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Telling general linguists about Altaic

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The hypothesis of an Altaic language family, comprising the Turkic, Mongolic, Tungusic, Korean and, in most recent versions, Japanese languages continues to be a viable linguistic proposal, despite various published claims that it is no longer accepted. A strong body of research continues to appear, developing and refining the hypothesis, along with publications that argue against a demonstrated relationship among these languages. This paper shows that many of the arguments against a genetic relationship fail to address the criteria demanded in modern historical linguistics, while many of the responses from proponents of the Altaic theory have failed to address the criticisms raised. We hope that arguments focusing on the real issues of phonological correspondences and morphological systems will shed greater light on the relationship among these languages.

1. Introduction

The Altaic theory holds that the Turkic, Mongolic, Tungusic and Korean (and in most recent versions, also Japanese) languages are genetically related (and, for those who are inclined to accept even wider relationships, that they are more closely related to each other than to any other language family now known). A convenient terminology is to refer to the Turkic, Mongolic and Tungusic languages as 'micro-Altaic'; and to these languages with the addition of Korean and Japanese as 'macro-Altaic.' Altaic is a controversial theory today in the same way that the Algic or Afroasiatic hypotheses, or the hypothesis of the Indo-European character of the Anatolian languages was controversial well into this century. That is, it is accepted and worked on by

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a large number of recognized scholars, but it is either questioned or openly rejected by a number of others.

The decades since World War II and especially the last few years have seen an unprecedented growth in Altaic comparative work as well as an unprecedented vigor in the debate over its validity. Nearly all of this work, pro and con, is usually published in outlets which appeal to the specialists in Central and East Asian languages and civilizations, rather than in ones devoted to general linguistics. It is thus doubly disturbing that a series of recent publications that ARE aimed at general linguists purvey, in tones of authority but without much basis in fact, a series of myths about the current state of the Altaic hypothesis.

Among the more confusing treatments of the Altaic relationship in several major recent encyclopedias of linguistics in English, German and Russian are Tekin (1994), and the relevant articles in Jarceva (1990) and Glück (1993). Tekin (1994) presents Altaic (by which he means Turkic, Mongolic, Tungusic and Korean) as an uncontroversial given and does not allude to Japanese at all. In the case of Glück (1993) and Jarceva (1990), what one would learn depends on which of several articles one reads. For example, in Glück, the (unsigned) article 'Altaische Sprachen' (p. 31) informs us that Altaic is a 'Sprachbund', without any mention of the hypothesis of a genetic relationship (although the references are Benzing (1953) and Poppe (1965), the latter of which deals in detail with the hypothesis of a genetic relationship among the Altaic languages). On the other hand, in the same encyclopedia, the articles (signed by Meyer-Ingwersen) on 'Mongolisch' (pp. 399 f.), 'Tungusisch' (p. 656) and 'Türksprachen' (pp. 657 f.) present these languages without further ado as members of an 'altaische Sprachfamilie' (without mentioning any controversy or alluding to Korean or Japanese), while the articles by Coulmas on 'Japanisch' (pp. 286 f.) and 'Koreanisch' (p. 341) tell us that Japanese shows typological similarities with Korean and allege that it is precisely these features which might point to a genetic relationship between these languages, and put the whole issue in the concept of 'mixed languages'.

Still more contradictory are the presentations in the relevant articles in Jarceva (1990). Here the interested reader will learn that Altaic either is purely hypothetical (Sanžeev, 'Mongol'skie jazyki', p. 306; Gadžieva, 'Tjurkskie jazyki', p. 527) or an ongoing controversy (Xelimskij, 'Tunguso-Man'čžurskie jazyki', p. 523), but that the Nostratic macrofamily of languages, which subsumes Altaic and which even most of its supporters will admit has not gained wide acceptance (certainly nothing like the support for Altaic) is a plain and incontrovertible fact (Dybo/Terent'ev, 'Nostratičeskie jazyki', pp. 338 f.). In same the work, one can also read (Koncevič, 'Korejskij jazyk', p. 240) that Korean is an isolate, but that it is included by some Altaicists within the Tungusic branch of Altaic. While there are many grains of truth in all of these statements, the non-specialist will probably be left

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confused about the present state of the Altaic discussion unless (s)he relies on Kormušin's article 'Altajskie jazyki', p. 28, where a much more balanced presentation may be found.

2. Claims on the status of altaic studies

More consistent, but even more misleading is a series of recent publications by influential linguists in Russia as well as the West telling general linguists that the Altaic hypothesis, far from flourishing, or even being the subject of an intense controversy, is quite simply dead (e.g., Vinogradov 1988, Bateman et al. 1990, Nichols 1992).

Vinogradov (1988: 162) says that Altaic (by which he appears to mean only Turkic, Mongolic and Tungusic) is identified as a language group mostly on typological grounds, 'with only a scant quantity of Common Altaic lexical and phonetic correspondences' [translation ours]. If true, this would mean that, by ordinary standards, there is no case for Altaic (although, curiously, Vinogradov himself nonetheless seems to acknowledge the relatedness of the Altaic languages). Similarly, although in considerably more detail, Nichols (1992: 4) claims that, after eliminating purely typological similarities between Turkic, Mongolian and Tungusic,

> the evidence [sc. for Altaic] was reduced to the pronominal root resemblances and a set of putative cognates. When the cognates proved not to be valid, Altaic was abandoned and the received view now is that Turkic, Mongolian and Tungusic are unrelated (see Unger 1990 [= our Unger 1990b]).

A few lines down she reiterates that 'the lexical stock [sc. of Turkic, Mongolic and Tungusic] proves to offer very few good potential cognates'. She then proposes contact as the explanation for the similarities among the pronouns of these three language groups (together with those of a number of other language groups of Northern Eurasia, roughly those which are sometimes classified as Nostratic or Eurasiatic).

Yet, even a reader with only a slight knowledge of the Altaic problem and its history might feel that a theory to which so many of the best specialists in Central and East Asian linguistics devoted a great part of their scholarly activities during the last century and a half should deserve better than a laconic remark like 'the cognates proved not to be valid'. The idea that Altaic has long been laid to rest is a less than accurate description of the current state of the art. Given the highly interesting, although controversial, new approach to linguistic diversity proposed in Nichols' book, such a statement may prove unduly influential among historical linguists who are not familiar with the current state of Altaic studies.

First, it is obvious, even from a cursory glance at the primary literature

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that the debate over Altaic goes on. A thirty-year campaign of attacks on Altaic by Doerfer (1963–75, 1966, 1967, 1968, 1972, 1973, 1974, 1981, 1985, 1988, 1993, and passim) had not only elicited numerous responses over the years but more recently provoked Miller (1991a, 1991b) into offering a detailed rebuttal together with a survey of the twists and turns in this cycle of the debate (going back to Clauson 1956). The attack itself, of course, continues with undiminished vigor (e.g., Doerfer 1993: 17–35) and has triggered another series of responses (e.g. Andreev & Sunik 1982, Manaster Ramer (1996), Manaster Ramer, Vovin & Sidwell (1997) and Manaster Ramer & Sidwell (1997a). Another vigorous exchange, focusing mostly on the relatedness of Japanese to the rest of Altaic, has just concluded between Janhunen (1992, 1994a, 1994b) on the one hand, and Miller (1994) and Vovin (1994a) on the other. Such criticisms and rebuttals and the continued exposition of more marginally critical (or often merely skeptical) views about Altaic (e.g., Róna-Tas 1991), sometimes in the context of a more general skepticism about the methods of comparative linguistics (e.g. Sinor 1988), cannot be explained by the assumption that the controversy is now over and that the Altaicists lost (or won, for that matter). Rather, the intensity of the criticism has always been, and continues to be, an index of the undiminished vigor of the Altaic comparative work.

It is precisely because we have had more Altaic work in recent decades than ever before done by more people than ever before (T. Tekin, V. I. Cincius, Roy A. Miller, John C. Street, S. A. Starostin, Alexander Vovin, A. V. Dybo, to mention just a few) that we find so much critical literature. A general linguist reading Nichols or Vinogradov might well think that the Altaicists had closed up shop, at the very time when the Altaic enterprise is branching out all over as never before.

Moreover, Vinogradov's and Nichols' work actually misrepresents the very substance of the Altaic debate as it has been framed since the mid-1950's and actually earlier. Pace Vinogradov and Nichols, there is no dearth of good potential cognates. Rather, it is precisely the exuberant abundance of shared vocabulary (some obviously and universally recognized, some more subtle and hence controversial) that has led to the two fundamentally opposed points of view that are widely held: either that this vocabulary is inherited from a common ancestor, Proto-Altaic; or that it involves extensive borrowing in remote prehistory (cf. also the compromise possibilities suggested by Róna-Tas 1974, 1976, 1991 and passim). If putative cognates really were in short supply, or, for that matter, if most of the available resemblances were the sort of superficial similarities to which most linguists give little credence, it would be incomprehensible why the Altaic theory has enjoyed such a long life without sharing the fate of such linguistic seven-day wonders as 'Maya-Altaic' (Wikander 1967–71), 'Korean as Indo-European' (Eckardt 1965), or 'the Dravidian and Manding substratum in Tocharian' (Winters 1988).

