Review Article

The Role of Paradigmatic Morphology in Historical, Areal and Genealogical Linguistics

Thoughts and Observations in the Margin of Paradigm Change. In the Transeurasian languages and Beyond (Robbeets and Bisang, eds.)

Stefan Georg
University of Bonn
stefan.georg@bn-online.net

Abstract

Paradigmatic morphology is a central and crucial concept for several branches of comparative linguistics. The observation of shared paradigms in languages which were not suspected of having a common ancestry stands at the cradle of modern genealogical linguistics and dominates the discussion(s) about not firmly established or merely putative language families or phyla to this day, the very different morphological techniques different languages use for the formation of paradigms mark the beginning of language typology, now a major pillar of the language sciences, and the question, to which degree languages—closely, distantly, or not at all related with each other—may borrow morphological paradigms (part or whole) from each other or might have done so in the past (which, if true and not properly detected, might lead to superficially persuasive, but factually erroneous, claims and hypotheses of genealogical relatedness) continues to be an important theoretical and practical issue in comparative linguistics.

The contributions to this volume address, i. a., all of these questions and areas and offer much food for thought about historical morphology, areal linguistics and, above all, language classification—going far beyond the “Altaic hypothesis”, which figures prominently in some of them.
Keywords

paradigmatic morphology – historical morphology – areal linguistics – language classification – Altaic hypothesis

Paradigm Change. In the Transeurasian languages and beyond (2014), edited by Robbeets, Martine and Bisang, Walter. Amsterdam/Philadelphia: John Benjamins, is a very interesting and well-edited volume, which brings together some first-rate pieces of scholarship in the realm of historical and comparative linguistics. The scope of the individual papers encompasses data-intensive historical studies of paradigmatic morphology in key languages and (hypothetical and established) language families, as well as more theoretically oriented investigations into the very notion of paradigms. The languages covered range from Africa and South-East Asia to Greek and Hungarian, with a special emphasis on those languages of Central and Inner Asia, which are often—but not universally—classified as belonging to the ‘Altaic’ family (or area) of languages.

1 Theoretical issues, South-East Asia and Africa

The roster of papers is opened by W. Bisang: On the strength of morphological paradigms: A historical account of radical pro-drop (23–60), which asks a concrete question: why do some groups of typologically isolating languages, i.e. languages poor in or even devoid of paradigmatic morphology, do not have obligatorily expressed subject and/or object pronouns (i.e. show the phenomenon of ‘pro-drop’—a term which is here not used with its generativist/universalist connotations), whereas otherwise typologically very similar languages do require such pronouns? Bisang’s examples for the former, pro-drop, type of languages come from East and South-East Asia (Classical and Modern Chinese, Khmer, Hmong, and Thai, representing the four language families Sinític, Mon-Khmer, Hmong-Mien and Tai), whereas the pronoun-requiring cases are West-African languages (Fongbe/Kwa and Yoruba/Defoid are his most important examples). His hypothesis is that this markedly different behaviour can be explained historically—the West African languages (which are all, justified or not, united in the vast Niger-Congo language phylum) are derived from (an) ancestor language(s) which had morphological (and thus obligatory) person (and number) marking on verbs, whereas all the mentioned Asian languages do not, or, in other words, they show no evidence for person/number paradigms (with bound morphemes) at every conceivable time-depth. The key factor here is the ‘strength’ of the erstwhile paradigms, due to which they remained relevant.
(as obligatory subject and object pronouns) in some of the descendant languages even after the bound morphology itself was lost. It is tempting to paraphrase a well-known dictum of historical typology here and to say that, sometimes, today’s syntax may be yesterday’s morphology (cf., of course, Givón 1971 for the opposite claim). When such paradigmatic morphology was not present in earlier stages of the languages, this strength could not make itself felt and the descendant languages show pro-drop characteristics.

Bisang’s scenario is certainly inherently plausible and in general well argued. He discusses some logical alternatives (e.g. universalist approaches, in section 3), rejects them and settles for his historical scenario (with a very elaborate definition of the concept of “strength” with respect to paradigms in section 4).

It goes without saying, though, that the general plausibility of this scenario has to be backed up by a thorough discussion of the historical facts (as far as they are known or recoverable) in the languages and families under discussion. In the African case, Bisang refers directly to the discussion on Proto-Niger-Congo, or, more specifically, the question whether this (remote) ancestor of (i. a.) Kwa and Defoid is to be reconstructed with or without person/number morphology (of the prefixing Bantu-type). This is of course the Güldemann—Hyman controversy (cf. Hyman, 2004, pro morphologically complex Proto-Niger-Congo, and Güldemann, 2010, pro an analytically structured proto-language, and, e.g., the morphologically complex nature of the Bantu verb as the result of grammaticalization processes).

After discussing these two approaches, Bisang declares a decision between them unnecessary, because it is, according to him, clear enough that morphological agreement patterns are a recurrent and time-stable phenomenon in the whole history of Niger-Congo, which could always exert its ‘strength’ to keep the overt expression of pronominal constituents alive. Turning to South-East Asian languages, where Bisang is on his home turf, he shows that Proto-Sinitic did have some morphology, but no person/number paradigms, and that the same holds for Mon-Khmer as well. But for the larger (and uncontroversial) Austro-Asiatic family (roughly: Mon-Khmer plus highly ‘pronominalized’, in fact morphologically super-rich, Munda) a similar question poses itself, namely: what is ‘older’—the relatively poor morphology of Mon-Khmer or the highly complex structures of Munda? Here the main protagonists of the two opposing views are Zide/Anderson (2001, Munda-like morphology is to be reconstructed for proto-Austro-Asiatic) and Donegan/Stampe (2004, Proto-A-A with little or no inflectional morphology)—if one knows that the former two scholars are specialists in Austro-Asiatic languages of the Indian subcontinent, i.e. Munda, whereas Donegan and Stampe work predominantly on South-East Asian languages of the family, it is hard not to think of ‘Teeter’s law’ here—it is therefore hoped that the discussion will go on. Bisang himself, though,
thinks again that a decision between these two approaches is not relevant for his purposes, since (with Proto-Austro-Asiatic being too remote in time to play a role here), Proto-Mon-Khmer was clearly devoid of ‘strong morphological’ paradigms when it entered into the linguistic contact situations, in which its historically known descendants are found.

I have to say that I missed one further major language family of the region from his discussion—Tibeto-Burman. This language family is actually, after Niger-Congo and Austro-Asiatic, a third one for which a strikingly similar discussion about a morphology-rich or an ‘a-morphous’ proto-language has been going on for some time—with largely isolating and area-typical languages in South-East Asia (Burmese, Lahu, Karen languages, to name but a few) and morphologically highly complex Kiranti languages in the ‘Indosphere’ (Eastern Nepal), or Gyarongic languages in Southern China—(for the position that Proto-Tibeto-Burman, and maybe even Proto-Sino-Tibetan, was characterised by a rich verbal morphology, cf. Jacques, 2006; for the opposite position cf. LaPolla, 2003)—again, it is, with all due respect for both scholars’ expertise, difficult not to catch a fleeting glimpse of the name of Karl Teeter here again, but, on a serious note, the questions Bisang asks should be asked for the Tibeto-Burman languages of South-East Asia as well. I am, though, aware that it may be asking a lot to expect the inclusion of yet another language family into the thorough and learned discussion of this long paper, many highly lucid and well-argued sections of which (among them one on the phonological aspects of the phenomena) cannot even be mentioned here, but I hope that this will be taken as a sign of the highly inspirational nature of this fine paper, which should, and doubtlessly will, spawn more thoughts and work on the matter.

