteem and find their roots [sic!] after 30 years of suppressing Japanese occupation” (p. 208) is hardly any better. The veritable barrage of criticism and polemic that greeted the publication of Ramstedt’s *Studies in Korean etymology* in 1949 has been described in the literature,⁷ but in the main it remains unexplained; and here it is not even alluded to. Also missed is the opportunity to clarify the circumstances behind the 1982 publication of the *Paralipomena of Korean etymologies* “collected and edited by Songmoo Kho” (1947-1993). We are told that “[t]he Korean Altaist Songmoo Kho, studying in Finland, even wanted to publish Ramstedt’s more dubious Korean etymologies, once rejected by Ramstedt himself” (p. 333). But in the “Foreword” to the *Paralipomena* Kho himself claimed that the impetus to publish these materials originally came

⁵ Polivanov’s 1927 article on the relationship of Korean to “the ‘Altaic’ languages” was reprinted in his posthumous collected *Stat’i po obščemu jazykoznaniju* (Polivanov 1968: 156-164), and later translated into English by D. Armstrong as “Toward the question of the kinship relations of Korean and the ‘Altaic’ languages”, in Polivanov (1974: 149-156). It is significant to note that as early as 1927 Polivanov held that “[t]he correspondences (1) Turkic *š* // Chuvash, Mongolian, Manchu-Tungus *l* and (2) [similarly for] *z* // ... *r* may be considered to be proven at the present time, after the works of Poppe and Vladimircov, and, therefore, there is full basis to expect their spread also to Korean” (Russian in reprint, p. 161; English translation, p. 153). Important on all these issues, especially concerning the relationship between Ramstedt’s later Korean studies and Polivanov’s strikingly prescient early achievements, is Ivanov (1957); cf. also on Polivanov’s career, L’Hermitte (1959) (where however the citation of Ivanov is incorrect).

⁶ E.g.: “In Korean studies, Ramstedt’s impact has not been equally resistant to the effect of time [in contrast with his Mongolian work, R. A. M]. ... His *Studies in Korean Etymology* (1949) and their posthumous additions (1953-1982) are even more obsolete today [than his *Korean Grammar* (1939)], to the extent that no serious scholar would use them for anything but historiographical purposes” (Janhunen 1999: 194).

⁷ By the reviewer in Miller (1984). And even after the fusillade of invective against the *Studies* had subsided, the posthumous publication of Ramstedt’s *Einführung in die altaische Sprachwissenschaft* by Pentti Aalto continued to draw fire in France: A. Sauvegeot (1959) denounced vol. 1, *Lautlehre* (Mémoires de la Société Finno-Ougrienne 104:1; Helsinki: 1957) of the *Einführung* by reason of what he termed “le déversement massif d’étymologies coréennes, dont certaines bravent l’imagination la plus fantaisiste”.

from Pentti Aalto and Harry Halén (Kho 1982: [3], 4). Something has happened to history here. This book now also adds *à propos* the *Prolegomena* that “R. A. Miller severely criticised the whole enterprise seeing nothing good in it” (*loc. cit.*) Without a citation of the review in question⁸ (and none is given!) it will be impossible for many readers of this book to verify how extremely misleading this last allegation is. That review criticised “the whole enterprise” precisely because it missed the opportunity such a publication provided for sorting out the many etymologies of positive value, the wheat as it were, from among the chaff of Ramstedt’s *Nachlaß*. Rather than thus further muddying the already troubled waters of the *Paralipomena*, and instead of attempting to relate Ramstedt’s etymological studies to the modern Korean search for a national identity, attention ought here to have been focused on an evaluation of his many positive accomplishments in this field, notably the many modern Korean forms in initial *CC-* that he convincingly explained in terms of putative cognates in other languages where initial *CVC-* sequences clearly pointed to the historical development of these distinctive Korean initial consonants through syncope-loss of an original first-syllable vowel, *CVC-* > *CC-*. Ramstedt’s etymological insight into these developments was all the more remarkable because he did not have available the documentary information of the Middle Korean written records now conveniently accessible to us; nevertheless, in case after case in his *Studies in Korean etymology* he correctly predicted the Middle Korean configurations that must underlie these modern initials on the grounds of comparison with other languages or internal reconstruction or both; and even in the *Paralipomena* there are not a few similar cases of absolutely accurate etymological insight.⁹ It is sad to see such striking scientific achievements passed over here in virtual silence.

⁸ Cf. the first citation in note 7 *supra*.