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Even those well-known scholars who deny the validity of Altaic, notably Clauson (1956 and passim), Doerfer (1963 and passim) and Janhunen (1992, 1994a, b) or express some skepticism about it, notably Róna-Tas (e.g., 1974 and passim) and Sinor (e.g., 1962, 1988), do not maintain that there is a shortage of putative cognates. Rather what they claim is that most (or all) of these are not cognates but borrowings. It is usually held, both by the supporters and the opponents of the Altaic theory, that the Altaic languages (or at least Turkic, Mongolic and Tungusic) are best studied together, precisely because specialists in the individual languages know that the abundance of shared vocabulary among the languages called Altaic are an important tool for the proper understanding of the (pre-)history of these languages, and of the peoples who spoke them. Regardless of whether one explains all (most, some, few) of these shared items as due to inheritance or to borrowing, the Altaic debate has never been about the existence of truly shared items, but about the proper interpretation of their very abundance. Only in the case of Japanese have some critics of Altaic (notably Doerfer 1974) claimed that the lexical evidence for linking this group to Altaic (as given in Miller 1971) could reflect mere chance. But even here those critics who have actually worked on the problem (and who actually know Japanese), e.g., Janhunen (1992, 1994a, b) agree that many of the Japanese forms in question are true shared forms and not chance resemblances; and again the only question is whether they represent common inheritance or widespread borrowings.

Nichols' claim that 'the evidence [sc. for Altaic] was reduced to the pronominal root resemblances and a set of putative cognates' (1992: 4) is closer to the mark than Vinogradov's, but it still does not convey an accurate impression. In fact, it is quite ironic, because connections involving less basic vocabulary are universally granted by critics and skeptics alike, whereas it is the validity of the pronominal root resemblances which is usually questioned by the critics or skeptics (e.g., Clauson (1969: 16), Róna-Tas 1976). Indeed, Doerfer (1966) rejects the significance of the Altaic pronominal roots in a work in which he admits hundreds of lexical commonalities, and later (Doerfer 1993: 25) seeks to explain away the personal pronouns as belonging 'zu den Babywörtern, offenbar enstanden aus expressiven Urlauten', while much of the anti-Altaic literature avoids dealing with the problem of the pronouns.

However, we know of not a single Atalic scholar critical of the Altaic theory who would agree with Nichols that the pronominal roots represent borrowings among Turkic, Mongolic and Tungusic. Moreover, what is at issue is not merely the pronominal roots, but also the crucial suppletive pattern (seen particularly in the first person forms), which proponents of Altaic regard as crucial evidence of the relatedness of these languages:

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	Early Turkic	Written Mongolian	Tungusic (e.g. Evenki and Manchu)
First person singular:			
nominative	*bän*	*bi*	*bi*
oblique stem	*män-*	*min-*	*min-*
Second person singular:			
nominative	*sän*	*či* (<**ti*)	*si*
oblique stem	*sän-*	*čin-* (<**tin*)	*sin-*

Finally, despite Nichols' claims, the Altaic literature abounds in discussions of many other putative morphological parallels as well. To be sure, the widespread morphological comparisons cited by Altaicists are no less controversial than the rest of the theory, but it will not do to ignore the extensive literature on this topic (see section 5.5).

Given the vast gap between Vinogradov's and Nichols' statements on the state of the Altaic question and the current vitality of the literature, whether pro or con, the question arises of how their extraordinary claims arose. Vinogradov's source turns out to be Krueger (1973), which, paradoxically enough, is actually a (mild) defense of Altaic. Vinogradov's statement about the dearth of lexical and phonetic correspondences turns out to be a misunderstanding, perhaps due to language difficulties. Krueger in fact maintains the validity of Altaic but remarks that little outside of typological similarities is left '[a]s long as the anti-Altaicists insist vigorously that shared vocabulary must only be explained as non-Altaic or extensive borrowing' (p. 572). In other words, Krueger is not claiming any lack of putative lexical cognates, but rather (and this is tautological) that one who assumes that the Altaic languages are unrelated will consider them borrowings and find only typological parallels.

As for Nichols, the only source she cites is Unger (1990b) and again some kind of misunderstanding must have occurred, although presumably for rather different reasons. Unger (1990b) turns out to be a very brief report on an 'Altaic panel' held as part of the Workshop on Linguistic Change and Reconstruction Methodology held at Stanford on July 28-August 1, 1987 and sponsored by the U.S. National Science Foundation. The Altaic panel consisted of Samuel E. Martin, John Whitman, the late Robert Austerlitz, Larry Clark, J. Marshall Unger, and is reported to have been 'particularly impressed by the serious flaws in the Poppe/Ramstedt reconstruction of Proto-Altaic brought to light by Gerhard Doerfer and András Róna-Tas and presented to the panel by Larry Clark' and to have taken 'note of a number of problems in recent efforts by [sc. Roy Andrew] Miller and [sc. John C.] Street to forge links between Japanese and the so-called "other Altaic languages"'.

There is nothing said here about any proof that the putative Altaic cognates are few or even that they are due to borrowing, as the critics of

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Altaic would maintain. In fact, the only strong criticism appears to be directed at the claim that Japanese is Altaic. But as Unger notes in his own (1990a) paper in this same collection, the membership of Japanese in Altaic was not traditionally a claim of the Altaic theory and this issue does not in any case bear on the validity of the claim that Turkic, Tungusic and Mongolic (and also Korean) are related, which is the claim at issue in Nichols' work (and what is most often meant by the 'Altaic hypothesis').

Thus, Nichols' characterization of the state of Altaic studies is based on a misreading of Unger (1990b). Furthermore, the only basis referred to by Unger for the very specific criticism of Altaic to which he does seem to subscribe is a paper by Larry Clark, which was not published in the proceedings, detailing the positions of Doerfer and Róna-Tas. Thus, we are dealing with a fourth-hand report: Nichols (mis)citing Unger citing Clark citing Doerfer and Róna-Tas. But of course Doerfer's and Róna-Tas's works on the topic of Altaic are well known and accessible and so we are in a position to assess their actual positions on the subject. While Doerfer does flatly reject the relatedness of Altaic languages, however, Róna-Tas does not. To be precise, they both clearly agree (as do the proponents of Altaic, obviously) that there are indeed numerous lexical correspondences among the Altaic languages. They also agree (pace the supporters of Altaic) that most of these are loanwords, but, crucially, Róna-Tas consistently refers to a thin oldest layer of vocabulary which he concedes may very well reflect common origin.

Moreover, there is a critical distinction between the hypothesis that the Altaic languages are related and the 'Poppe/Ramstedt reconstruction of Proto-Altaic', so that even if we, like Unger, were to be 'particularly impressed by the serious flaws in the Poppe/Ramstedt reconstruction of Proto-Altaic brought to light by Gerhard Doerfer and András Róna-Tas and presented to the panel by Larry Clark' (1990b: 479), we would not be entitled to reject the relatedness of the Altaic languages. In fact, given Róna-Tas's ambivalent position about Altaic, and assuming that Clark presented it accurately, it is logically impossible that he could have presented evidence against the relatedness of Altaic. Moreover, without access to Clark's paper, we do not even know whether these 'flaws' merely bear on particular problems in the works of Ramstedt (e.g., 1952, 1957, 1966) and Poppe (1960) or the Altaic hypothesis is general.

Finally, an examination of the published papers from the panel shows that the very specific skepticism expressed by Unger (1990b) on behalf of the panel with regard to the Altaic reconstructions could not have constituted a rejection by that panel of the relatedness of the Altaic languages. The only one of these published papers which specifically disputes the Altaic hypothesis is Unger (1990a), though no detailed arguments are given beyond a brief response to Hamp's (1970) critique of Doerfer's (1963–1975) critique of Altaic, and the focus of the paper is on the question of whether Japanese is

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Altaic. It should also be noted that Unger's (1990a) criticisms of the Japanese-Altaic connection are – naturally – directed at pre-1990 work seeking to relate Japanese to Altaic. This work is now viewed by many pro-Altaic scholars as superseded by Starostin (1991), making Unger's criticisms now primarily of historical interest.