J. Nichols: Derivational paradigms in diachrony and comparison, 61–88, is a demanding read. The author attempts to show ways beyond the evaluation of word lists for the subclassification of established language families. For this, she looks at ‘mini-paradigms’, viz. those of (de-) causativization and the three-cell paradigm of posture verbs (static, dynamic and semantic causative). From the answers to four questions (What is the derivational morphology (e.g. causativization/decausativization)? What is the base of each derivational paradigm? Are the [whole] words cognate? Are the roots cognate?) she then plots a certain set of ‘trees’ (for Germanic, Slavic, Nakh-Dagestanian, Romance, Turkic and Uralic languages), which are said to depict the ‘relative distance’ between the languages of these established language (sub-) families and discusses the outcomes in order to see, how well these square with the classifications as established by different, traditional, methods. The pictures the different trees paint differ widely (and do contain plots of ‘distance’, which are actually contradicted by the real subclassifications of these families), but on the whole Nichols views them as producing ‘remarkably good phylogenies plus
some geographical information (p. 76); at least better ones than those produced on the basis of the notorious ‘Swadesh 100 words’ list. Having thus established the usefulness of such a small data set for the subgrouping of established families, she turns to the question, whether this could also be useful in long-range comparisons or, then, phylum recognition. For this purpose, a combined ‘tree’ with all previously discussed Eurasian languages (and both causatives and posture verbs in the picture) is plotted—the best pieces of information it gives are, though, of a geographical nature, rather than revealing the true genealogical makeup of the sample; on the other hand, Nichols is probably right when she says (p. 86) that ‘in the long run geography is probably more interesting than information about descent’. The editors of this volume, in their foreword, describe Nichols’ approach as one, which ‘could identify deep resemblances between languages at relatively remote time-depths’. I, for one, am unable to read Nichols’ paper as supporting such an optimistic conclusion.

The one paper in this volume which is concerned with the very notion of ‘paradigm’ itself and ways of distinguishing between paradigms and non-paradigms in a meaningful way is B. D. Joseph: On arguing from diachrony for paradigms, 89–102. Illustrated with examples from Greek (various chronological stages) and Latin, the author asks the question, whether paradigms (understood as organized sets of inflectionally related forms) are necessary constructs, or rather secondary entities without any meaningful ‘reality’. Arguments in favour of the theoretical justification (and usefulness) of the notion of paradigm are found on the historical pane, and, more precisely, in the realm of analogical change, which, quite often, operates between morphological forms, which constitute a paradigm, leaving others, outside of the paradigm, untouched. Evidence for such intra-paradigmatic analogical processes comes from Latin (e.g. the paradigm-internal leveling of honōr [< honōs], honōris, but unaffected extra-paradigmatic honestus) and Greek, showing that there are indeed processes which pertain to (and thus define) the ‘within’ of paradigms, and by consequence also their ‘without’, showing that (true) paradigms are indeed bound by a ‘privileged sort of connection’, which is not, e.g., present in sets of derivationally obtained forms. True paradigms thus are a relevant construct, they have ‘psychological reality’ for speakers, which is also necessary for the wholesale borrowing of paradigms (which does happen between languages, contrary to what is often claimed, and which is a recurring theme in this collection of papers). This approach to paradigms as the main domain of analogical processes is certainly not entirely new, but Joseph’s presentation of it is particularly lucid and enlightening.

L. M. Hyman, who already figured prominently in Bisang’s contribution, brings us back to the African continent with Reconstructing the Niger-Congo Verb Extension Paradigm: What’s Cognate, Copied or Renewed?, 103–125.
The extensions of his title are Africanist parlance for deverbal verb-forming (valence-increasing, valence-decreasing and valence-neutral) derivatives. Hyman asks the question, whether a system of such extensions is reconstructable for the putative proto-language of this vast phylum, and whether this, if present, can be used as a criterion for inclusion in or exclusion from this macro-family. The problem is thorny, and its solution has to struggle with, as Hyman points out, insufficient observations of regular phonological correspondences in the past, the presence of systems similar to that of Niger-Congo in the other macro-phyla of the African continent (Afro-Asiatic, Nilo-Saharan, 'Khoisan'), which may render comparisons too vague to be really group-defining, and demonstrated cases of cyclically renewed—and even some borrowed—extensions in some languages. The main conclusion of Hyman’s detailed and data-rich discussion is that, still, noun-classes remain the best criterion for membership in Niger-Congo, and that verb extensions are also useful, but require a great deal of scrutiny before they can be used as indicative for family membership. Generally, there are many lessons to be learned from this and similar studies, especially for fields in which the genealogical classification is still, to say the least, in flux, and where arguments from morphology are sometimes found, which suffer ostensibly from the negligence of some of the pitfalls Hyman manages to expose here so carefully. There is no reason to deny that I have the perennial (and, I must say, profoundly misguided) hypothesis about an ‘Altaic’ phylum in Central and East Asia in mind, on which I will have some more words to say below.

2 Hungarian, Japanese, Korean

A distinctive sub-class of verbs in Hungarian are the so-called ik-verbs, the object of É. Á. Csató: Perceived formal and functional equivalence: the Hungarian ik-conjugation (129–139); these verbs are characterized by a suffix -ik in the 3rd person singular (as opposed to -ø in the ‘indefinite’ and -i in the definitive/ objective conjugation). Origin and primary function of this conjugation type (and its characteristic suffix, which, however, is not its only anomaly—according to Csató it is, because of these, ‘not well integrated into the Hungarian grammar’, 133) have been the object of numerous studies and hypotheses in Hungarology, but a consensus solution seems to be elusive until today. For her solution, the author departs from the observation that a sizable group of Hungarian loan-verbs from Turkic (as listed by Róna-Tas/Berta, 2011) were inserted into the system of Hungarian as (intransitive) ik-verbs. Yet they do not contain any suffixal element in the donor language, which could explain the
The role of paradigmatic

-ik-suffixation in Hungarian (Turkic loan-verbs with a suffix -(V)g or -(V)k in the donor language are replicated with a segmental reflex of this suffix). Csató’s explanation departs from the observation that Hungarian reflexive verbs with the suffix -(V)kVdik seem to contain a reflexive marker *-ik, which may play a role here, but not one of direct borrowing, but rather of ‘perceived formal and functional equivalence’. There is indeed a certain degree of functional and formal similarity between Hungarian -ik and the Turkic suffixes in question, which could be responsible for this ‘carry-over’ influence on Hungarian (as opposed to ‘take-over’, which would imply a direct adoption of a donor suffix in the recipient language). This explanation is certainly intelligent and interesting, but the fact that most ik-verbs are, at the end of the day, not reflexive, may be regarded as a perennial obstacle to such an interpretation (which, I should add, is certainly one of the better approaches presented so far).

The long and rich paper by S. Ko, A. Joseph, and J. Whitman: Comparative consequences of the tongue root harmony analysis for proto-Tungusic, proto-Mongolic, and proto-Korean (141–176) sticks somewhat out of the remainder of this volume, in that it is the only contribution which does not deal with morphological paradigms at all (although the domain in which the discussed phenomenon manifests itself most clearly is of course the realm of paradigms)—its object is phonology, more precisely vowel harmony, the hallmark of (not all, but certainly most) of the languages of the Uralic and ‘Altaic’ groups and language families. While the traditional ‘Altaic’ language families have for decades been described as displaying (some variant of) palatal or front-back harmony, the picture began to change about the 1980s, when advances in phonology (and phonetics) allowed to restate especially Tungusic harmony in terms of tongue-root based systems, which was soon seen as the dominating principle in most of Mongolic as well (not in Kalmyk/Oirat, which continues to be described as possessing a true front/back-based system). The authors offer a rich array of data and come to the conclusion that RTR (Retracted Tongue Root) harmony systems are to be reconstructed for the three proto-languages mentioned in the title of their contribution. Against this background, it would be more likely than not that a possible common ancestor, ‘Altaic’, if it is to be accepted, would have to be reconstructed with a similar system, because they find that ‘the shift RTRH to PH (Palatal Harmony, St.G.) is better motivated than a shift in the opposite direction’ (170). On the other hand, the authors also state that Tongue Root Harmony has been observed to spread rather easily across language family boundaries (e.g. in Africa), so the question may be in need of further investigation, and, preferably, while the palaeoasiatic families Chukchi-Kamchatskan, Nivkh and Yukaghir do play a role in this paper, with due consideration of harmony features in Uralic languages as well. The paper offers
very interesting insights, including a rather scathing negative assessment of
the vowel reconstructions in Starostin et al. 2003 (the authors remain neutral
on and open for different positions towards the validity of the ‘Altaic’ hypoth-
esis, although they adopt it as a working model for some of their historical
considerations).