⁹ Frequently in his 1949 *Studies* Ramstedt was able to postulate protoforms that today are precisely confirmed by Middle Korean text-examples later discovered or at any rate unknown to him, a remarkable achievement for any historical linguist. A number of these reconstructions were performed through the combination of comparisons with other languages and what we today would identify as internal reconstruction within Korean itself (though that term is anachronistic for Ramstedt’s period). These include **psi* for NKor. *ssi* ‘seed, kernel’, reconstructed in Ramstedt (1949: 231), and now confirmed by MKor. *psi* ‘id.’; and **pučak* > **pčak* for NKor. *ččak* ‘one of a pair or couple’, in Ramstedt (1949: 19), confirmed by MKor. *pčak* ‘id.’, cf. MTrk. *bučuaq* ‘half’. In some cases Ramstedt’s equally prescient reconstructions relied on the comparative method alone, as in the reconstruction **skai-* that he postulated, but only in the *Paralipomena*, p. 66 (!), to account for NKor. *kkayat/l-* ‘perceive, wake up to; be spiritually awakened’, now confirmed by MKor. *skăy-* ‘id.’ (cf. Miller 1997-1998: 155). In other instances he relied entirely on internal reconstruction, as in his **psŭ-* reconstructed (1949: 236) to account for NKor. *ssŭ-* ‘to be bitter’, confirmed by MKor. *psŭ-* ‘id.’, and his **psŭ-* for NKor. *ssŭ-* ‘to use’, confirmed by MKor. *psŭ-* ‘id.’. In a number of other cases the initial consonant sequences that Ramstedt reconstructed do not account for an intrusive MKor. *-s-* that appears in the text-materials. Thus, for NKor. *ttay* ‘time, occasion’ Ramstedt (1949: 248) has **putai* on the basis of both the

4. Second only to Ramstedt's pioneering Korean etymological studies in its importance for the history of Altaic linguistic scholarship was his 1916 publication of the initial formulation underlying the reconstruction of Proto-Altaic **p-*, here erroneously dismissed as "a minor article" (p. 244); nor does the halting account (p. 245) of what this formulation, especially as elaborated by Pelliot in 1925¹⁰ was all about, do Ramstedt's contribution any greater justice; at the very least it was not as here alleged, a "rule for the weakening of an initial *p*" (loc. cit.). Pelliot's criticism, here only obliquely hinted at, in the main concerned Ramstedt's frequent attempts to

comparative method and internal reconstruction; but MKorean has *pstai*. Similarly, by the comparative method alone in the *Studies* (1949: 111) he reconstructed **pki-* for NKor. *kki-* 'to insert, place between', but MKor. has *pski-*; and in the same way **pk-* (1949: 115) for NKor. *kkü-* 'to put out (a fire)', but MKor. has *pskü-*. With these examples, however, must further be compared Ramstedt's **pta-* (1949: 246) for NKor. *tta-*, *ttang* 'the earth, ground, land', MKor. *sta.h-* 'id.', and **pto* for NKor. *tto* 'also, and, again', MKor. *sto* 'id.'. Finally, there is the instructive case of NKor. *ttal* 'daughter', for which Ramstedt (1949: 252) reconstructed **ptal* on the basis of the comparative method. MKorean has only *stäl* for this word; but in a still earlier source, the Chinese-Old Koryŏ bilingual glossary *Chi-lin lei-shih* from the first part of the 12th century, the word is written phonograms as †*pätäl* (Sasse 1976: 115, no. 154). Again, Ramstedt's application of the comparative method is here brilliantly vindicated by forms in early texts totally unknown to him: in effect, his **ptal* reconstruction simply reached further back into time than the chronologically shallower resources of our Middle Korean texts. (The possible relationship between the earlier **patäl*, **ptäl* forms and the later *stäl* forms is discussed, but unfortunately without reference to Ramstedt's researches, in Lee Ki-moon 1977: 108.) Examples such as these, in which reconstructed forms originally arrived at by neogrammarian principles are later confirmed by written records, are rare in the historical-linguistic literature; all these reconstructions by Ramstedt, far from being "obsolete today" and something "no serious scholar would use", are indeed rare and still valuable examples of carefully insightful reconstruction.