A further paper in the same panel, Martin (1990: 484), is also narrowly focused on the Korean-Japanese relationship and does not address the possible relationship of the Turkic, Mongolic and Tungusic families. More recently, however, Martin (1996: 60) states, 'I am somewhat suspicious of the Altaic hypothesis as it is currently enshrined' and 'I do not believe that my 1966 paper proved the genetic relationship nor do I believe that Miller 1971 proved [the Japanese language's] relationship to the Altaic languages' (here using 'Altaic' as a cover term for Turkic, Mongolic and Tungusic, whose relationship is not assumed). Referring to the possibility that these three western Altaic families may go back independently to a Nostratic stage, Martin (1996: 112) adds, 'I am somewhat friendlier toward Altaic, feeling that it is perhaps more than 50% likely (and Macro-Altaic rather less than 50%)'. In personal correspondence Martin tells us that, 'Those figures, of course, are based on gut feelings, not on a sophisticated study of the evidence presented up to the present.'

Thus, Unger's panel can at best be said to have been ambivalent according to the published record as to whether the Altaic languages are demonstrably related, if it addressed this precise issue at all. In addition, whatever the position of this panel might have been, it is our position that it had in any case no claim to be taken as representing the 'received view', as Nichols calls it and this for two reasons.

First, it is striking that the panel was heavily weighted towards specialists in Korean and Japanese and that the only papers actually published are on these languages. No aspect of Turkic, Mongolic or Tungusic is discussed in any detail in any of these papers, something which is highly disturbing, since for almost a century and a half, the whole Altaic controversy focused on the possible relationships between these very three language families.

Second, both Unger (1990b: 480) and the editor of the volume (Philip Baldi, in fn. 1 in Unger (1990b: 482)) acknowledge that the panel was quite unrepresentative of the field, apparently because some of the invitees did not attend, including (by a most unfortunate coincidence) all those who were 'sympathetic to "Altaic"' (in Baldi's words). The very fact that Unger and Baldi acknowledged in print the existence of scholars 'sympathetic to "Altaic"' and sought (however unsuccessfully) to include some of them in the panel serves to confirm that there is no basis to Nichols' claim that 'Altaic was abandoned,' though, as we have seen, even this unrepresentative and apparently un-'sympathetic' panel did not apparently reach such a conclusion.

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3. Possible relations within Altaic

Misleading in a different way is the statement about Altaic in Bateman et al. (1990: 4):

> The original concept of Altaic (comprising Turkic, Mongolian and Tungusic) is now widely regarded as discredited; in contrast, an affiliation of Japanese, Korean and possibly Tungusic is increasingly favoured.

The reference to a supposed specific alternative to Altaic suggests that the Japanese-Korean-Tungusic theory has taken the place of the Altaic theory. Yet, we know of no substantive work on the reconstruction of the phonology, morphology, or lexicon of such a Japanese-Korean-Tungusic family, while those who work on Altaic (e.g. Starostin, Vovin, Miller, etc.) continue to do so without excluding Turkic and Mongolic. As a matter of fact, although Bateman et al. cite Doerfer 1985, Unger 1990a and Whitman 1990 (the latter two in prepublication form), none of these works asserts the existence of a language family comprising Tungusic, Korean and Japanese to the exclusion of Mongolic and Turkic.

To be sure, there are occasional statements in the literature, such as Martin (1991: 269): 'I believe the majority view today would hold that Japanese and Korean are more likely to be related to each other than to any other language and that the historically adjacent Tungusic languages are the likeliest candidates for a further relationship'. However, this appears in a paper concerned only with Japanese and Korean relationships (not with Altaic) and does not refer to any specific proposals, published or otherwise. And we should note that elsewhere (Martin 1996: 112) he considers a relationship among Turkic, Mongolic and Tungusic to be 'perhaps more than 50% likely', so that it cannot be taken to represent any claim about a supposed language family including Tungusic, Japanese and Korean but excluding Turkic and Mongolic.

Finally, even if the Japanese-Korean-Tungusic theory should gain support, it would not necessarily contradict the idea of an Altaic language family, since this grouping could simply be a node in the Altaic family tree. This would be consistent with the very striking similarities (see especially Vovin 1994b) between the numeral systems and morphologies of Tungusic, Korean and Japanese, although it is also possible that similarities shared by this grouping to the exclusion of Turkic and Mongolic are archaisms surviving from Proto-Altaic. Thus, such Japanese-Korean-Tungusic isoglosses are in no way incompatible with the Altaic theory.

Equally misleading to the non-specialist is the claim (e.g. Comrie 1992, Lyovin 1997) that the traditional Altaic theory connects only the western languages (Turkic, Mongolic and Tungusic), and that some combination of Korean, Japanese and Ainu has been marginally associated with the western languages by some scholars. In fact, the status of Korean, Japanese and Ainu differs greatly in Altaic studies. While it is true that the oldest literature on

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Altaic dealt only with Turkic, Mongolic and Tungusic, the fact is that Korean has been an integral part of the Altaic theory for most of the post-war period. Indeed, practically all scholars who have accepted the relationship among the Turkic, Mongolic and Tungusic languages since Poppe (1960) have also included Korean in their definition of Altaic (although in practice many Altaicists have not worked with Korean in any depth until recently). On the other hand, the relationship with Japanese was worked out more recently and still, while accepted by many Altaic scholars (such as Miller, Starostin and Vovin) to be Altaic, is not granted by others (e.g. Tekin and Baskakov).

Finally, the inclusion of Ainu in Altaic is not accepted by most serious Altaic scholars, who in fact reject Patrie's (1982) arguments on this point. One prominent Altaicist (Vovin 1993) tentatively suggests evidence for an Austroasiatic origin of Ainu. Most other specialists continue to view Ainu as a language isolate.

4. History of the debate

Having reviewed some of the most glaring misrepresentations of the current state of the Altaic debate in works intended for general linguists, we will now offer a sketch of the history of Altaic linguistics, based to a large extent on original research into this complex and controversial subject.

In order to understand the roots of the Altaic debate, it is crucial to realize that the concept of Altaic emerged in the 19th century out of the significantly earlier Ural-Altaic theory. The term 'UralAltaic' can easily be misleading, since we naturally assume that it was intended to refer to a putative language family composed of two subfamilies: Uralic (Finno-Ugric and Samoyedic) and Altaic (Turkic, Mongolic and Tungusic, without Korean or Japanese, which were not considered at the time). But that is not how it was intended at all, since the early writers on Ural-Altaic did not treat the three Altaic families as being more closely related to each other than to Finno-Ugric or Samoyedic, nor did they conceive of the latter two as forming a grouping of their own. In fact, the Ural-Altaic theory was proposed, under the label of 'Scythian', in a posthumous and unfinished work of Rask (1834), who also saw a connection to several more distant language groups (Basque, Caucasian, Chukchi-Kamchatkan, Eskimo-Aleut). Moreover, some vague idea similar to that of Ural-Altaic must have existed much earlier, because we find Strahlenberg (1730) offering arguments to refute the view which he says was widely held in his time that all the peoples of High Asia, generally and inaccurately called Ta(r)tars, were one ethnic and linguistic group.

As for the term 'Ural-Altaic', it was introduced (perhaps by Kellgren) in place of 'Scythian or 'Tatar' and referred the Ural and Altai mountains rather than to any linguistic division into 'Uralic' and 'Altaic'. As for the term 'Altaic', when first introduced (around 1845 by Castrén (see Castrén

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1847, col. 234 [= 1856: 75], 1850: 2–3 [= 1862b: 152], 1862a: 318)), it was as yet another synonym for 'Ural-Altaic'. The history of the term 'Altaic' in its modern sense (excluding Uralic) is somewhat obscure. The earliest quotable instance seems to be by Kellgren (1847: 194), from whom it must have been picked up by Schleicher (1850: 63). However, due to Kellgren's early death in 1856 and Schleicher's minor role in the (Ural-)Altaic field, it took some time for this terminology to become widely accepted, and it is possible that, when the term resurfaced some decades later, that it had been coined afresh by someone else. At any rate, we do not find this term in many much later works, such as Müller (1876: 70) and Munkácsi (1884), though it does appear in Müller (1882: 258 and passim, 1890).

Moreover, it is not clear that Kellgren or Schleicher had any clear conception of Altaic (in the sense of Turkic, Mongolic and Tungusic) as a linguistic unit, nor can we be sure who first formulated this idea. To be sure, Klaproth (1823: 295) remarked that, '[d]ie Tungusischen, Mongolischen und Türkischen Dialecte zeigen unter sich einen sonderbaren Zusammenhang' [The Tungusic, Mongolic and Turkic dialects show a remarkable connection with each other], but he spoiled this by finding 'noch merkwürdiger' [even more remarkable] the supposed resemblances between Manchu (a Tungusic language) and the Indo-European languages of western Europe.