J. M. Unger: *Old Japanese bigrade paradigms and Korean passives and caus-
atives*, 177–196, is another contribution to the history of a certain type of verb-
paradigm, the so-called *bigrade* (or ‘*nidan*’) verbs in Japanese (according to
the traditional *katsuyōkei* system of Japanese ‘conjugations’). The merits of
this paper deserve to be assessed by scholars who are more at home in Japa-
nese and Korean historical linguistics than this reviewer can claim to be, but
it should be mentioned that Unger argues in favour of reconstructing a mor-
pheme *-gi*, which he views as the ‘*ultimate source of the Japanese passive*
(189) and as historically identical with the Korean passivizer *-gi*, under the
assumption of a Japanese-Korean genealogical unity, and that he extensively
discusses variant opinions of other scholars about this part of Japanese histori-
cal morphology.

3 The Altaic Languages—Language Family or Convergence Area?

It cannot be avoided to have a more extensive look at M. Robbeets: *The Japa-
nese inflectional paradigm in a Transeurasian perspective*, 197–232, since this
paper carries a rather far-reaching ambition, but also, so I think, presents its
points in ways which might mislead readers, who are not privy to the often ar-
cane field of comparative ‘Altaic’ linguistics, more than it will enlighten them.
Nobody who knows the work and the position of the co-editor of this volume
will be seriously surprised to find here another paper from her pen, which pur-
ports to make yet another point in favour of the genealogical relationship of
Turkic, Mongolian, Tungus, Korean and Japanese (‘Altaic’1) Actually, I am yet
to see a piece of writing by this author which does not try to make this point.
Having been involved in this debate for a while, I approached this paper with
a good measure of skepticism, and, it must be said, Robbeets’ discussion of
verbal ‘paradigms’ (departing from a set of morphemes in Japanese, which is

---

The author’s attempt to rename this by putting the syllable ‘Eur-’ into the name of a language
group/family, which only very secondarily expanded into Europe, moreover the distinctive
use of the traditional ‘Altaic’ for the ‘core’ groups of Turkic, Mongolian and Tungus, while
inserting ‘Eur-’ when adding Korean and Japanese to these, strikes me as illogical and should
not be followed.
said to exhibit charateristic properties, which would allow for this term) as putative new
evidence for this grouping did little to change this. I put quotes around the
term paradigms here, because Frellesvig (2010: 116–117), whom she cites on the
subject, also does, and not without reason.2

The material evidence this paper brings forward to convince readers, once
more, that these languages are genealogically related, may seem superficially
sound for the untrained eye, and to illustrate that and why it is not, it will be
necessary to have a closer look at each of these items (for an assessment of
an earlier—and different—list of morphemes by Robbeets, which served the
same purpose, cf. this reviewer (2011)).

The list is relatively short and can be summarized as: *a- (verbal root, ‘to be’), *-rA
(‘nominalizer’), *-mA (‘nominalizer’), *-i/-ø (‘nominalizer’), *-ø
(‘imperative’).

All of these are described as showing a variety of functions, sometimes co-
inciding between the different language families—this obviously serves the
purpose of making the point being made look even stronger, but obtaining this
picture involves a considerable degree of reification. To give just one example:
P. 210 presents the Manchu verbal noun suffix (as I would call it) -rA as showing
the functions of ‘clausal nominalizer’, ‘relativizer’ and ‘finite marker’, suggest-
ing a remarkable polyfunctionality, which is then repeated by other morpho-
logical elements in other languages to give the impression of a significant and
non-trivial functional parallelism in the languages compared.3

This can relatively straightforwardly be shown to be just another instance of
reification (or, maybe better: as instances of ‘multiplying entities by inflating

---

2 An attentive reading of Frellesvig reveals that the term ‘paradigm’ can only be used here for
some practical, and mostly traditional, reasons, and that its content is certainly not congru-
ent with the definitions of this term as given by other linguists, including some who contrib-
uted to this very volume, as e.g. Brian Joseph, s.a. I was quite surprised to find that the first
paragraphs of Robbeets’ section 3 repeat Frellesvig’s explanation of the ratio of the tradition-
al Japanese katsuôkei system (from his pp. 116–117) almost verbatim (with some omissions
and a few changes, as e.g. substituting his amo example for the principal parts of the Latin
verb by ferro), including Frellesvig’s hesitating hint at the possibility that this system may
indeed have been inspired or influenced by an early acquaintance of Japanese scholars with
Latin grammar—without proper acknowledgement. The wording on page 206 (on Korean
converbs) is also remarkably similar to that of Martin, 1992: 70 (which is mentioned, but Mar-
tin’s formulations are not marked as citations, although I feel that they should have been).

3 Pretty much the same kind of argumentation is found passim in Robbeets (2013) as well,
where the main point was to imply that all these languages underwent similar ‘grammatical-
ization’ processes (but without an areal element); cf. Georg 2013 for the demonstration that
no grammaticalization is involved in this at all.
It goes without saying that this is, of course, an idealized scheme, and nobody should be overly surprised to find exceptions, category changes and other snags in the wealth of languages and historical stages this table tries to summarize in a single paragraph; but experience (which every reader can try to repeat) shows that this goes a long way to guide observers through the systems of (terminal) verbal suffixes of rigid SOV languages (and helps to reduce baroque terminology inflation).

The typical picture the majority of languages with this typological makeup show is that their verbal forms may be relatively neatly divided into the following three, usually very distinct, groups: finite verb forms, verbal nouns (often referred to as ‘participles’ in the literature), and converses (a.k.a. ‘gerunds’ in many descriptions); if we plot these with the basic functions they can fulfill (finite usage, i.e. ending and constituting an independent sentence, attributive usage, i.e. modifying nouns, and adverbial usage, i.e. modifying verbs), we can draw the following table (+ = possible function, - = impossible function):

| Function       | Vfin | Verbal Noun ('Participle') | Converb ('Gerund') |
|----------------|------|---------------------------|-------------------|
| finite         | +    | +                         | -                 |
| attributive    | -    | +                         | -                 |
| adverbial      | -    | -                         | +                 |

4 It goes without saying that this is, of course, an idealized scheme, and nobody should be overly surprised to find exceptions, category changes and other snags in the wealth of languages and historical stages this table tries to summarize in a single paragraph; but experience (which every reader can try to repeat) shows that this goes a long way to guide observers through the systems of (terminal) verbal suffixes of rigid SOV languages (and helps to reduce baroque terminology inflation).

5 And generally nominal, ‘Altaic’ languages usually show no (or at best a rudimentarily developed) morphological class of adjectives—all qualifying nouns can be used as attributes for the modification of other nouns—this general characteristic of nouns extends, naturally, also to verbal nouns, which explains their behaviour neatly, without having to summon exceptional polyfunctionalities, let alone grammaticalization. ‘True’ (i.e. syntactically subordinating) relativization is alien to Altaic languages, and only found in areas of heavy areal interaction with non-‘Altaic’ languages (such as Persian, Slavic and the like).
Robbeets’ ‘clausal nominalization’ function is, then, nothing more than an epiphenomenon of the attributive (i.e. basically nominal) function of verbal nouns, including the ability to assume case affixes (just like any other noun) for further differentiation of their functional role in the sentence, as in Robbeets’ (Manchu) example 4b: ala-ra-de, lit. ‘at (DAT) tell- (ala-) -ing’, (-ra, verbal noun) → ‘when (s.o.) tells/told…’; this also explains the ‘relativizing’ function of Altaic (and other) verbal nouns, as in her example 4c: bargiyata-ra, lit.: ‘protect-ing person’ → ‘person, who protects’. Note that verbal nouns (the only category with two + -signs in the table), as a rule, can also occur in finite function, for which in some languages an additional copula may be used, while others can do without one or differentiate nuances by the use and non-use of such copulae. So, the whole rigmarole of functions Robbeets attributes to these verbal nouns pretty much boils down to their nouniness, and while this is certainly a typically ‘Altaic’ trait, its real domain are the (exceptionally numerous) postpositional SOV languages of the world with their strong determinans-ante-determinatum syntax.

Not much needs to be said about the zero-marked imperative (‘-ø’), since such formations are known from many language families (including Indo-European, Uralic, Semitic and others), which are not (normally) suspect of being related, so this non-marking cannot really be used for such a demonstration (Robbeets seems to be aware of this herself (222), but includes it nevertheless in her final list of allegedly significant commonalities, because it ‘fits a cell in the closed set of forms’).