¹⁰ In his famous "Les mots à *h-* initiale, aujourd'hui amuie, dans le mongol du XIII^e et XIV^e siècles" (cited without title and incompletely, p. 245); this was followed by his equally important "Les formes avec et sans *q-* (*k-*) initial en turc et en mongol" (Pelliot 1944, not cited), where Pelliot continued to place under contribution a wide range of Ramstedt's publications, and where he himself described the goal of his 1925 article: "de fournir une liste aussi large qu'il était alors possible des mots altaïques où **p-* initial était amuie en turc, donnait encore en mongol médiéval une *h-* aujourd'hui amuie dans presque tous les dialectes mongols, était amuie ou représenté par *h-* dans certains dialectes tungus, mais, dans d'autres, était représenté par *p-* en jurčîn ancien et *f-* en jurčîn tardif, par *f-* en mandchou, par *p-* en goldi et en olča" (1944: 73). Particularly important in Pelliot's 1944 paper, and too often overlooked, is the clear documentation it provides for his acceptance of the Altaic hypothesis, both in the passage cited above, and in other references to "le **p-* altaïque" (e.g., p. 99 note). This was an impressive evaluation of the worth of Ramstedt's work by the acknowledged "Master Orientologist" of his day.

suggest numerous Chinese loans early in the history of Altaic, and especially in the history of the Tungus sector of Altaic. This was a problem that continued to trouble Ramstedt throughout his career, particularly later in his Korean etymological work. In any serious assessment of Ramstedt's achievements it must be dealt with; here it is barely identified, much less discussed.

5. The nature and extent of Ramstedt's association with Japanese scholars during his ten-year diplomatic residence in Japan (21 March 1920 - 24 March 1923; 20 January 1924 - 14 November 1929) is scarcely discussed. Even so, enough is said here (p. 217) to document his first-hand acquaintance with Shiratori Kurakichi (1865-1942): "Shiratori ... invited [Ramstedt] to a dinner at the University. ... At Shiratori's request Ramstedt lectured on the Finno-Ugrian peoples and comparative Altaistics at the Imperial University", etc. What ought to have been mentioned is that this information in turn certainly throws new and positive light upon the important question of whether or not Ramstedt laid Shiratori's 1914-1919 paper on the comparison of Korean with "Ural-Altaic" under contribution in the course of his own Korean etymological studies. As has already been pointed out, the amount of duplication between Shiratori's paper and Ramstedt's 1949 *Studies* makes it likely that Ramstedt knew Shiratori's work, even though he never cited it by title or name.¹¹ The probability that he did know it is now greatly enhanced by this evidence that indeed Ramstedt and Shiratori were acquainted, and that on a more than casual basis. Otherwise, the suggestion that "Ramstedt's inspiring presentations ... influenced ... young scholars: Izuru Shimura [read: Shinmura] in the field of Altaistics, Kyōsuke Kindaichi in research into the Ainu language, and Shimpei Ogura with his Korean interests" (p. 220) lacks the documentation that would be necessary for it to be credible. Shinmura (1876-1967) was only three years younger than Ramstedt; both Kindaichi (1882-1971) and Ogura (1882-1944) can scarcely be described as having been "young scholars" while Ramstedt was in Tokyo; and Ogura was appointed professor at Keijō (i.e. Seoul) Imperial University in 1926, midway during Ramstedt's stay in Japan, having begun his own Korean studies in the peninsula in 1911. Under these circumstances, and in the absence of relevant documentation, the claim for Ramstedt's "influence" on the scholarly work of any of these men seems unjustified.

Similar problems of credibility present themselves too frequently in the purely narrative passages that constitute the bulk of this book; chiefly they are caused by the author's unfortunate habit of never making clear precisely what fragment of the Ramstedt papers he is citing, or more usually paraphrasing, most often in a rambling

¹¹ Shiratori's long article "Chōsengo to Ural-Altaigo to no hikaku kenkyū" was published between 1914 and 1916 in vols. 4, 5, and 6 of the *Tōyō Gakuhō*; it is now more conveniently available in a careful reprint correcting a few obvious typographical errors and together with an essential index of Korean forms in vol. 3 of the *Shiratori Kurakichi zenshū* (Tokyo 1970). The question of whether or not Ramstedt was acquainted with this paper has been raised several times in the literature (e.g., Miller 1984: 148-149; 1997-1998: 148). A definitive answer is still not at hand, but the information here presented by Halén goes much of the way toward its eventual solution.

oratio obliqua style that frequently leaves the reader wondering whether it is Ramstedt or Halén who is speaking; a style moreover that lends itself to genial chronological confusion in its description of more than one important event in Ramstedt's career. The account of what was and was not lost of Ramstedt's field-notes in the famous railway-luggage debacle of 1901 (esp. p. 73) is tantalizingly ambiguous; it is not possible to reconcile the chronology proposed for the exchange of papers between Ramstedt and Poppe with the literature (p. 281); in these and too many other cases the plain historical narrative is confused by unnecessary and unannounced departures from a simple chronological order of events.