Undoubtedly, it was the discovery of compelling evidence that Finno-Ugric and Samoyedic form a unit which came to be called Uralic (Donner 1881; first presented in 1878) which led to the fairly general acceptance of the idea that Turkic, Mongolic and Tungusic formed another unit.

In any case, for a generation or so, the term 'Altaic' was itself ambiguous (the last attestation of it in the sense of Ural-Altaic that we have found is Winkler 1900), thus helping to make the point that the relatedness of Turkic, Mongolic and Tungusic languages was first postulated merely as part of a larger scheme, encompassing Finno-Ugric and Samoyedic as well and that the idea of Altaic in the modern sense took shape only gradually.

However, for three basic reasons there was a lack of consensus about the validity of either of these classifications. First, despite the warnings of authors such as Klaproth (1823: ix–x, 39–40) and Castrén (1847: 5) about the distinction between typological and genealogical classification of languages, much of the work of Ural-Altaic was based on contrasting some of the supposed typological characteristics of these languages, such as agglutination, rigid SOV-syntax, and vowel harmony, with the typologies typical of the two best-known language families of the time, Indo-European and Semitic. Second, the early work, despite warnings that go back to Abel-Rémusat (1820), did very little to establish that the forms being compared were really inherited from a common ancestor, as opposed to being borrowed. Third, the early work did not formulate any specific rules of sound correspondence among the Ural-Altaic languages and so there were no constraints on what forms could be compared, although this absence of constraints did not always

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make it easier to find 'cognates.' As a result of all this, there was considerable skepticism among competent scholars about any of the forms of the Ural-Altaic theory, even though Ural-Altaic, presented as a fait accompli, made its way into numerous encyclopedias and reference works (no more reliable than now) published in the early part of this century, very likely as a result of the wide circulation of Winkler's numerous book-length concoctions – which, however, both proponents and critics of Altaic view(ed) as rather clear examples of how comparative linguistics should NOT be done. This disconnect between the actual state of Ural-Altaic studies and the presentation of it in works of general linguistics was, of course, a neat parallel to what we have described in section 1 as the present situation regarding Altaic.

It is also of capital importance to note that, throughout, the Ural-Altaic grouping was generally regarded as better established than Altaic proper (Turkic-Mongolic-Tungusic), something which was clear both to the enthusiastic proponents (Grunzel 1895) and to the skeptics (Bang 1895).

The same caution, not surprisingly, also characterizes the position initially taken by Ramstedt (1903: III), the creator of Mongolic comparative studies who would soon also become the creator of modern Altaic comparative studies as well:

> Der unter solchen umständen nutzlose meinungsaustausch über die sog. altaischen und uralaltaischen sprachen dauert aber immer noch fort, und hat unter den besten forschern unserer zeit einen berechtigten skeptizismus hervorgerufen.

Indeed, such a position seemed quite justified given the lack of any systematic work on sound laws, which seemed to be the necessary prerequisite before one could be sure that the varying comparisons of vocabulary and morphology could be validated (or rejected). The strongest argument available at the time for Altaic was the pronominal system, which reveals striking similarities between Turkic, Mongolic and Tungusic not, as is often assumed, only regarding the nominative forms, but also in their suppletive inflection patterns (Müller 1890, Grunzel 1895, Ramstedt 1906). But on the other hand much of the existing work was of such poor quality as to discourage rather than promote hope in Altaic (or Ural-Altaic), e.g., the treatises by Winkler, Grunzel, or the embarrassing essays on the 'close affinity' of Ural-Altaic numerals by Halévy (1901).

But the breakthrough that would allow Altaic proper to be put on a firm footing was around the corner, in the form of Ramstedt's (1914/15, 1915/16, 1916/20, 1923 and passim) work on Mongolic-Turkic and Mongolic-Tungusic sound correspondences, although, as is usually the case in the history of science, some of this had been anticipated by others, whose work received less attention because they published in journals (Schmidt 1898) or languages (Gombocz 1905) not widely accessible to the interested scholarly

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public. In any event, it was primarily Ramstedt himself, seconded by Poppe (1926), Pelliot (1925) and a few others, who over the next half-century erected a rather elaborate framework for Altaic, supported by a number of nontrivial and regular sound correspondences and a growing set of lexical and morphological cognates. It would also be Ramstedt (1924), as well as Polivanov (1927), who would independently of each other add Korean to the Altaic family and it was the two of them who also took the first, hesitant, steps towards including Japanese in this modern theory of Altaic (although we must admit that Polivanov regarded Japanese as an Altaic-Austronesian Mischsprache). It was the rise of this (essentially modern) Altaic theory which gradually made Altaic a far more appealing proposition than Ural-Altaic, so much so that in the future the debate would concentrate on Altaic alone.

As the pre-war years wore on, the Altaic theory thus assumed an ever more definite and (to some) compelling shape. Then, however, the rise of Marrism in the Stalin era made comparative linguistics anathema in the USSR, resulting in Polivanov's internal exile and death at the hands of Stalin's henchmen in 1938 and apparently almost cost Cincius her freedom for her incredibly brave book on Tungusic comparative phonology (Cincius 1949), which represents the sole real contribution to comparative Altaic in its time. The war also interrupted Altaic work in the West, and Ramstedt died soon after the war, while Poppe spent the first post-war years rebuilding his research career, now in the US.

As of the end of World War II, no Altaic comparative grammar or dictionary had yet been published and the future of Altaic studies did not look at all promising. Although Ramstedt, Poppe (especially 1926) and Vladimircov (together with Poppe, e.g. 1924) had laid the foundations of a comparative phonology of the Altaic languages, the factual content of which, i.e. the bulk of the lexical comparisons forming its basis, has by and large ever since been accepted even by those scholars who prefer to interpret these comparisons as due to borrowing, the Altaic theory was still widely perceived as unproven (e.g. Deny 1924, Bang 1926). Instead of discussing the crucial issues, much of the work done on Turkic in particular simply ignored the Altaic connections, while on the other hand a certain amount of work comparing Turkic, Mongolic and Tungusic was being done by linguists who were not convinced that these comparisons involved a genealogical relationship (notably Kotwicz 1953).

Yet, despite this ambivalence towards Altaic, no substantial body of work seeking to refute the results of Ramstedt, Poppe, et al. seems to have materialized between Ramstedt's and Poppe's first appearance on the scene and Clauson (1956), although Németh (1912) had come out with some strong doubts about Ramstedt's Turkic-Mongolic correspondences.

Although there were some discussions concerning the relationship of Mongolic and Tungusic (Sanžeev, Kotwicz), the only exception to prove the

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rule seems to have been the work of Shirokogoroff (1924, 1931a, b), whose critique of Altaic was marred by such implausibilities as his insistence (pace Ramstedt, Pelliot and Poppe) on reversing the direction of the sound changes proposed by Schmidt, Ramstedt and Pelliot, so that initial /p/ comes ultimately from /h/ or indeed from ∅, rather than the other way around. The posthumous work of Kotwicz (1953), who had become perhaps the second best-known name in Altaic studies as a result of a series of earlier contributions (1931, 1932, 1936a, b and passim), made matters worse, since it concludes with an inconsistent and confusing statement of the Altaic problem, maintaining that the Altaic languages do form a language family while at the same time proposing that they are not derived from a common ancestor, but then adding that conceivably they are after all so derived (pp. 312–313). As we will see, this kind of confusion would come to inform much of the subsequent debate about Altaic.

Ramstedt's posthumous (1952, 1957, 1966) magnum opus was a mixed blessing. For all its importance and erudition, it was marred by a host of factual errors, which were apparently due to Ramstedt's continued reliance on what had once been a practically photographic memory for linguistic forms in innumerable languages, but was failing by the end of his life; and to his editor's failure to verify the data before publishing it. Still, Ramstedt's work was thought by many (e.g. Collinder 1955: 2, Posch 1956: 77) to have put an end to all doubts and to have made Altaic a fact which no one could reasonably dispute.

But as it happens, it was Posch's (1956) off-hand comments, appearing in a review of a work on Mongolian that failed to admit the validity of Altaic, which sparked off the biggest round of anti-Altaic activity to date. Clauson (1956) was immediately moved to expound in broad strokes the reasons for the opposition to Altaic which was apparently widely felt, he says, in Turcological circles. For the next forty years, there would be a prodigious outpouring of anti-Altaic writings by Clauson himself (1959a, b, 1962, 1969), Ščerbak (1959, 1966, 1987 and passim), Sinor (1963) and various writers who followed them, and above all by the dean of anti-Altaic studies, Gerhard Doerfer (1963–75 and passim). Besides the frequency and intensity of the attacks, what was new was that, while some of these authors did no more than revive the skepticism characteristic of the earlier period, Clauson, Doerfer, Ščerbak and Serebrennikov went much further, seeking not merely to cast doubt on the Altaic theory but to prove that the Altaic languages are unrelated.