The argument in favour of the presence of the ‘verbal root *a-, ‘to be’ ‘in Turkic is quite forced and implausible, since Robbeets manages to find it only in the form of a denominal verb-forming suffix +A-; her argumentation in favour of this identification misinterprets a statement by Erdal (1991: 454 [not: 434]) about the relative frequency of the verbalizer +A- and its functional equivalent +la- (she renders Erdal’s observation of +A- being frequent with deverbal nouns in -Xn rather generously as ‘always’, which is not true) and

---

6 A very superficial glossing, here chosen for illustrative purposes only.
7 Which it actually does not—this becomes clear when one reads Frellesvig and Robbeets’ paraphrase of this author’s statements on the matter. Moreover, her formulation that ‘Among the Tungusic languages only Manchu and Sibe have preserved the zero imperative’ (222) is turning the facts upside down, since Tungusic has, originally, non-zero marked imperatives, and the heavily mongolized languages Manchu and Sibe lost these, obviously under Mongolian influence: on Tungusic imperatives cf. also Alonso de la Fuente (2012).
8 Here and elsewhere, capitalized vowels in Turkic examples denote vowels which undergo harmony: -A = -a/-e, -U = -u/-ü, -I = -i/-I, -X = -i/-I/-u/-ü.
Her footnote 5 tries to combine the (positive) aorist -r with its negative counterpart -mAz, by the assumption of an ‘extra vowel’, which could, somehow, explain the -r ~ -z alternation here. But she quotes her witness (Erdal 2004, 84–85) for this idea only selectively, omitting the numerous counterexamples Erdal offers himself, and his statement ‘All this does not help us (...) unless we are ready to make some bold etymological assumptions.’ Erdal is right.

Benzing 1959, from whom Robbeets takes the first of these examples, offers two alternatives for the explanation of tuhrâm, of which the one involving the verbal noun *-dUk may be not appealing to every observer, but can also not be dismissed off-hand (cf., however, the Volga Bulghar data, which strengthen this assumption, Tekin, 1988: 41; Erdal, 1993: 76–80). Chuvash historical linguistics is difficult.

Robbeets generously germanizes the name of this scholar, here and elsewhere, as ‘Krüger’. He might not be amused.
surprised to see this derivation, since in a relatively recent paper (2013: 167),
Robbeets explicitly states: ‘I follow Johanson’s (1975) view that the aorist [the
Turkic form in *-r, St.G.] has actually been preserved in Chuvash, surviving as
a so-called finite ‘future’ in -Ă’. On my first reading of this, I was willing to con-
gratulate Robbeets on the fact that she was able to understand Johanson’s very
involved reasoning in that paper to begin with, but it may be necessary to re-
vise this impression. Now, it seems very difficult to reconcile the one view with
the other, so it may be allowed to ask, which of these approaches we should
chose from?

It will not be denied here that Mongolian shows a deverbal nominalizer *-r,
nor is it wrong to assume that this is probably historically present in the con-
verb marker *-rA (a dative of this nominal form) of this language, but, in order
to illustrate the way this author operates with data from these languages, I have
to mention that Robbeets’ example (5b) is wrongly translated (already in her
source, Sárközi, 2004: 47)—instead of ‘He went to find her mother’ it must be
(depending on the discourse context, which we do not have): ‘S/he went to find
his/her (own) mother’, the given interpretation being impossible because of the
(misglossed) reflexive-possessive accusative suffix. It is difficult to say (yet),
whether Khitan (a fragmentarily attested ‘Para-Mongolic’ language of the XIth
century, which only recently begins to be partially understood) preterite forms
in -Vr belong here, but I will grant this possibility—mentioning however that
Robbeets’ observation that this marker is only found with ‘telic’ verbs (which
fact, so she says, should be regarded as highly reminiscent of the situation in
Korean and Tungusic and thus as very important for the comparative study of
these) finds its neat explanation in the simple fact that all Khitan verbs known
so far are—**telic!**

---

12 Cf. the assessment of that publication in Georg 2013.
13 Johanson lets the *-r of the Turkic aorist simply disappear in Chuvash, without much in
the way of discussion, cf. ibid. 130. Apart from this, I wish to add that his work is certainly a
major contribution to the study of Chuvash morphology. Note, in passing, that it, expect-
edly, treats the *-r-preterites of Chuvash, as outlined above, as going back to *-d-forms, just
like everybody else does (112).
14 Closing the inspection of Robbeets’ illustrations of the Turkic aorist in *-r, one of her Old
Turkic sentence examples, from the Runic Old Turkic *Irk Bitig* (the ‘Book of Omens’, on
which cf. Rybatzki 2010) must be mentioned—she gives it twice, in 2013: 167, with the
final verb form as *yorï-rl*, and again in the present book (212), as *yorï-r*, to make the same
point. Both are wrong, correct is only: *yorï-yur*, cf. Tekin, 1993: 21; Thomsen, 1912: 205–206.
15 Ironically, the very verb which is cited in the Khitan example (5c) may be an exception,
since there is ample reason to view it simply as the verbum substantivum ‘to be’, which,
Both her sources for Tungusic *-ra-i (Menges, 1968; Benzing, 1956) derive
this formant from *-ra-gi (i.e. with an intervening velar consonant), which
Robbeets omits, without discussion, because what she needs here is a suffix without such a consonant (but cf. particularly Menges (1943: 243, Fn. 19), for
the demonstration that there must have been a velar here). Also for Tungusic,
it must be noted that Robbeets’ interpretation of Sibe (a still spoken variant of
Manchu in Xinjiang) -m (finite verb marker/imperfect, explained as ‘< -mbA’) is contradicted by other (including earlier) sources for this language, which
clearly show that its pedigree is nothing but the ubiquitous Manchu finite vb.

Then, alas, I need to comment on the putative use of Old Turkic -I as a
converb—this is not acceptable, either. The Old Turkic ‘vowel’ converb shows
the forms -A and -U after consonants (on their distribution cf. Erdal, 2004: 31,
Two of the (possibly) three scribes of the Hami-Manuscript are known: Waptsun Šäli and Tu Tsupa Äkä, neither of them bearing a Turkic name (Geng et al. 1988, 5).

With this demonstration, I do not intend to decree ex cathedra that the Turkic ‘-I-converb’ outside the causative context has to leave all grammars and studies here, now and for good—but I think it is important to show, how insecure the ground we are walking on -yU after vowels), whereas the shape -I is confined to causative formations with the suffix -Xt, where the high unrounded (front or back) vowel is explicable as due to (coronal) assimilation. Attestations of a converb -I in other contexts are spurious, and Robbeets’ explanation of alleged petrified -I’ converbs’ are, in reality, instances of the deverbal noun-forming suffix -I—and sometimes of something entirely different, as e.g. in bari ‘all (of it)’, which she wants to explain as bar- ‘to go’ + ‘conv.’ -I. Not really, I’m afraid: this is, quite clearly and universally accepted, an instance of the nomen existentiae băr + Possessive Suffix 3rd Sg., i.e. ‘its existence, ‘what there is-of it’, as evidenced by the long vowel in this noun (in all Turkic languages, which allow quantity differences to be discerned, cf. Turkmen baari ‘весь, целиком’). A form with the same shape, but a different interpretation, is given in her example 2ob as witness for the -I-converb in non-causative forms, but this is doubtful, if not spurious, as well. She does not give the source of this sentential example, but with some effort it is possible to identify it as being from the Hami-Manuscript of the Maitrisimit, the Old Turkic version of the Maitreyasamināṭaka, edited by Geng/Klimkeit (1988). The inspection of the manuscript itself (op cit., Vol. II, 2) reveals that there is, at best, a marginal possibility to read a final -I here (the difference between -I and—expected—u is only a tiny quantity of ink filling a given space or not, in a manuscript—not a blockprint—which is relatively clearly written, but not a calligraphic masterpiece either). It is true that -I has been read here before, and neither Robbeets nor the present writer will be able to decide this finally, but it should not be overlooked that the same manuscript, in the same expression, has, rather unambiguously, bar-u only three lines before and qutığar-u (same converb, different verb) six lines after this occurrence, and, what is perhaps even more important, that the (older, cf. Laut 1986, 163) Sängim-Manuscript of the same work, in the same passage, has, again (expected) bar-u here (Tekin, 1980 I, 166—60 r, l. 11). So, rather than proclaiming the trouvaille of an exceptional archaism (with its intended, far-reaching consequences), it seems to be more advisable to check and re-check the manuscripts themselves, and, if -I is to be read coute que coute, to admit for a scribal error (scribes err, and this scribe errs, too, cf. the mentioned edition of Geng et al., passim), possibly reinforced by the fact that the verb in question has /-I/ as its stem-final aorist vowel in the first place.18

---

17 Two of the (possibly) three scribes of the Hami-Manuscript are known: Waptsun Šäli and Tu Tsupa Akä, neither of them bearing a Turkic name (Geng et al. 1988, 5).