The *oratio obliqua* presentation of the bulk of the narrative also throws into unfortunately high relief a number of racial and ethnic slurs that are especially offensive because, lacking documentation of source, it is often anything but clear, again, whether they are the views of Ramstedt a century ago, or of Halén today: "[T]he Cheremis were immensely conservative and superstitious, unwilling to learn anything new" (p. 32); "... being a genuine Mongol he always tried to get away using his traditional ploys" (p. 50); "... work with lazy Mongols was always very tedious" (p. 60). Even when these deprecations are directly attributed to Ramstedt, one must seriously question their utility in a work of scholarship. How does it help us in evaluating Ramstedt's career to be reminded that he was "convinced that their small size, yellowness and the dried-up state of the Japanese was caused by the extremely heavy climate" (p. 239); or that he found the Chinese "unbelievably mean and unreliable" (p. 252); or that "[i]n his opinion, the Koreans wanted to be far too tidy and dandyish" (p. 293)?

Evidence of haste combined with carelessness in preparing this book for the press are too frequent to enumerate. One must pass in charitable silence over the frequently clumsy and often unclear English of the text (after all, if it had been published in Finnish or Swedish most of us would not be able to read it at all). But it is inexcusable that *pin-yin* and Wade-Giles transcriptions of Chinese are switched back-and-forth without warning (e.g., p. 52 note 29 but pp. 181, 257); that in the transcription of a single book-title three of six vowels are written incorrectly, including the vowels in *Chosŏn* 'Korea' (p. 321 has "Chösen", where Ramstedt would have written "Chosŏn", and *khin* for *khun*, Ramstedt's *khun*); and especially that the book's wrapper has Ramstedt's dates incorrect (1873-1959 for 1873-1950).

Nowhere is this book willing to come to grips with the essential conundrum that confronts us in any serious evaluation of Ramstedt's life-work, i.e. the hypothesis of an original genetic connection between all the Altaic languages, including, for Ramstedt, Korean. Mostly the author is careful to shield himself from the anticipated attacks of the anti-Altaists by constantly labelling everything associated with this hypothesis as 'alleged' ("[t]he alleged affinity of the traditionally held Altaic languages and Korean", p. 261; "... word-initial plosives alleged to have been in the protolanguage", p. 281). Nearer the end of the book he seems less cautious: "V. D. Kolesnikova's investigation (1972) ... provided indisputable support for a basic affinity [of the Altaic languages]" (p. 332); but as often in these pages, we are left in

the dark as to what “V. D. Kolesnikova 1972” is or says.¹² And immediately thereafter, on the same page, he again becomes timid on this whole central issue, writing that “[a] pragmatic approach makes its own specific contribution to the treatment of this question”, a *fiat* that this reviewer at least finds meaningless. Attempting to be on both sides at the same time, Halén has predictably fallen down into the middle of the chasm that today still separates the Altaists from the anti-Altaists.

But despite all the problems, questions, and disappointments with which this book confronts us, surely any reader will be brought up short in its criticism by the truly touching final paragraphs in which Halén mourns the demise of Turkology, along with “Mongolian, Tungus and general comparative Altaic studies” at Helsinki University, a process that began in 1984 and that now has apparently culminated in the total obliteration of serious academic interest in any of these fields. He tries to be brave: “[a]ll hope may not be lost ... some progress can perhaps dimly be perceived” are the melancholy exhortations with which he begins and ends his final paragraph (p. 338). Disappointing as this book is, it is not nearly as disappointing *per se* as is the evidence that it offers for that very decline in Altaic scholarship in Finland that Halén mourns. And so must we all.

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¹² Again, as in the enigmatic reference to B. A. Serebrennikov (note 1 *supra*), something has apparently been lost here on the way to the printer. In 1972 V. D. Kolesnikova published two important papers in V. I. Cincius (1972), the first, “Nazvanija častej tela čeloveka v altajskix jazykax” (Kolesnikova 1972a), the second, “K xarakteristike nazvanij častej tela čeloveka v tunguso-man'čžurskix jazykax” (Kolesnikova 1972b). Both taken together certainly document an effective refutation of the claim that “these [Altaic] languages lacked common terms for parts of the body”, and perhaps they are the work of Kolesnikova to which Halén here has reference. But of course the story does not stop in 1970. Doerfer (1988) seized upon both of Kolesnikova's 1972 papers as materials by means of which he might further discredit the genetic hypothesis that Kolesnikova was defending. A decade later his treatment of the body-part terms was soundly refuted, by Manaster Ramer & Vovin & Sidwell (1997-1998). But now this refutation has in turn been strongly attacked (Georg 1999-2000). It appears that we must wait even longer before the tide again turns, this time in a direction that will vindicate Ramstedt's life-work once more.

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