It is only apparently paradoxical that the same four decades have also seen an unprecedented growth in the work on Altaic, such as a new comparative Altaic phonology (Poppe 1960), a Tungusic comparative dictionary (finished and published after another decade, Cincius 1975–77), a set of radical changes in the Altaic reconstruction by Illič-Svityč (1963, 1965), Baskakov's (1970b) comprehensive treatment of Altaic, especially the morphology,

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Miller's (1971) and Starostin's (1991) work on Japanese as an Altaic language, etc. Therefore, we shall try to bring into focus the central points in the schism between Altaicists and anti-Altaicists.

5. Objections to the Altaic hypothesis

5.1 *Basic vocabulary*

Gerard Clauson, the instigator of the post-World War II round of the debate (Clauson 1956, 1962) and at the same time the author of one of the most outstanding achievements of 20th century Turkology (Clauson 1972), began with a purely personal argument to the effect that, having been trained in Turkic, he found few if any lexical parallels when he started looking at Middle Mongolian texts. While it will be self-evident to most linguists today that this impressionistic approach to linguistic relationship entertained by Clauson in the early days of his attack on Altaic cannot be accepted, it is clear that the critique of Altaic did not stop there.

Clauson's (1956) second argument involved a rather idiosyncratic adoption of Swadesh's lexicostatistics: he compared the Turkic, Mongolic and Manchu Swadesh lists of 'basic vocabulary' and concluded that these families were unrelated because much of their basic vocabulary did not actually look related. He concluded that whatever lexical parallels existed between these two families were due to rather recent borrowings. Ščerbak (1966) continued in Clauson's footsteps, basing his rejection of Altaic on a survey of certain a priori lexical lists, but unlike Clauson he did not even bother to select anything that could be considered basic vocabulary. Instead he compared three word lists, comprising 'celestial objects and phenomena' (e.g. 'sun', 'moon', etc.), time intervals such as 'year', 'day', etc. and words for domesticated animals.

Other authors have sought to advance more precise arguments against Altaic involving particular subsets of 'basic vocabulary' (often defined quite differently than in Swadesh's work). Of particular importance in this debate has been the role of numerals and body part terms. Time and again, going back to Németh (1912), we find the argument that Altaic languages share few, or no, lower numerals ('1' through '10' or, according to Doerfer (1988) only '2' through '5') or basic body part terms (such as 'hand', 'heart', etc.), that all related languages do have cognate terms for these meanings and that consequently the Altaic languages cannot be related. However, the fact that lower numerals need not be shared between related languages and are not shared by all Uralic languages in particular, has been noted repeatedly in the literature (e.g. Collinder 1935), but this fact has usually been ignored (e.g. Ščerbak 1989: 150–151). On the other hand, when Doerfer (1965, 1974) did acknowledge this lack of shared lower numerals in Uralic and in Afro-Asiatic, his solution was not to give up the opposition to Altaic or even merely to give up this particular argument. Rather, he proposed that Uralic

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and Afro-Asiatic, both of them language families which he has never worked on and whose validity among specialists is unquestioned, are themselves spurious, i.e., that neither Samoyedic and Finno-Ugric, nor Cushitic and Semitic are related!

5.2 *Personal pronouns*

Another related sort of argument against Altaic has often involved the personal pronouns of the first and second person. The observation that Turkic, Mongolic and Tungusic share a perfectly similar set of pronouns for the 1st and 2nd persons, involving not only an outward resemblance of the nominative forms, but also a quite strikingly similar suppletive pattern, had been noticed from the very beginning of Altaic studies (Castrén 1850, Müller 1890, Kotwicz 1936). It seems hard to avoid a thorough discussion of these phenomena when dealing with the mutual relationships of the languages in question, especially when one wants to convince the public that genetic inheritance is out of the question, but nevertheless this is what is done all too often. In fact, if mention is made at all of the pronouns, like in Doerfer 1965, they are usually dismissed, because they are allegedly 'Babywörter' or 'Lallwörter,' similar in a great variety of languages and therefore insignificant for determining genetic relationships. This 'Lallworttheorie' seems to be at best an ad hoc claim to dismiss evidence which clearly stands in the way of explaining all similarities between the Altaic languages as either due to borrowing, mere chance, or universal tendencies towards similar sounds for similar meanings (what German-speaking linguists refer to as 'Elementarverwandtschaft'). To claim that the Altaic pronouns involve this kind of 'elementary resemblance', on a par with the phenomenon underlying such widespread lexical items as 'mama', 'papa', 'ata', 'ana' or the like – these latter ones obviously the true 'Babywörter' – is tantamount to claiming that personal pronouns should exhibit the latter kind of similarities across languages, something which, of course, is not the case. One could as easily imagine a theory of 'baby-genitive','baby-dative', and so on as an explanation for how Sanskrit, Greek, Latin, and so on have such similar morphologies, or 'baby-suppletivism' to explain away the 'be/was' relationship in Germanic, and so on. Any and all of the phenomena usually taken to imply the relatedness of a set of languages would be subject to a similar 'explanation', and no language families would need to be recognized at all.

It is quite significant in this connection to consider the views of Róna-Tas – who, as we have noted, takes a rather skeptical attitude towards the Altaic theory, but whose deep respect for the data has prevented him from declaring it dead and undeserving of further study. Although he treats most of the comparisons made by Altaicists as borrowings, he regards these pronoun systems (after explaining away certain details which might be explained otherwise), as

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(in his usual cautious formulation) 'possible candidates for genetic identity' (1976). Needless to say, Nichols' suggestion, mentioned above, that the Altaic (and more generally Nostratic) pronominal parallels are due to borrowing has not been entertained by any of the critics of Altaic.

5.3 *Phonological correspondences and phonetics*

Another common form of argument against Altaic involves an excessive concentration on phonetic reconstructions, rather than the more important issue of phonological correspondences. The best-known example of this approach has involved the pair of Proto-Altaic phonemes sometimes written */r_2/ and */l_2/. The crucial point here is that there are correspondences between, on the one hand, Mongolic, Tungusic and Korean and Chuvash (a divergent Turkic language) and, on the other, the rest of Turkic:

Chuvash, Mongolic, Tungusic, Korean	Non-Chuvash Turkic	*Reconstructed
r	r	*r_1
r	z	*r_2
l	l	*l_1
l	š	*l_2

Gombocz (1905, 1912–13) proposed that the original sounds here notated */r_2/ and */l_2/ were sibilants */z/ resp. */š/ (i.e. that Chuvash, Mongolic and Tungusic had innovated; Gombocz did not deal with Korean). Ramstedt, on the other hand, proposed that the original sounds were rhotic resp. lateral (i.e. that non-Chuvash Turkic had innovated). Ever since, there has been an endless and essentially pointless debate over what came to be known as 'rhotacism' vs. 'zetacism' and 'lambdacism' vs. 'sigmatism', that is, whether Proto-Turkic (and Proto-Altaic, for those who accept it) had */z/ and */š/ or whether it had some /r/-like and /l/-like sounds, often written */r_2/ and */l_2/. Advocates of the Altaic theory usually reconstruct */r_2/ and */l_2/, while opponents of the theory usually reject this reconstruction and argue that the original phonemes in these case were */z/ and */š/.

Now, it is always nice to deduce the actual phonetic values of protophonemes when possible. But this is of only secondary interest to comparative linguistics, which is more concerned with phonological correspondence than with phonetic identities. Thus for Altaic, the issue is only whether there exists a such a correspondence between non-Chuvash Turkic vs. the other languages. The phonetic values are absolutely irrelevant, as has been noted many times both by Altaicists (e.g., Polivanov 1927 [1968: 161, n. 12]), by skeptics (Róna-Tas 1988: 111) and by avowed anti-Altaicists (Doerfer 1988). Nevertheless, the erroneous view of rhotacism and sigmatism as the 'main pillars of the Altaic theory' (Tekin 1986) is still widely held. Advocates of Altaic undoubtedly have contributed to the undeserved

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prominence of this issue within Altaic studies during the last decades, mostly by tacitly assuming that only the assumption of primary */r_1, r_2, l_1, l_2/ deserves any consideration (cf. recently Starostin 1991).

5.4 *Borrowing*

As we have noted, the primary controversy in the Altaic debate today involves not any paucity of shared lexical and morphological material, but the interpretation of common forms as cognates or borrowings. While all scholars involved in the debate agree that extensive borrowing has occurred among these languages, those who deny the affiliation of the Altaic languages maintain that ALL of the common material is the result of borrowing (or chance resemblances), while those who favor the affinity of Altaic claim that some or much of the shared material is the result of common inheritance and that the traditional tools of historical linguistics allow us to distinguish most instances of the two.