18 With this demonstration, I do not intend to decree ex cathedra that the Turkic ‘-I-converb’ outside the causative context has to leave all grammars and studies here, now and for good—but I think it is important to show, how insecure the ground we are walking on
Finally, Robbeets’ treatment of alleged Mongolian verbal nouns (and converbs) in -i is entirely based on the half-page discussion in Poppe (1955) (264, where he mostly presents—but mislabels as verbal nouns—finite forms, which becomes clear when textual attestations are scrutinized); while Poppe merely offers some rather ad hoc guesses about these forms, any further discussion of them must, however, be based on the detailed study of Doerfer from the same year (1955, 257–259), who presents a thorough examination of the forms in question in Middle Mongolian, showing that they convey distinctions of number and also gender (a rather ‘un-Altaic’, but real, trait of Middle Mongolian).

The enumeration of questionable data and interpretations from Robbeets’ paper must end here, though it could certainly go on for a while. Since its author is very positively convinced of having solved the riddle of ‘Altaic’ once and for all, it had to be demonstrated (tedious as all these discussions of philological details and caveats may be), why the present reviewer cannot avoid pointing out that the argumentation at hand uses too many unconfirmed, marginal, un-/misunderstood, out-of-context and distorted pieces of data to convince its audience (which will mostly consist of general and typologically oriented linguists and students of language contact) that everything is fine with ‘Altaic’, that it is a family and nothing else, and that the critics of this concept (who work under the assumption that the undeniable commonalities between these languages are due to long-standing areal convergence processes) obviously need not be listened to. The question of ‘Altaic’ (and of ‘phylum recognition’ in general), so I am convinced, is too difficult and too important to allow for such sweeping statements to go uncommented.

As in Georg 2011, I can only repeat here that not all examples given by Robbeets (but an alarmingly high number of them) are necessarily wrong and misleading, and that not all suffixes she mentions for the languages in question are non-existent (but scrutinizing the evidence presented removes quite a number of them, casts doubt on some more, and leaves the argumentation full of lacunae). And I am also happy to repeat that, yes, the Altaic languages do have morphological elements in common, which, by form and function, do invite
their comparison beyond the boundaries of, respectively, Turkic, Mongolic, and Tungusic (with, however, different possibilities for different solutions).  

In the abovementioned paper, I have shown that a longer roster of morphological comparanda defended by Robbeets in other publications can be shown to be easily expandable by the inclusion of (formally and functionally) similar morphological elements from material found in the Volga-Finnic languages Cheremis and Mordvin (where a conspiracy of late borrowing and, the taboo word must be mentioned, chance resemblances is responsible for the surface picture we get, as long as we do not ask too many questions). Robbeets is aware that such lists can, and have been, produced as a warning against rash conclusions on the basis of surface similarities, even if paradigms are involved, and she cites a famous example herself, which shows that even languages as distant as Proto-Indo-European and Proto-Eastern Miwok (California), where no suspicion of relationship can be involved this side of ‘Proto-World’ and areal contact is equally out of the question (Callaghan, 1980: 337), may show rather stunning similarities of this kind. It is fair, and remarkable, that Robbeets draws attention to the real danger of such a fallacy herself, but she thinks that it is enough to thwart it with the dry remark that the fortuitous coincidences ‘involve only five cells of the paradigm, not extending to the third person plural’ (203). Oh, then...
Now, I think this offers the opportunity for a similar demonstration—this time, I do not think that any borrowing is involved, and that only the explanation of chance resemblance can be invoked. The language family for this test is Dravidian, where we find the full set of Robbeets’ ‘Altaic’ affixes. It is true, Dravidian languages have sometimes been viewed as genealogically coordinate with ‘Altaic’, most notably by K. H. Menges (1964, 1975), Vacek (e.g. 1987, from a long row of papers and books on this idea) and some others—so far without any success or positive echo to speak of. The Dravidian ‘comparanda’ which could be added to Robbeets’ list, are:22

| ‘Altaic’ acc. to Robbeets (223) | Dravindian |
|--------------------------------|------------|
| *a- | ‘to be’ | *â- | ‘to be’, 184 |
| *-rA | nominaliser | *-al | nominaliser, 199 |
| *-am | nominaliser | *-am | nominaliser, 199 |
| *-i | nominaliser | *-i | perf. part. (vn), 331, 332 (Kannada and Telugu) |
| *-Ø | imperative | *-Ø | imperative, 357 |

What does this tell us about Dravidian and ‘Altaic’? Nothing. What does it tell us about ‘Altaic’ itself? Only so much that the evidence presented (even if it were impeccable in itself, which it is not) is indistinguishable from—noise.

I concur with M. Robbeets that the investigation of morphological commonalities is an important area of the debate, and that it should be moved more into the focus than it has been done before, when the discussion revolved mostly around lexical comparisons and sound-correspondences. But: all these areas are important and inform each other to draw the complete picture of this language family (if it is one), or, then, this large-scale convergence area—and all of these aspects and subfields have to be pursued with the same
evidence, overwhelmingly questioned (and often vehemently rejected) by reviewers and other specialists for the languages involved, so they cannot be used for this purpose just as if nothing were wrong with and nothing critical had ever been said in print about them.

22 All from Krishnamurti (2003) with page numbers. This list is not intended to be a contribution to Dravidian studies, and I allowed some licenses (taking /l/ as a match for /r/, cv ~ vc), just like Robbeets’ list does.
degree of scrutiny, scrupulousness, exactitude and genuine respect for the data and readers who may not be willing or able to control the data themselves. The impression one gets from the argumentation found in this paper is that of a still very immature field—where it suffices to scan grammars to find some suffixes, say, -i, with (somehow) reasonable similarities of form and function, anywhere in the family, in languages old and new, in the centre or on the margin of grammatical systems, well or not so well attested or understood, and to label them, when needed for the ‘bigger picture’, as ‘Proto-’ (Turkic, Mongolic etc., and finally ‘Altaic’), while signals of caution, difficulties of interpretation, and sometimes also known and uncontroversial facts are simply ignored. The value of the ‘results’ such an approach leads to is—nil. To illustrate how real these dangers are, it might be useful to give yet another illustration from a non-‘Altaic’ language family, this time from a really well-explored realm, Indo-European. Pali (Middle Indian), Greek, Albanian, Old Church Slavonic (OChSl), Classical Armenian, Old High German (OHG), and Late Phrygian are, certainly, all established members of the Indo-European language family. Now, in all these languages we can find, somewhere in the roster of their nominal paradigms, at least one nominal genitive singular form with the shape /-u/. It seems, prima facie, to be quite plausible to assume that this remarkable set of coinciding genitives, in so many constituent languages, should be reconstructed, as such, for the ancestral language—if one approaches the matter in the way outlined above, i.e. without much in the way of an in-depth understanding of the languages involved. The following table tries to illustrate these suspicious -u-genitives and their usual historical explanations.23

---

23 I refrain from giving references for these forms and reconstructs, they come from the usual handbooks; of course, some Indo-Europeanists might and will favour, and some handbooks give, different explanations, often with good reasons; it is, however, absolutely clear that a ‘Proto-Indoeuropean genitive morpheme **-u’ will not be among them. Similar examples of superficially resembling, but by no means cognate, affixes across this family (and other established stocks) could easily fill a booklet. If we allowed plural and pronominal genitives into the picture, the ‘-u-genitive’ list could easily be expanded as well. Robbeets says, 203: ‘(…) the probability that a certain correspondence in verb morphology is due to coincidence will be lower than that for a similar correspondence within the lexicon, because the body of elements open to comparison is much smaller’. This is a puzzling statement, because the opposite is true, cf. also Janhunen, this volume, 314f. I am, incidentally, aware that my list uses nominal morphology, whereas Robbeets’ list is about verbal suffixes—this does, however, by no means detract from the general lesson to be learned from this, which is that the reliance on surface similarity for the meaningful comparison of languages is and will remain an elementary (and more often than not fatal) error. Note further that Indo-European as a family is firmly established, whereas ‘Altaic’ is not.
With analogical generalization of -u after velar consonants, imitating the pattern of the postponed definite article. The form is a variant of more frequent geba (= Nom./Gen.) and originally represents the dative. Thus not identical with the Greek form, further details of this fragmentarily attested language are difficult. I cannot resist mentioning Pereltsvaig/Lewis 2015 here, who show that, in some circles, attempts are being made these days to switch out the light which has illuminated Indo-European linguistics for generations (by switching on some computers) and to reduce this discipline to the pre-modern guesswork stage in which large areas of the Altaic debate still are in—the belief that all that processing power can replace the available knowledge about these languages (and spare the toil to familiarize oneself with them) and will produce ‘results’ which are worth the paper they are printed on, is impossible to comprehend. But it helps to get into the newspapers, of course.