Unger (1990b, naming L. V. Clark as the source) cites a list of criteria, by which Turkic loanwords in Mongolic are said to betray themselves. It may be useful to quote this list here:

> [A]n alleged match between Turkic and Mongolian is likely to be a loan, if:
>
> (1) The M(ongolic) word contains a sound due to a change in T(urkic) that occurred after the period of presumed M-T unity.
> (2) The M word refers to an item present in M culture only after the presumed period of unity.
> (3) A M or a T word can be shown to have been borrowed from some third language group.
> (4) M lacks a counterpart of a T root X and a T derivational suffix Y but contains a derived form combining X and Y.
> (5) A M word that matches a T word in form but has a more restricted meaning coexists with a M word of different form but the same meaning as the T word.
> (6) Two M words are exact synonyms but only one seems to have a match in T.
> (7) The T member of a match can be traced back to pre-thirteenth-century T.
> (8) The T member has a reflex in all T witness languages but in only one M language.

Of these criteria, (3) is obviously a truism which does not need any comment here. Criterion (2) comes nearly as close to a truism as does (3). It is of course correct that especially the earlier period of Altaic research used a host of words for culturally complex items of demonstrably late origin to 'prove' the relationship, a vice which lasted well into the period of Ramstedt and Poppe.

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But mainly thanks to the painstaking efforts of Doerfer (especially 1963–75), most of these have been weeded out successfully. As Bloomfield (1946: 106 and passim) notes (somewhat obliquely), one would reconstruct Proto-Algonquian **paãkesikani* 'gun' and **ekoteewaapoowi* 'whisky' if one did not know that the objects referred to by these terms are post-Columbian. Meillet (1908: 344) is more explicit that the sound correspondences between Greek *kánnabis* 'hemp' and the Germanic words such as OHG *hanaf* are indistinguishable from those found in Indo-European cognates, even though archaeological and philological evidence indicates that these were independent borrowings into Greek and into Germanic from some unknown non-Indo-European language of southeastern Europe.

Of (4) through (6), Unger admits (1990b: 481) that 'there is more room for doubt' (i.e. of their usefulness to pin down loanwords), since uncontroversial language families, such as Indo-European contain clear cognates that exhibit the traits described by these criteria. Unger states that criterion (8) 'may not have counterparts in other language groups', but again we can find examples within Indo-European: within the Celtic branch of I.E. the inherited term for 'daughter' (**dhugHtēr*) appears only in inscriptional Gaulish, where, however, its phonological shape alone rules out any possibility of borrowing from some other Indo-European language.

Criterion (1) would seem more conclusive, but, as we have seen, the one shibboleth within Turkic, which spells the difference between Common Turkic and the only considerably divergent branch of this family – Chuvash – namely /r/ and /l/ vs. /z/ and /š/, fails to offer watertight evidence of the kind required by this principle. Its very nature, not to speak of its chronology, remains basically unclear, no matter how emphatically some Turcologists have stood up for one of the other of the two possible approaches. Of course, it goes without saying that a Mongolic form that clearly shows features of a particular subgroup within Turkic could not be regarded as an inherited cognate in both languages, but in fact, those cases are quite rare and easily weeded out. The majority of the Mongolic-Turkic matches, which according to most critics of Altaic fall under criterion (1), belong to what they call 'the oldest layer of Turkic loanwords in Mongolian', allegedly dating from a time when virtually none of the sound changes which divide the Turkic languages into subgroups had already occurred, with the sole exception of rhotacism/lambdacism, which in turn is said to be reflected by loanwords in Mongolian.

Criterion (7) is clearly false and the most pernicious. The thirteenth century witnessed the powerful emergence of what was soon to be the Empire of the Chingiskhanid Mongols. From that time on, a host of words of clearly identifiable Mongolian origin penetrated the Turkic languages. Thus, a Turkic form of only late attestation may well be a Mongolic loan and must be verified carefully. But Clark/Unger turn this note of caution on its head, suggesting that if a Turkic form is of EARLY attestation, before the 13th

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century, any Mongolic match of it can only be a loan from Turkic into Mongolic (and often thence into Tungusic). Obviously, this 'principle' suggests that any match between Turkic and Mongolic must be a loan from some period and betrays the preconception that there can have been no Altaic proto-language, whatever the earliest data from the languages may show. To assume that all parallels among these languages are in fact loans from some period, and need only to be revealed as such, obviously begs the question of whether they are genetically related or not.

5.5 *Morphology*

Most of the discussion concerning the relationship of these languages has so far centered on the lexicon and phonology as a guide to determining inheritance vs. borrowing. Unfortunately, morphology, which would be crucial in establishing a relationship, has received much less attention over the years. While there is extensive literature on the morphology of each of the language families involved, comparative work involving the full range of languages under consideration needs considerably more study. Indeed there is a plethora of shared grammatical formants among these languages and an important task remaining in Altaic studies is to consider not simply individual shared forms, but determine if we can construct entire shared systems between the families. An example we have already alluded to is the pronominal system in (micro-)Altaic, with its characteristic suppletive patterning.

Although work on Altaic morphology may be said to have begun with Ramstedt (1903), the real first milestone on the way to a comprehensive comparative study of Altaic morphology is undoubtedly Ramstedt (1952). This work, now dated and marred by frequent use of unreliable data, remains a classic, which must be read as a starting point for all students of Altaic morphology. It deals with Turkic, Mongolic, Tungusic and Korean morphology, but not with Japanese. Its Korean data is severely hampered by the inaccessibility to the author of Middle Korean sources, so it is based primarily on modern Korean forms. In addition, much of the data is in error, due partly to the author's failing memory at the time, as well as to the inevitable problems of editing a posthumous manuscript.

Much subsequent work in Altaic morphology concentrated on only Turkic, Mongolic and Tungusic. Many of the specialists in these languages had only a superficial knowledge of Korean and even those who considered Korean to be Altaic, discussed the comparative aspects of Korean morphology in only a cursory manner, if at all. Among those who did address (micro-)Altaic morphology from this perspective, some of the best work is Baskakov (1974) on the pronominal system; Baskakov (1970a, 1981) and Poppe (1977) on the nominal system (primarily on case endings); and

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Poppe (1972), Nasilov (1978) and Baskakov (1981) on verbal morphology.

On the eastern side of Altaic, such work as Martin (1990) shows a large body of common morphology between Japanese and Korean, but does not extend its conclusions to the western languages. This lack of coordination between specialists in the eastern and western languages has been a major stumbling block to Altaic studies both in morphology and other aspects of the endeavor.

Attempts at comparative work dealing with the five families together are of course more difficult and the results have been highly suggestive, but incomplete. Miller (1971), an important work in the conception of Altaic as a family consisting of all five branches, makes a number of insightful observations about morphological matters, but does not attempt to reconstruct a comprehensive system of the kind that would demonstrate a conclusive relationship. Itabashi (1988, 1989, 1991, 1993) focuses specifically on nominal case suffixes and does so primarily from the Japanese perspective, but again shows a number of enlightening points about the relationship with the western languages.

Most recently, Vovin (1994b) contains an extensive summary of shared grammatical formants from the perspective of all of the branches. However, as a review article of another work, it is merely a summary of the reconstructed forms, without further explanation or illustration of the supporting forms in the attested languages. In a more elaborate form, this important work could well serve to flesh out the morphological parallels among these languages and clarify their relationship.

5.6 *Indo-European and consistency*

Another serious problem has been the prevalence of certain a priori assumptions on which many critics of Altaic seem to differ crucially from comparative linguists generally. One is the rather widespread belief that it is legitimate to seek to disprove the relatedness of a given set of languages (Doerfer 1963–75, 1993, Janhunen 1992), a position justly lambasted by Hamp (1970). In his words, 'we can only demonstrate relationship, never non-relationship' (p.189). This, of course, should not be misunderstood as rendering any criticism of Altaistic concepts a priori futile. In should be clear, though, that any such activity can only be directed at the actual claims which are thought to support the Altaic theory, e.g. trying to point out weaknesses in the handling of data, internally contradictory assumptions, etc. to the virtual effect that the theory in question might be regarded by some as 'yet unproven', 'weakly founded' or the like. Criticisms of this kind, which have been accompanying Altaic studies throughout the decades, surely must be welcomed by any adherent of Altaic, since, in a normal science, reliable results are best achieved out of controversial discussions of both the data on

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which a given theory is based and the methods employed to explain them. Pointing out the shortcomings of the existing models of Altaic reconstruction is thus a necessary tool to further any progress in Altaic studies. Since, however, 'disproving' the theory, i.e. actually showing the languages in question to be unrelated, is intrinsically impossible, any attempt to this effect is nonsensical in itself, but the burden of proof that there is a relationship rests of course with the proponents of the theory.