| Language | Nom. | Genit. | Where? | < * |
|----------|------|--------|--------|------|
| Pali     | dātā | dātu   | -r-stems (dātar-) | *-uh | *-r̥s |
| Greek    | λύκος | λύκου  | -o-stems | *-o-so |
| Albanian | plak | ipaku  | nouns in velars | *-ei |
| OChSl    | syna | synou  | -u-stems | *-ous |
| Armenian | žam | žamow  | -u-stems | no ending, -u is the stem-forming suffix |
| OHG      | geba | gebu  | -ā-stems | *-ā (Instr. > Dat. > Gen.) |
| Phrygian | -ος | -ού (= /u/) | thematic stems | < Old Phr. -o-wo | ?26 |

Not only are all these -u-genitives not cognate, they also cannot be, if anything we have learned about Indo-European in two centuries is valid. Everything they are is superficially resembling, and this is precisely what historical-comparative linguistics is not concerned with, and, if done properly, cannot be fooled by, in other words: being able to differentiate between superficial ‘phaïnomena’ and real comparanda is what makes historical-comparative linguistics a science. Altaic linguistics still seems to have a long way to go, until it can be described as having reached a comparable level. As it stands, it is hard to avoid the conclusion that it is still mostly poking in the dark.27
4 Paradigm Borrowing, Turkic, Mongolic and Tungusic languages of Inner and Northern Asia, and the Proper Meaning of ‘Ural-Altaic’

It is often heard in less-than-well-informed circles that morphological paradigms are (at least mostly) impervious to borrowing of any kind, but this is, and has always been, a fallacy, as the contributions of L. Johanson and B. Pakendorf to this volume once again demonstrate. L. Johanson: A Yakut copy of a Tungusic viewpoint aspect paradigm, 235–242, offers an example for this and lets us have a closer look at the Yakut (North-Eastern Turkic) synthetic imperfect paradigm (bar-ar-ım etc.), which is (for a Turkic language) untypical in that it lacks an overt past marker in a form with clear past reference. He explains this—cautiously, and without claiming to have found the final solution—as being due to the close interaction of Yakut with neighbouring Northern Tungusic Ewen (Lamut), which offers a structurally similar paradigm as a potential model—the type of borrowing which is involved here is, again, of the ‘carry-over’ (or ‘substrate’) type (cf. the comments on Csató’s paper above; Csató’s ‘perceived identity’ of the central morphs, which involve –r– in both languages, may also have played a role here), rather than a straightforward borrowing which involves the ‘take-over’ of morphemes. Stachowski 2006 (duly mentioned by Johanson) offers a very similar solution to this problem,28 which differs at best in some nuances of the functional interpretation of the processes involved.

Hans Nugteren, the unrivalled master of Gansu-Mongolic linguistics, takes a closer look at interesting areal phenomena in this fascinating area in: Amdo29 Altaic directives and comparatives based on the verb ‘to see’, 243–256. The object of his study is the grammaticalization of verbs around the semantics of ‘to see, to look’ to markers with allative/directive semantics and also, very interestingly, to markers of the basis of comparisons (roughly: ‘looking at A, B is big --> B is bigger than A’). The main languages studied are (Huzhu) Mongghul, (Minhe) Mangghuer, Eastern Yugur, (Nantoq and Dahejia) Baoan and little studied

28 Robbeets, this volume 213 (see above), thinks that it is all Proto-Turkic, but this is based on her epic misunderstanding of the Chuvash preterite.
29 The author uses this historical name for Eastern Tibet for the region which straddles the present-day Chinese provinces of Gansu and Qinghai, where the southermmost Mongolic and some Turkic languages show very aberrant structures in comparison to their respective ‘family mainstream’, which is, to a large degree, due to intensive interaction with local variants of Tibetan (but the welter of areal convergence phenomena in this linguistically chequered region is by no means fully understood, nor can it only be described by the dry and simplifying label ‘Tibetanization’), cf. also Georg (2001).
Kangjia from the Mongolian and Salar from the Turkic language family, together with local (Amdo) dialects of Tibetan and the aberrant (creole?) variant of Chinese, which is known under the name Wutan. Especially the TO SEE --> COMPARATIVE grammaticalization, though present in some Turkic languages outside of this area, is very peculiar to the region, and it is, as Nugteren rightly observes, not possible to determine its locus of origin in the languages involved (Baoan, Kangjia, Amdo Tibetan, Wutan), since it seems to be absent from older Mongolian and other variants of (including literary) Tibetan.

I. Nevskaya: Innovation and archaisms in Siberian Turkic spatial case paradigms: A Transeurasian historical and areal perspective, 257–285, discusses some historical developments and characteristics of the case systems in Shor, Tuvan, Tofa, Gorno-Altay (Oirot), Khakas and Yakut (i.e. in practically all Turkic languages of Siberia), with a good measure of comparative data from Old Turkic as well, to elucidate their status as (independent or contact-induced) innovations or, then, as archaisms. The case studies discussed in this very enlightening paper are: the ‘stative’ dative in some languages, i.e. a dative form which is used with locative semantics in Tuvan (for past and future events, whereas present events require the ordinary locative, a quite unusual patterning in the realm of nominal case marking), Shor (for ‘non-specific’ location) and Gorno-Altay (for ‘temporary’ location), old and new directives in Old Turkic, Gorno-Altay and Tuvan, new and old prolatives (‘along...’), where Khakas and Shor preserved the old prolate semantics of the ‘equative’ in -ČA, and the development of locative/ablative suffixes to partitive function, which is best known to Turkologists from Yakut, and also found in Tofa. Personally, I find the partitive interpretation she ascribes to Old Turkic ablatives a bit forced (279): ‘(...) what is actually expressed (...) is partitive meaning in the sense of splitting something and taking one of its parts away’. One of the examples given for this is: tabgač xagan-ta (= Chinese emperor-ABL) bădizči (= decorator) kălür-t-üm (= I brought) ‘I brought decorators from the Chinese emperor’. Yes, but it is not the emperor (marked with the ablative), who is split up and taken away in pieces. The mentioned Gorno-Altay partitives for quantities of food (explicitly given as the semantic domain of this usage of the ablative suffix) have straightforward parallels in European languages, cf. j’ai bu du vin and the like (in German, the use of a partitive Genitive or a von + Dative construction has a bit of an archaic or over-formal ring, but can still be heard or read30), so these may not necessarily be of assistance for an explanation of the celebrated

---

30 Cf. the classic locus from W. Busch, Max und Moritz, Zweiter Streich: “dass sie von dem Sauerkohle eine Portion sich hole...”.

JOURNAL OF LANGUAGE CONTACT 10 (2017) 353-381
Yakut partitive cannot be separated from the partitives of Northern Tungusic, and the origins of this usage should be sought more in an areal context with these than in the history of Turkic itself. This dialect is described as being markedly distinct from other variants of Ewen/Lamut, cf. Kuz’mina 2010; Novikova (1958: 188) reports that, in 1953, speakers of Lamunkhin requested that a special written norm be created and introduced for it, which, however, did not necessarily be warranted, since less complicated situations still remain.

31 Generally, the Yakut partitive cannot be separated from the partitives of Northern Tungusic, and the origins of this usage should be sought more in an areal context with these than in the history of Turkic itself.

32 This dialect is described as being markedly distinct from other variants of Ewen/Lamut, cf. Kuz’mina 2010; Novikova (1958: 188) reports that, in 1953, speakers of Lamunkhin requested that a special written norm be created and introduced for it, which, however, did

Her conclusion from all this is interesting, yet also a bit surprising, at least worthy of some more thinking (307): because of the intricate set of conditions and factors which obviously were at work in the contact situation at hand (and the combination of which will, then, be ‘cross-linguistically very rare indeed’), she concludes that the answer to the ‘question (whether we) need to worry that undetected instances of paradigm copying have occurred in linguistic prehistory (...) is an unequivocal ‘no’. Given the fascinating details she presents in a very readable and magisterial way, one is tempted to agree for this case, but it should also be stressed that the generalisation which is implied here may not necessarily be warranted, since less complicated situations still remain.
thinkable—whether the possibility of their as yet undetected existence should ‘worry’ linguists, is a different question, and possibly suspect cases would need to be investigated individually—and preferably with Pakendorf’s exemplary scrutiny. This paper should be compulsory reading for all linguists who have to deal with morphological borrowing in their fields of interest—whether they reckon with morphological borrowing all over the place, or whether they tend to be very reluctant to admit of this in the first place—or operate, preferably, between these extremes.

The closing paper of this inspiring volume, J. Janhunen: *Ural-Altaic: The Polygenetic Origins of Nominal Morphology in the Transeurasian Zone*, 311–335, may have, for some, a somewhat surprising title, since the general rejection of the ‘Ural-Altaic’ hypothesis (which claimed a common origin of the Uralic and the ‘Altaic’ languages) is of course, *pace* its partial resurrection in the guise of, the once popular but nowadays mostly rejected, ‘Nostratic’ endeavour, done and dusted. But what Janhunen does is of course *not* an attempt to revive this hypothesis (and the subtitle of his paper should leave no doubt on that). On the contrary, being a well-known critic of Altaic, he tries to give it back its true—and, so I might be allowed to add, only meaningful—content, namely that of the time-honoured designation for the major linguistic *convergence area* of the Eurasian continent (he actually prefers to speak of the ‘Ural-Altaic Phenomenon’). The paper discusses a number of superficially resembling morphological elements from the realm of nominal morphology from these languages and families, and concludes that genetic explanations of these, though repeatedly proposed or declared valid in the literature, are misleading. The paper offers very refreshing and thought-provoking reading, not least due to its exceptionally precise formulations and distinctions of the relevant concepts it works with. Janhunen’s statement that (351) “speaking of ‘Altaic’ instead of ‘Ural-Altaic’ is a misconception, for there are no areal or typological features that are specific to ‘Altaic’ without Uralic”, and that therefore “(a)ny future study of the ‘Transeurasian’ languages should (...) return to the roots of Ural-Altaic studies and consider the contribution that Uralic can make to the understanding of the historically important and linguistically challenging Ural-Altaic Phenomenon” can be subscribed to without reservation.33

---

33 I might, though, add a minor point of disagreement with the author’s judgement that ‘Uralic was cut away from this (Ural-Altaic, St.G.) context due to the false illusion of a specific
5  Concluding Remarks

Unfortunately, the contribution of one of the editors of this fine volume had to be described as a fly in the ointment—not because the present reviewer thinks that the point it tries to make is wrong (though he does), but rather since it seems to mark another low in the quality of the arguments put forward in favour of the genealogical reality of ‘Altaic’, with a quite careless attitude towards known and well-described facts with which this is presented to an audience, which will, so I dare say, not necessarily consist of specialists in Old Turkic philology and similar disciplines who are willing (and able) to scrutinize the presented data lege artis. The Altaic debate—open as it still is, which I see no reason to deny—was once led by scholars who, on both sides of the trenches, were thoroughly familiar with the languages being compared and, above all, with what can be and is known about their history—and went from there. M. Robbeets’ contribution shows little signs of such a depth of knowledge, or of respect for the data in the first place, and, thus, commits errors, which were inconceivable (and sometimes outright impossible, if I may only repeat my bewilderment over her re-interpretation of the Chuvash preterite), say, half a century ago. She and others may think that this is enough for today, and for the audience she addresses in the Studies in Language Companion Series, but the present reviewer reserves the right to voice his dissent, and, moreover, to think that the discipline and her audience deserve better than ever repeated declara-

‘Core Altaic’ or ‘Micro-Altaic’ family (...)’. I read the history of the separation of these two branches of the earlier, Castrén-ian, Ural-Altaic a bit differently (cf. Georg, 2011)—rather, it seems to be possible to describe the history of these sister disciplines along the lines that the study of Uralic (Finno-Ugric and Samoyedic) languages gradually developed an increasingly firmer ground of sound comparisons and solid results, which, mostly during the course of the second half of the xixth and well into the xxth century, elevated this field to a level of sophistication, which put it quickly on a par with that of comparative Indo-European linguistics; the study of the ‘Eastern Ural-Altaic’ (or, then, ‘Altaic’) languages failed to yield such palpable successes, and Uralicists, who preferred to work on that part of this ‘phenomenon’, which produced so many valid genealogical explanations of the observable facts, while the ‘rest’ simply did not, gradually excluded Turkic, Mongolic, Tungusic, let alone Korean and Japanese, from their considerations for elucidating the early and the pre-history of Finnish or Nenets. Viewing this ‘rest’ as another (genealogically coordinate or, increasingly and since Ramstedt definitely, separate) ‘family’ in its own right, may, then, be seen as a hyper-conservative continuation of the erstwhile concept for those languages which were left after the study of Uralic matured, and not necessarily as the cause for the latter development.
the slightest scrutiny as, yet again, another house of cards. If ‘Altaic’ should be demonstrable in the end, everything the critics of this concept will have lost is not more than, as the saying goes, an old error. But this has not been attained yet, and as long as the scholarly level of the discussion is not elevated considerably, the working hypothesis that it is not so, that the ethno-linguistic history of Inner Asia has to be described along an entirely different path (i.e. one of areal consolidation, convergence, not divergence, of its major language families) needs to be heard, and the debate will have to stay with us for some more time.

References

Alonso de la Fuente, José Andrés. 2012. The first imperative of Tungusic. *Studia Linguistica Universitatis Iagellonicae Cracoviensis* 129: 7–34.

Benzing, Johannes. 1956. *Die tungusischen Sprachen. Versuch einer vergleichenden Grammatik*: Wiesbaden: Franz Steiner (Akademie der Wissenschaften und der Literatur. Abhandlungen der Geistes- und Sozialwissenschaftlichen Klasse, Jahrgang 1955. Nr. 11).

Benzing, Johannes. 1959. Das Tschuwaschische. In J. Deny *et al.* (eds.): *Philologiae Turcicae Fundamenta. Tomus Primus*, 695–751. Aquis Mattiacis (Wiesbaden): Franz Steiner.

Callaghan, Catherine A. 1980. An “Indo-European” type paradigm in Proto Eastern Miwok. In K. Klar *et al.* (eds.): *American Indian and Indo-European studies: papers in honor of Madison S. Beeler*, 331–338. The Hague: Mouton.

Campbell, Lyle. 1997. *American Indian Languages. The Historical Linguistics of Native America*. New York/Oxford: Oxford University Press.

Campbell, Lyle and William J. Poser. 2008. *Language Classification. History and Method*. Cambridge: Cambridge University Press.

Comrie, Bernard. 2010. The role of verbal morphology in establishing genealogical relations among languages. In Lars Johanson and Martine Robbeets, M. (ed.): *Transeurasian verbal morphology in a comparative perspective: genealogy, contact, chance*, 21–31. Wiesbaden: Harrassowitz.

Doerfer, Gerhard. 1955. Beiträge zur Syntax der Sprache der Geheimen Geschichte der Mongolen, *CAJ* 1: 219–267.

Donegan, Patricia and David Stampe. 2004. Rhythm and the synthetic drift of Munda, *Yearbook of South Asian Languages and Linguistics* 2004: 3–36.

Eliasson, Stig. 2010. Chance resemblances or true correspondences? On identifying the language of an ‘unintelligible’ Scandinavian runic inscription. In Lars Johanson and Robbeets Martine (eds.): *Transeurasian verbal morphology in a comparative perspective: genealogy, contact, chance*, 43–79. Wiesbaden: Harrassowitz.
Erdal, Marcel. 1991. *Old Turkic Word Formation. A Functional Approach to the Lexicon* (2 Vol.). Wiesbaden: Harrassowitz.