A second dangerous assumption is the blanket rejection of, or at least profound skepticism about, the very idea of reconstructed protolanguages and protoforms (Shirokogoroff 1931b: 90–94, 187–193 and passim; Sinor 1988, etc.) and, among those who do accept such concepts, the rejection of the possibility of comparing reconstructed proto-languages (e.g., Proto-Turkic, Proto-Mongolic and Proto-Tungusic) among themselves (Doerfer 1973), overlooking the obvious fact that within Indo-European studies, for example, Proto-Germanic, Proto-Slavic or Proto-Celtic are reconstructed as well as confronted with each other to achieve better results than could be hoped for if one confined the comparative work to the attested stages of the respective language subfamilies only. A third is the belief that there may be some third choice between borrowing and common descent, that it makes some sense to speak of languages as related and forming families without being derived from a common ancestor (Kotwicz 1953, Doerfer 1966, 1974).

A further such assumption is that features shared by the Indo-European languages (or, less often, some other uncontroversial language family) should be used as a standard against which Altaic is to be judged. Thus, if the Indo-European languages, especially the older ones, such as Sanskrit, Greek and Latin, exhibit certain similarities which the Altaic languages do not, then this is considered in and of itself an argument against the validity of an Altaic language family. The alleged absence of shared numerals, or the impossibility of reconstructing a comparatively complete Proto-Altaic morphological system have often been quoted in support of this. But a closer look at the language families of the world makes it difficult to identify any features which remain constant in every uncontroversial family.

The relationship of Samoyedic to Finno-Ugric might be considered as rather distant, though ever since the times of Otto Donner (in fact, since Castrén's times) Uralic has been a textbook example of a valid language family, as has Hamito-Semitic (Afroasiatic). In fact, the latter two families do show some 'weaknesses' by Indo-European standards, such as a lack of shared numerals, but this apparently has not led to the consequence that these alignments have been abandoned, but rather to the sober insight that shared numerals or, for that matter, any other specific subsystem of language, do not necessarily constitute the indispensable core on which any language family must be based. In fact, we have every reason to believe that numerals should not be a particularly stable semantic field in languages, and the Indo-European example is probably a misleading one. One of the

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activities that promotes language contact is trade, and trade is an activity in which numerals are essential and one must know the numerals of one's trading partners.

But in addition, we find that many of the claims made by anti-Altaicists do not hold even by Indo-European yardsticks. First of all, some critics are often inconsistent when they compare Altaic to Indo-European or some other language family. A good example is Doerfer's (1988) comparison of Altaic body part terminology with those of seven well-established language families (in each case taking three languages or three branches into consideration): Kartvelian (Georgian, Chan (Laz-Mingrelian), Svan), Indo-European (Russian, German, Latin), Semitic (Arabic, Hebrew, Syriac), Dravidian (Tamil, Kui, Brahui), Uralic (Finnish, Ugric, Samoyed), Bantu (Swahili, Kikongo, Nguni), Austronesian (Indonesian, Melanesian, Polynesian). Doerfer claims that in each case we find that the control families share a much higher percentage of the basic body terms than do the three Altaic branches he examines. However, Doerfer rejects any putative Altaic cognates if they exhibit even the most minor deviations from the sound correspondences he rejects, e.g., Turkic **yürek* 'heart' and Mongolic **ǯirüken* 'id.', where the vowels are (in his view) irregular. However, he adopts a more lenient policy in dealing with Kartvelian 'tongue', where the initial of Georgian *ena* vs. Mingrelian *nina* and Svan *nun* is irregular and with Indo-European 'tongue', where the initials of Russian *jazyk*, Latin *lingua* and German *Zunge* also do not correspond in the expected manner. Moreover, Doerfer even has different ways of deciding what is a match depending on the language family he is dealing with. For most language families, he counts as matches only words which have a related form and the same meaning. However, in the case of Uralic and Dravidian, this would yield too few matches for his argument to stand up and so he silently changes his criteria for a match, allowing words with completely different meanings to count (e.g., Finnish *alka-* 'to begin' counts as matching words for 'head'), switching languages at will (e.g., replacing Finnish with Mordvin, Lapp, Cheremis and/or Ziryan) and (in the case of the Dravidian language Brahui) counting forms which are clearly not cognate (as he sometimes himself seems to admit) but merely look-alikes. It is by this kind of gross inconsistency that Doerfer arrives at the conclusion that Altaic languages share fewer core body part terms than Dravidian or Uralic, when in fact by a consistent set of criteria Uralic has one out of 11 ('eye') and Dravidian has two, ('eye' and 'ear').

The number of body part terms shared in Altaic (as with others in fact) depends on how one sets the criteria, but as claimed by Manaster Ramer, Vovin & Sidwell (1998) there may well be a significant number of shared terms for body parts in Altaic. Again, the crucial point is not how many terms Altaic shares for body parts or any other semantic field, since as we have seen, there is no minimum number that is found in all widely recognized

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language families and it all depends on which forms we accept as a match. The issue is rather that the data be evaluated in a consistent manner.

5.7 *Which version of Altaic*?

An even more serious problem has been that some critics of Altaic do not attack the actual Altaic theory, but invent their own 'Altaic' and criticize it instead of the Ramstedt-Poppe theory or any subsequent version. Doerfer (1963–75, vol. I: 58–63, 97–105) offers his own set of phonological correspondences which are supposed to hold within the set of words which he considers to constitute the oldest layer of vocabulary shared by Turkic, Mongolic and Tungusic. This is fine so far as it goes, but when it comes to criticizing the Altaic theory, Doerfer has for three decades continued to use exclusively his own proposed correspondences, not those of Ramstedt or Poppe, to judge whether particular Turkic, Mongolic and Manchu-Tungusic forms are matches. Thus, Doerfer (1988) tries to show that the Altaic languages do not share related basic body part terms, but his case in part depends on not allowing the sound correspondences proposed in the Altaic literature but substituting his own.

While we cannot go into all the details of how Doerfer's straw-man system differs from that of the Altaicists, one aspect may serve as an example. Poppe (1960) proposes a phonological system in which several different initial consonants in proto-Altaic (including *d-, *ǯ, *n- and *ñ) developed to *y- in proto-Turkic. In Mongolic initial *d- (except before *i), *ǯ and *n- remained unchanged, while initial *ñ- merged with *n-. Thus Poppe (1960) cites such correspondences as Turkic *yel* 'Mähne' = Mong. *del* 'id.'; Turk. *yük-* 'Last, Traglast, Gepäck' from *yü-* 'transportieren' = Mong. *ǯöge-* (Poppe (1960: 28) cites *ǯüge-*) 'transportieren, fahren'; Turk. *yämä* 'noch, auch, ebenfalls (ursprünglich wahrscheinlich 'zusätzlich') = Mong. *neme-* 'hinzufügen'; Turk. *yaz* < *$yār_2$ 'Frühling' = Mong. *nirai* < **nārai* 'neugeboren, frisch'. As a result, there are no native Turkic forms beginnings with *d-*, *ǯ n-* (aside from the anomalous pronoun *ne* 'what?'), or *ñ-*, and Turkic forms with initial *d-*, *ǯ-* and *n-* (other than *ne*) are borrowings or secondary developments.

Doerfer (1963–75, vol. I: 97), which deals primarily with Mongolic and Turkic, sets up a series of correspondences between these two families, which he attributes entirely to borrowing, in which 'Urtürkisch' initial *q-, *k-, *t- (and *d- (*ð?)), *p-, *b-, *č- and *s- correspond only to the same values or to phonemes differing only in voicing in Mongolic. In addition, he lists an unstated 'Urtürkisch' form ('später *ǯ') corresponding to Old Turkic *y-* and Mongolic *ǯ-*. Thus, Doerfer can (rightly) dismiss attested Turkic forms with initial *d-*, *ǯ-* and *n-* (other than *ne*) as borrowings or secondary developments, but he is relieved of the need to account for any Turkic equivalents to Mongolic forms with these initials. (It is ironic that by insisting only on

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correspondences that are phonetically similar, Doerfer seems to be in the same camp with the more enthusiastic proponents of long-range relationships, who look for superficial phonetic similarities among modern forms, rather than for regular phonological correspondences that may have phonetically quite different realizations.)