Erdal, Marcel. 1993. *Die Sprache der wolgabulgarischen Inschriften*: Wiesbaden: Harrassowitz.

Erdal, Marcel. 2004. *A Grammar of Old Turkic*, Leiden/Boston: Brill.

Frellesvig, Bjärke. 2010. *A History of the Japanese Language*, Cambridge: Cambridge University Press.

Geng, Shimin and Hans-Joachim Klimkeit. 1988. *Das Zusammentreffen mit Maitreya. Teil 1: Text, Übersetzung und Kommentar, Teil 11: Faksimiles und Indizes*: Wiesbaden: Harrassowitz.

Georg, Stefan. 1999/2000. Haupt und Glieder der Altaischen Hypothese: die Körperteilbezeichnungen im Türkischen, Mongolischen und Tungusischen, *UA/Jb N.F.* 16: 143–182.

Georg, Stefan. 2001. Tibetisch-mongolische Sprachkontakte im Gansu-Korridor. In Stefan Wild and Hartmut Schild (eds.): *Akten des 27. Deutschen Orientalistentages (Bonn—28. September bis 2. Oktober 1998). Norm und Abweichung*, 763–776. Würzburg: Ergon.

Georg, Stefan. 2011. *The Poverty of Altaicism*. Downloadable at https://www.academia.edu/1638942/The_Poverty_of_Altaicism.

Georg, Stefan. 2013. Rev. of: Martine Robbeets and Hubert Cuyckens (eds.): *Shared Grammaticalization: With Special Focus on the Transeurasian Languages*, Amsterdam: John Benjamins, *Cahiers de Linguistique Asie Orientale* 42: 178–200.

Givón, Talmy. 1971. Historical syntax and synchronic morphology: An archaeologist’s field trip, *Chicago Linguistic Society* 7: 394–415.

Greenberg, Joseph H. 1961. Some universals of grammar with particular reference to the order of meaningful elements. In Joseph. H. Greenberg (ed.): *Universals of Language*, 73–113. Cambridge/Mass: M.I.T. Press.

Güldemann, Tom. 2010. Proto-Bantu and Proto-Niger-Congo: Macro-areal typology and linguistic reconstruction. In Osamu Hieda et al. (eds.): *Geographical Typology and Linguistic Areas. With Special Reference to Africa*, 109–140. Amsterdam: Benjamins.

Hyman, Larry Michael. 2004. How to become a Kwa verb, *Journal of West African Languages* 30: 9–88.

Jacques, Guillaume. 2006. La morphologie du sino-tibétain. Downloadable at https://www.academia.edu/2261533/La_morphologie_du_sino-tibétain.

Johanson, Lars. 1975. Das tschuwaschische Aoristthema. *Orientalia Suecana* 23/24: 106–158.

Kane, Daniel. 2009. *The Kitan Language and Script*. Leiden/Boston: Brill.

Krishnamurti, Bhadriraju. 2003. *The Dravidian Languages*, Cambridge: Cambridge University Press.

Krueger, John R. 1961. *Chuvash Manual. Introduction, Grammar, Reader, and Vocabulary*, Bloomington/The Hague: Mouton.
Kuz’mina, R.P. 2010. Jazyk lamunchinskikh ėvenov: Novosibirsk: Nauka.
LaPolla, Randy. 2003. Overview of Sino-Tibetan Morphosyntax. In Thurgood Graham and LaPolla Randy (eds.): The Sino-Tibetan Languages, 22–42. London/New York: Routledge.
Laut, Jens Peter. 1986. Der frühe türkische Buddhismus und seine literarischen Denkmäler, Wiesbaden: Harrassowitz.
Martin, Samuel Elmo. 1992. A Reference Grammar of Korean. A Complete Guide to the Grammar and History of the Korean Language. Rutland/Tokyo: Tuttle.
Menges, Karl-Heinrich. 1943. The function and origin of the Tungus tense in -ra and some related questions of Tungus grammar. Word 19: 237–251.
Menges, Karl-Heinrich. 1964. Altajisch und Dravidisch. Orbis 13: 66–103.
Menges, Karl-Heinrich. 1968. Die tunguischen Sprachen, in: W. Fuchs et al. (eds.): Tungusologie (Handbuch der Orientalistik 1. Der Nahe und der Mittlere Osten 5. Altaiistik 3. Tungusologie), 21–256. Leiden: Brill.
Menges, Karl-Heinrich. 1975. Dravidian and Altaic. cAJ 19: 202–205.
Mostaert, Antoine. 1977. Le matériel mongol du Houa I I Yu 華夷譯語 de Houng-Ou (1589), Vol. 1. Bruxelles: Institut Belge des Hautes Études Chinoises.
Norman, Jerry. 1974. A sketch of Sibe morphology. cAJ 18: 159–174.
Novikova, K.A. 1958. Osnovnye osobennosti ėvenskich govorov Jakutskoj ASSR. DSIJaSSSR 11: 185–205.
Pakendorf, Brigitte. 2007. Contact in the prehistory of the Sakha (Yakuts). Linguistic and genetic perspectives. Utrecht: LOT.
Pakendorf, Brigitte. 2009. Intensive contact and the copying of paradigms: an Even dialect in contact with Sakha (Yakut). Journal of Language Contact 2: 85–110.
Pereltsvaig, Asya and Martin W. Lewis. 2015. The Indo-European Controversy. Facts and Fallacies in Historical Linguistics. Cambridge: Cambridge University Press.
Poppe, Nicholas. 1955. Introduction to Mongolian Comparative Studies, Helsinki: Suomalais-ugrilainen seura.
Robbeets, Martine. 2005. Is Japanese related to Korean, Tungusic, Mongolic and Turkic?. Wiesbaden: Harrassowitz.
Robbeets, Martine. 2013. Genealogically motivated grammaticalization. In Martine Robbeets and Hubert Cuyckens (eds.): Shared Grammaticalization. With special focus on the Transeurasian languages, 147–175. Amsterdam/Philadelphia: Benjamins.
Robbeets, Martine. 2015. Diachrony of Verb Morphology: Japanese and the Transeurasian Languages. Berlin: Mouton.
Róna-Tas, András and Árpád Berta. 2011. West Old Turkic. Turkic Loanwords in Hungarian, 2 Vol. Wiesbaden: Harrassowitz.
Rybatzki, Volker. 2010. The Old Turkic ìrq bïyïg and Divination in Central Asia. In Matthias Kappler et al. (eds.): Trans-Turkic Studies. Festschrift in Honour of Marcel Erdal, 79–102. Istanbul: Pandora.
Sárközi, Alice. 2004. *Classical Mongolian*, München: Lincom Europa.
Stachowski, Marek. 2006. Der Ursprung des synthetischen Imperfekts im Jakutischen, *Sec* 11, 135–138.
Starostin, S.A. *et al.* 2003. *Etymological Dictionary of the Altaic Languages*, 3 Vols. Leiden/Boston: Brill.
Tekin, Şinasi. 1980. *Maitrisimit nom bitig. Die uigurische Übersetzung eines Werkes der buddhistischen Vaibhāṣika-Schule. 1. Teil: Transliteration, Übersetzung, Anmerkungen, Teil 2. Analytischer und rückläufiger Index*, Ostberlin: Akademie.
Tekin, Talât. 1988, *Volga Bulgar kitabeleri ve Volga Bulgarca*, Ankara: TDK.
Tekin, Talât. 1993. *Irk Bitig. The Book of Omens*, Wiesbaden: Harrassowitz.
Thomsen, Vilhelm. 1912. Dr. M.A. Stein’s manuscripts in Turkish “Runic” script from Miran and Tun-Huang, *J RAS* 1912: 181–227.
Vacek, Jaroslav. 1987. The Dravido-Altaic relationship: some views and future prospects, *ArOr* 55: 134–149.
Zide, Norman and Gregory Anderson. 2001. The Proto-Munda verb and some connections with Mon-Khmer. In P. Bhaskara Rao and K.V. Subbara (eds.): *Yearbook of South Asian Languages and Linguistics*, 510–540. Delhi: Sage Press.
Zikmundová, Veronika. 2013. *Spoken Sibe. Morphology of the Inflected Parts of Speech*, Prague: Karolinum.