Since Doerfer's straw-man correspondences are designed to account for loanwords between Turkic, Mongolic and Tungusic, the fact that they sometimes do not allow us to relate some of the basic vocabulary is not surprising. We are dealing with a circular argument: Doerfer selects sets of the most trivial phonetic correspondences among these language groups, based precisely on words which are most clearly borrowings and then argues that Altaic is an illusion because, predictably, under these correspondences, we cannot relate the words for 'hand' or 'heart', or the like. By the same logic we could 'disprove' the relationship of Germanic to other Indo-European languages by refusing to admit the occurrence of Grimm's Law or Verner's Law. It would then be impossible for anyone to treat English *father*, *mother* and *brother* as cognate with, say, Latin *pater*, *mater* and *frater* and we would only recognize obvious loans, such as *paternal*, *maternal* and *fraternal*.

This point is particularly important since Doerfer (1963–75) begins his attack on the Altaic theory, pointing out rightly (v. I: 63) that, '[d]ie Beweislast obliegt dem Behauptenden, nicht dem Leugnenden' (The burden of proof rests with the one who asserts [the hypothesis of relationship], not with the one who denies [it]). Yet it follows from this generally accepted reasoning that one can only deny a case that has actually been asserted.

6. Conclusion

To sum up, it seems clear that most of the principled and methodological arguments of the opponents of the Altaic theory have little merit and do not in almost any particular case rise to the standards of discourse in modern comparative linguistics. The status of Altaic, although not quite the same as that of families like Algonquian, Romance, or Indo-European, is perhaps comparable to that of Austro-Asiatic or Afro-Asiatic: large families whose relatedness is suggested by a great deal of evidence, but where a great deal of work still remains to be done on even some very basic questions.

But if this is the case, then how can we explain the different perceptions of the validity of Altaic on the part of a significant number of linguists? To a large extent it is due to little more than the unusual tenacity, energy and outspokenness of the opponents of the theory, together with the equally unusual inability of its supporters to deal decisively with them. There have only been a few attempts to answer the anti-Altaic attacks and these tended to concentrate on the most superficial problems. Poppe, for example, a firm supporter of the Altaic relationship, who was responsible for enormous

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progress in the field, was surprisingly reticent in his response to its opponents. To be sure, Gercenberg (1971), Andreev (1971) and Ligeti (1971) did note that the Clauson-Ščerbak method of comparing word lists in search of dissimilarities is irrelevant to the question of whether the languages are related, because similar lists of Indo-European languages show the same kind of lexical divergences as the Altaic ones. Ligeti also demonstrates the wealth of shared vocabulary that the Altaic languages do exhibit in the semantic fields that Clauson compared. However, there has been little response so far to the much more sophisticated arguments of Doerfer (1988).

Yet all this is not to say that the great Altaic debate has been wholly unproductive. It may in fact have contributed to two developments, one clearly positive, the other at least not entirely negative. The first is that, while the controversy over the big picture has raged at one level, a considerable amount of detailed technical work was done at a different level (even less noticeable to nonspecialists) which resulted in Altaicists as well as their critics giving up a number of questionable proposals in earlier work. For example, Poppe (1974) treats as loanwords from Mongolic into Tungusic a number of forms he formerly (1960) treated as cognates. Of course, this does not imply any weakening of the Altaic theory, or of Poppe's commitment to it. In this manner, the actual Altaic theory created by Ramstedt and Poppe, has thereby been refined and developed by the further work of scholars like Illič-Svityč, Starostin and Vovin.

On the other side of the ledger, we find that Doerfer, in particular, argues strongly against Clauson's naive assumption of recent borrowings and at length accepts some of the central sound correspondences of Altaic. To be sure, he still does not accept the central premise that these sound correspondences reflect the different ways in which Turkic, Mongolic, Tungusic, Korean and Japanese developed from Proto-Altaic and offers instead the idea that, for example, Mongolic borrowed the forms at issue not from any attested variety of Turkic, nor yet from Proto-Turkic, but something he calls 'Ur-Türkisch.' This is a preprotolanguage which would have the phonological distinctions needed to account for the forms found in Mongolic (and in Tungusic), e.g. an initial */p-/, as such only to be found in some Southern Tungus languages such as Nanai (the intermediary stages of the development 'Urtürkisch' *p- > *f- > h- > ∅- are found in a variety of Altaic languages, among them Manchu, Middle Mongolian, several archaic Mongolic languages; Turkic data alone, however, would only allow us to reconstruct Proto-Turkic *h-). As noted by Poppe (e.g. 1965: 153), of course, there is no Turkic evidence for this Ur-Türkisch, only Mongolic (and Tungusic) evidence and concludes that, 'What Doerfer reconstructs as Proto-Turkic is actually much older than Proto-Turkic. It is nothing but common Altaic'. Nonetheless, the differences between Doerfer's reconstruction and that of the Altaicists, while by no means trivial (pace Poppe 1983: 272), have been shrinking.

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In conclusion, opinions continue to be divided about the validity of the Altaic theory, but even a cursory look at the relevant literature (including the very sources cited by Vinogradov and Nichols) reveals that the issue is far from being settled in the negative. The debate which began with Schott's (1836) response to Abel-Rémusat (1820) is now in yet another cycle, whose beginning may be dated to the publication of Clauson's (1956) critique of the relationship between Turkic and Mongolic, although today the principal focus seems to be the relationship of Japanese to the rest of Altaic. The frankly poor quality of Ramstedt's Korean-Altaic comparisons would have made at least a superficial criticism very easy, but this was precluded by the lack of scholars interested in Altaic and trained in Korean. At least this latter situation has changed fortunately during the last decades. And some convergence appears to be occurring e.g. in the views of Janhunen who, after strongly rejecting Altaic (1992, 1994a, b), now accepts the affiliation of Mongolic and Tungusic (1996).

The work of Illič-Svityč (1963, 1965) and Starostin (1991), among others, has moved Altaic studies considerably beyond the stage it had reached with Poppe (1960). This is not to minimize Poppe's own accomplishments, of course, without which the later work would not have been possible. Especially the recent book-length treatise by Starostin surely displays some serious weaknesses, including a number of errors in the forms cited, not to mention its failure to cite (almost) any alternative positions on even the most controversial issues of Altaic studies.

Nonetheless, for those who accept Altaic, the work seems a major step forward on a number of points. Its reconstruction of three series of obstruents and affricates, for example, makes a significantly more plausible case than Poppe's formulation. In addition, there is a more comprehensive integration of Korean into the overall Altaic development. The treatment of Japanese in light of these revisions in the reconstruction of the other branches also marks an improvement over Miller (1971).

Comrie's (1993) review of Starostin (1991) points out that when Starostin cites competing (Austronesian vs. Altaic) etymologies for a Japanese form, he frequently does no more than dismiss the Austronesian explanation as 'doubtful,' and therefore adopts the Altaic connection by default. Comrie is right to note that, by the same logic, we could simply dismiss the Altaic etymology without discussion and prefer the Austronesian. But Vovin (1994b) compares the Altaic and Austronesian analyses of the affiliation of Japanese in great detail and discusses precisely the specifics of how the Altaic affiliation of Japanese gives a stronger accounting of the facts than the Austronesian. Therefore this weakness in Starostin (1991) is best remedied by reading the book in conjunction with Vovin (1994b). Comrie also criticizes the large number of errors in Starostin's citations. Here Comrie's review is very helpful in pointing out some of the incorrect forms Starostin has used. But notwithstanding these weaknesses, Comrie's review fails to show what

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the book does accomplish and the real strengths of its progress over earlier work.

Weeding out questionable etymologies is the first and foremost task for all researchers in the field, be their inclination pro- or anti-Altaic. The future of Altaic studies and of course the status of Altaic as a valid language family, will then depend on the stock of etymologies which survive this process: good etymologies, with all they imply, are what a valid language family is built upon. Despite the great amount of work which has been done to this effect (and most of the credit here surely must go to Doerfer 1963–75), this enterprise still has far to go, especially as regards Korean and Japanese. Many more researchers, though, are needed with a good knowledge of both Micro-Altaic and Korean/Japanese. At the same time, the rather muted response to the attacks on Altaic, by Poppe himself and by others, is no longer the order of the day. Much clarity will be attained with the appearance of a series of responses to those attacks (e.g. Manaster Ramer 1996, Manaster Ramer & Sidwell 1997b, Manaster Ramer, Vovin & Sidwell 1998). But even on the basis of existing work, it is clear that the Altaic field is flourishing and is the source of promising new research. Pronouncements of the demise of the Altaic hypothesis, like that of Mark Twain, have been greatly exaggerated.

Finally, we would like to note that the authors of this paper come from different backgrounds and do not fully agree among themselves about the ultimate fate of the Altaic hypothesis. Manaster Ramer, Michalove and Sidwell accept the relatedness of macro-Altaic but question many specific points of the reconstruction of Proto-Altaic as it currently stands. Georg remains skeptical about the geneticity of either micro- or macro-Altaic and is actively exploring the hypothesis of an areal solution to the problem, but agrees with the criticism voiced here about the methodology of some opponents of the Altaic theory.

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